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SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

PURE MATHEMATICS. By F. PURYER WHITE, M.A., St. John's College, Cambridge.

Analysis.—The various methods of summation of divergent series can be brought under the following general scheme. By means of a definite matrix we effect a linear transformation on the partial sums s_n. If the matrix is such that (1) from any convergent sequence s_n we get to a sequence s'_k, which also converges and to the same limit; and (2) at least one non-convergent sequence is changed into a convergent sequence, then on it can be built a summation method. (1) is Hardy's condition of consistency. The aggregate of series for which s'_k is convergent is called the field of convergence. A summation method is only useful, however, if we can add and subtract summable series term by term, multiply by a constant, or make a finite number of changes in the series, take away a finite number of terms, or add or change a finite number with the same effect as for convergent series. Call this condition (*a*). It will increase the usefulness if (*b*) we can multiply series. The Cesàro method leads to easy analysis, satisfies (*a*) and essentially (*b*), but has only a relatively restricted field, *e.g.* it will only sum power series on the edge of the circle of convergence. The Borel method has a large field—it will sum Σz^n in $R(z)<1$, and any power series within a polygon which includes the circle of convergence—but the analysis is difficult and it does not satisfy condition (*a*) entirely, *viz.*, if $a_0 + a_1 + \ldots$ has sum s, it does not follow that $a_1 + a_2 + \ldots$ has sum $s - a_0$. The foregoing account is taken from the introduction to a paper by K. Knopp (*Math. Zs.*, **15**, 1922, 326–53), who investigates Euler's method and includes it in the general theory. If the original series be written $b_0 - b_1 + b_2 - \ldots$ then Euler's transformed series is $\Sigma a'_k$, where

$$a'_k = \frac{1}{2^{k+1}}\Delta^k b_0, \quad \Delta^k b_n = \Delta^{k-1} b_n - \Delta^{k-1} b_{n+1}.$$

Ames in 1901 showed that the condition of consistency (1) is

satisfied. Further, the transformed series of the power series Σz^n converges for $|z+1|<2$, and the series $1^p - 2^p + 3^p - 4^p + \ldots$, where p is a positive integer, transforms into a finite series of $p+1$ terms whose sum is $\frac{2^{p+1}-1}{p+1} B_{p+1}$. Knopp determines the matrix of Euler's transformation, and shows that condition (a) is satisfied. By repetition of Euler's process he obtains a summation method (E_p), which is essentially the same as Borel's method, a power series being summable (E_p) within the Borel polygon. Also for any inner point of the polygon the pth transform is *absolutely* convergent if p is suitably taken. He then defines summation (E_p) for any order p, real or complex, and determines the matrix of the transformation. Finally, he gives examples of series which are summable (C_1), *i.e.* by Cesàro's method, and not summable (E_p).

A. Loewy (*Math. Zs.*, **15**, 1922, 261–73) proves by elementary methods two reciprocity theorems concerning the simultaneous reduction of each of two irreducible equations by the addition of roots of the other; theorems which were deduced in 1906 by Landsberg by means of the Galois group. They lead to new results on irreducible equations with real coefficients with roots expressible by real radicals. Hölder has shown that an algebraic equation with only real roots irreducible in a real field of rationality can be solved entirely by real radicals only if its degree is a power of 2. This result is extended to irreducible equations with real coefficients of degree $2^\mu P$, where P is odd. The existence of any single root expressible by real radicals implies the existence of at least $P-1$ imaginary roots of the equation, and thus an irreducible equation with real coefficients of any odd degree P can never have more than *one* root expressible by real radicals, and this is then the only real root. If the degree is 2 P, the number of roots expressible by real radicals is at most 2^μ; if it is precisely this, then there are no other real roots, but if it is $\nu<2^\mu$, there are necessarily $\nu(P-1)$ imaginary roots, and there may be other real roots not so expressible.

In 1913 L. W. Jensen stated without proof two theorems concerning the zeros of the derivatives of algebraic polynomials with real coefficients, or of real integral functions of class zero or one. If $f(z)$ is the function in question, then the theorems are—

(1) Every root of $f'(z)=0$ lies within one of the circles described on the line joining a pair of conjugate imaginary roots of $f(z)=0$ as diameter; the same is also true for the roots of $\delta f(z)+f'(z)=0$, where δ is any real number.

(2) If $g = a_0 + a_1 z + \ldots + a_k z^k = o$ has only real roots, then every imaginary root of $F(z) = a_0 f(z) + a_1 f'(z) + \ldots + a_k f^{(k)}(z) = o$ lies within one of the ellipses described with the join of a pair of conjugate imaginary roots of $f(z) = o$ as minor axis, and with the major axis $\sqrt{k}$ times as long.

The first theorem has been proved by J. L. Walsh (*Ann. of Math.*, **22**, 1920, 128–44) as far as algebraic equations are concerned.

J. v. Sz. Nagy (*Jahresber. D. Math. Ver.*, **31**, 1922, 238–51) proves both of Jensen's theorems, and shows that they are also true if $f(z)$ is replaced by the function $e^{-\frac{1}{2}\gamma^2 z^2 + \delta z} f(z)$, where γ and δ are real numbers. Jensen's first theorem thus holds also for the roots of the equation $(-\gamma^2 z + \delta) f(z) + f'(z) = o$. Nagy generalises these theorems further.

The idea of the *class* (genre) of an integral function was introduced by Laguerre, who enunciated the proposition that the derivative of an integral function $f(z)$ of class p with only a finite number $2q$ of complex zeros has, besides the real zeros given by Rolle's theorem, only $2q + p$ other zeros, real or complex, and that, further, the class of $f'(z)$ is p. This was first proved by Borel (*Leçons sur les fonctions entières*, cap. 2) by the consideration of a sequence of functions having for limit the integral function. G. Valiron (*Bull. d. Sciences Math.*, **46**, 1922, 432–45) gives another proof, using the upper limit of the modulus of the logarithmic derivative and a theorem due to Jensen connecting the number of zeros and the number of poles of a meromorphic function within an annulus. He further shows that the class of a function of integral order cannot exceed the class of the function.

Hurwitz in 1889 proved that the Bessel function $J_{-\nu}(z)$ has exactly 2 $[\nu]$ complex zeros, where $[\nu]$ is the greatest integer in ν and, according as $[\nu]$ is even or odd, none or two are purely imaginary (*cf.* Watson: *Bessel Functions*, p. 483). E. Hilb (*Math. Zs.*, **15**, 1922, 274–9) investigates the complex zeros of $aJ_{\nu}(z) + bJ_{-\nu}(z)$, where a and b are real numbers. If $b \neq 0$, then for an even $[\nu]$ there are exactly $[\nu]$ complex zeros with a positive real part, but for an odd $[\nu]$ there are either $[\nu] - 1$ or $[\nu] + 1$ according as a/b is positive or negative. He applies the result in particular to the zeros of $Y_{\nu}(z)$.

Geometry.—C. Segre (*Atti Torino*, **56**, 1921, 143–57; **57**, 1922, 575–85) shows that if a surface belonging to at least four dimensions contains a double infinity of plane curves, it is either a cone or a ruled cubic of S_4 or a Veronese quartic surface of S_4 or S_5. It easily follows that if a surface belonging to space of five or more dimensions contains a triple infinity of curves

which belong to ordinary space, it is either a cone or a rational normal ruled quartic surface of S_5, the curves being rational cubics. Segre then investigates surfaces in space of five or more dimensions which have only a double infinity of curves belonging to three-dimensional space; they are (1) surfaces of S_5 contained in a V_3^3 (the locus of a rational ∞^1 of planes); (2) surfaces of S_5 or S_6 contained in the cone V_3^4 which projects from a point a Veronese quartic surface; (3) the elliptic normal ruled surface of order six of S_5; or (4) rational surfaces of S_6 or higher space which are representable on the plane by means of linear systems of cubics or by particular systems of quartics. The second note points out and supplies a logical defect in the proof, the result being unaffected.

In n-dimensional geometry varieties V_k of k dimensions often occur through each point of which pass an infinite number of straight lines lying wholly in the variety; if $\infty^i (i \geq 1)$ pass through any point of the variety it will contain ∞^{k+i-1} lines. We can always suppose that $i \leq k-2$, since for $i = k-1$ the variety would reduce to a linear space S_k, just as, for $k = 2$, the plane is the only surface with a double infinity of straight lines. For $k = 3$, the varieties of this kind are all known; if through any point of a V_3 there pass ∞^1 lines of the variety, it is a non-degenerate quadric of S_4 or else consists of ∞^1 planes. E. G. Togliatti (*Atti Torino*, **57**, 1922, 91–102) examines the case $k = 4$, and proves that if a V_4 is covered by ∞^4 straight lines (and not by ∞^5), it is either the locus of ∞^2 planes, or of ∞^1 non-degenerate quadric varieties V_3^2 (of S_4), or else belongs at most to a S_7, in which case it is obtained by cutting with the S_7 the V_6^5 of S_9 which represents, in Grassmann's way, the straight lines of S_4.

In space of three dimensions the surface represented by the vanishing of a symmetrical determinant of s rows and columns, of which the elements are homogeneous forms in four variables of degree m, has in general $\frac{1}{6}(s+1)s(s-1)m^3$ nodes (Salmon). Similarly, in space of four dimensions, the variety represented in like manner, the forms now being in five homogeneous variables, has a double curve of order $\frac{1}{6}(s+1)s(s-1)m^2$. B. Segre (*Atti Torino*, **58**, 1922, 162–70) determines the genus of this double curve, which he shows to be the partial intersection of three varieties which meet again in a second curve and touch along a third. The rank of the double curve is the number of points common to the curve and the Jacobian of the three varieties and two general linear forms, taking away the number of these points which fall at points in which the double curve meets the

further intersection of the three varieties. Thence the genus is proved to be

$$\frac{1}{8}(s+1)s^2(s-1)\,m^4-\frac{5}{12}(s+1)s(s-1)\,m^3+1.$$

For example, the Hessian of the general variety of order n in space of four dimensions ($s=5$) has a double curve of order $20\,(n-2)^3$ and genus $75\,(n-2)^4-50(n-2)^3+1$.

G. Scheffers (*Math. Zs.*, **16**, 1923, 43–77) investigates surfaces possessing conjugate nets of curves which can be represented on a plane by straight lines through a suitable projection. They are a large family and may be regarded as the dual of Lie's translation surfaces. They include developables, which have an infinite number of such nets. If such a surface is not a developable and has two different nets with the property, then the lines in the plane touch a curve of the fourth class.

W. Blaschke (*Math. Zs.*, **15**, 1922, 309–20) investigates the differential geometry of ruled surfaces in elliptic space, showing that they are characterised by four integral covariants, and that there is always a certain set of four such surfaces which are invariantly connected.

Blaschke has shown that surfaces for which conjugate points have a fixed geodesic distance have the following properties: (1) any point has only one conjugate point and conversely; (2) there are closed geodesics without singular points, all having the same length and all being bisected by a pair of conjugate points; (3) the line element is of the form $ds^2=du^2+G(u,v)dv^2$, u being the latitude and v the longitude and G a single-valued function of position on a sphere. He further guessed that the sphere is the only surface with these properties. P. Funk (*Math. Zs.*, **16**, 1923, 159–62) brings a certain amount of support to this guess by showing that the sphere cannot be varied so as to give another surface with the properties.

As far back as 1915–16 Bieberbach showed that of all plane finite regions of given diameter the circle has the greatest area, and Szasz and Rosenthal that of all convex curves of given diameter d the curves of constant breadth d have the greatest circumference. K. Reinhardt (*Jahresber. d. Math. Ver.*, **31**, 1922, 251–70) investigates the corresponding problems for polygons and proves that (1) for n an odd number, 3 or greater, of all plane simple n-gons of given diameter, the regular one has the greatest area; and (2) if n is not a power of 2, of all plane convex n-gons of given diameter d, those and those only have the greatest perimeter which have equal sides and are inscribed in Reuleaux polygons with constant breadth d, so that each vertex of the Reuleaux polygon is a vertex of the n-gon.

METEOROLOGY. By E. V. Newnham, B.Sc., Meteorological Office, London.

Annalen der Hydrographie und Maritimen Meteorologie, 1922, Heft x, p. 271, contains a summary of a long paper by W. Wiese, of Petrograd, entitled "Die Einwirkung des Polareises im Grönländischen Meere auf die Nordatlantische zyklonale Tätigkeit." The paper has not yet been published in Russian, nor is it likely to appear in that language for several years to come. It is possible that some of the relationships found may be accidental, and will need to be modified when more observations and completer information are available; but the main climatological peculiarities of years when ice is abundant in the Greenland seas, as compared with those of years when ice is scarce, appear so different that few meteorologists are likely to hesitate to accept the contrasts as genuine, and the paper represents an important advance in our understanding of the meteorology of Western Europe and the North Atlantic, while throwing some light upon the causes underlying the occurrence of periods amounting to a year or more during which the climate of Western Europe appears to have undergone an important change. In the following abstract much is omitted or abbreviated, but it is hoped that everything of the first importance has been included, particularly in those parts which have a direct bearing upon the meteorology of the British Isles.

According to Prof. Meinardus, the quantity of ice in the polar seas east of Greenland depends not only upon the régime of the winds, but also upon the amount of ice which collects in the regions near the source of the current which crosses the polar basin from Northern Siberia to East Greenland.[1] The surface waters of the polar seas are not everywhere equally favourable for extensive formation of ice, but are particularly favourable for this purpose north of the mouths of the great Siberian rivers. This region may be called the "factory" for north-polar ice. The amount of ice in the seas eastward of Greenland may therefore be expected to depend to some extent upon the temperature in the "factory" during the time of principal ice-formation, namely in autumn. Observations for a long period are unfortunately not available for the exact area required, which centres about the month of the Lena, but those made at Obdorsk (66° 31′ N., 66° 35′ W.) and Turuchansk (65° 55′ N., 87° 38′ W.) between 1877 and 1910 serve sufficiently well. For the quantity of Greenland ice the eastward extension of the ice between latitudes 67° and 71° N. will serve, and this is obtainable directly from the charts prepared by the Danish Meteorological Institute. Since four and a half years represents

[1] *Ann. d. Hydr.*, 1904, p. 362.

roughly the time taken by ice in travelling from Northern Siberia to Greenland, the mean temperature for Obdorsk and Turuchansk in autumn was tabulated against the quantity of ice in the region just mentioned between April and July five summers later, and a correlation coefficient was calculated. This worked out at $-0{\cdot}83 \pm 0{\cdot}05$, which implies a considerable degree of interdependence, a warm autumn resulting in a small quantity of ice being available for transportation. Since the temperature in the north of Iceland in spring is dependent to a large extent upon the quantity of ice off Greenland, we might expect to find some connection between the mean temperature each spring in that region and the mean temperature in the Siberian area in autumn, four and a half years earlier, although the existence of other factors controlling Icelandic temperature would lead one to expect only a small correlation coefficient. The coefficient actually found was $+0{\cdot}27 \pm 0{\cdot}11$.

In order to study the influence of the Greenland ice upon the mean distribution of pressure at different seasons over the North Atlantic, the classification of Meinardus of the years 1880–1916 into years of plentiful ice (which we may denote by E +) and of scarcity of ice (E –) has been employed. The mean atmospheric pressure over the North Atlantic for E + and E – years, divided according to season, was then calculated and mean isobars were drawn, with the following results :

1. *Spring.*—In E + years the Icelandic low-pressure centre was less well developed than in E – years, while the Greenland anticyclone extended farther east. Compensation for the exceptionally low pressure in the Icelandic region in E – years was found to take place in the high-pressure systems of the Azores and Siberia, which were then unusually well developed. Thus in E – years the pressure gradient for westerly and south-westerly winds over Europe and the North Atlantic was steep, and the wind circulation vigorous in consequence.

2. *Summer.*—It is evident that at this season the variations in the amount of polar ice can have a considerable effect upon the temperature in these regions. A rough computation of the amount of ice melted daily in the Greenland seas between latitudes 65° and 75° shows that sufficient heat is normally absorbed to lower the air temperature about 2° C. up to a height of 3 km. Since the quantity of ice in individual years departs as much as 30 per cent. from the normal, a variation of over 1° C. might result in this way. Although the exact effect of these variations of absorbed heat upon the distribution of pressure cannot be estimated, the isobars for E + and E – years differ very notably. In E + years there is a considerable

eastward expansion of the Greenland anticyclone, and a general displacement of the isobars towards the south.

3. *Autumn.*—At this season the effect of the quantity of polar ice upon the pressure distribution is particularly well marked, and must be attributed to the action of the ice upon the spreading out of polar water, which is greatest at this season. The greater part of this spreading out takes place towards Norway and Barents Sea by the agency of the East Iceland current, which fact explains why the effect of the quantity of ice is observed to the east of Greenland rather than to the west. In E+ years the North Atlantic pressure minima are feebly developed, particularly the centre over Barents Sea, and the high-pressure systems to the south are displaced equatorwards. This effect is particularly pronounced towards the British Isles, the mean pressure at Valencia changing from 762 mm. in E− to 759 mm. in E+ years. The Siberian anticyclone retreats far towards the east, the 765-mm. isobar being found in longitude 40° E., whereas in E− years it actually reaches longitude 0°, with a separate centre over the western part of Central Europe. As in spring, there is a vigorous atmospheric circulation over the northern part of the North Atlantic in E− years, the low-pressure centres being well developed and the high-pressure belt being north of its normal position.

Another method of studying the effect of the quantity of Greenland ice upon the distribution of pressure is to consider the paths of cyclones. Calculating for ten-degree intervals of longitude the mean latitude of all cyclonic centres occurring within these intervals, for E+ and E− years, striking differences are obtained, as the table on page 9 shows. Here again the greatest effect is observed to be in the autumn, when between longitudes 60° and 80° E., the mean path is nearly 6° farther south in E+ years than in E− years. In the spring the effect is practically confined to these longitudes, and at all three seasons is greater hereabouts than near the American continent.

Yet another method of dealing with this problem is the construction of lines of equal frequency of occurrence of cyclonic centres. These show in spring and summer two main centres of activity, towards which, it may be noted, travelling cyclones tend to move. One centre is near Iceland, another, more intense, in Barents Sea. To these must be added in autumn a centre near Greenland. Cyclones which leave these centres show a tendency to follow certain definite tracks. Those leaving the Icelandic centre either move north-eastwards, keeping near Greenland, or eastwards towards Norway, or south-eastwards towards Great Britain; those from Barents

Sea either towards the northern or southern parts of Nova Zembla. In E + and E − years these centres of activity and dominant tracks present certain contrasts.

Spring.—In E + years the two main centres are feebly developed and the Greenland track is displaced between 2° and 5° S. Near its origin it is indistinguishable from the track from Iceland to Norway, the two first becoming distinct in longitude 10° W. From this point two tracks emerge, one passing south-eastwards towards Sweden, the other, which is comparatively feebly developed, north-eastwards. The British track is not well developed in these years. As regards the two paths leading towards Nova Zembla, the more southerly is generally followed.

In E − years the Iceland–Norway track divides into two secondary tracks near the Norwegian coast, one to the north-east along this coast and the other across Sweden, rejoining the first in the Murman Sea. The British track is well developed during these years, and the northernmost of the Nova Zemblan tracks particularly so. This method of treatment, it will be noticed, brings out differences that are not shown when single mean paths only are constructed.

Summer.—In E + years there is a general displace-

		60° W.	50° W.	40° W.	30° W.	20° W.	10° W.	0°	10° E.	20° E.	30° E.	40° E.	50° E.	60° E.	70° E.	80° E.
Spring	E −	53·0°	53·6°	55·4°	58·8°	61·1°	62·0°	63·5°	64·8°	65·9°	67·5°	68·5°	70·2°	71·2°	71·3°	72·4°
	E +	49·2°	54·0°	56·8°	58·9°	60·5°	61·2°	62·9°	64·8°	66·7°	67·9°	69·2°	68·5°	67·8°	66·7°	66·8°
	diff.	+ 3·8°	− 0·4°	− 1·4°	− 0·1°	+ 0·6°	+ 0·8°	+ 0·6°	—	− 0·8°	− 0·4°	− 0·7°	+ 1·7°	+ 3·4°	+ 4·6°	+ 5·6°
Summer	E −	56·9°	57·3°	58·1°	60·0°	62·7°	63·8°	65·5°	67·3°	69·3°	70·6°	72·4°	72·1°	71·7°	71·8°	69·8°
	E +	54·3°	53·9°	55·9°	58·1°	59·7°	60·5°	62·4°	64·4°	66·3°	67·3°	68·8°	69·4°	68·7°	67·2°	68·0°
	diff.	+ 2·6°	+ 3·4°	+ 2·2°	+ 1·9°	+ 3·0°	+ 3·3°	+ 3·1°	+ 2·9°	+ 3·0°	+ 3·3°	+ 3·6°	+ 2·7°	+ 3·0°	+ 4·6°	+ 1·8°
Autumn	E −	54·2°	55·8°	57·9°	60·8°	63·0°	65·6°	68·0°	69·0°	71·0°	71·8°	71·9°	72·0°	72·2°	71·0°	72·7°
	E +	54·2°	55·5°	57·9°	59·2°	61·0°	61·7°	63·5°	65·3°	66·8°	67·6°	67·6°	66·7°	66·2°	66·0°	66·7°
	diff.	—	+ 0·3°	—	+ 1·6°	+ 2·0°	+ 3·9°	+ 4·5°	+ 3·7°	+ 4·2°	+ 4·2°	+ 4·3°	+ 5·3°	+ 6·0°	+ 5·0°	+ 6·0°

ment of the low-pressure centres towards the east as a result of the extension of the Greenland anticyclone in the same direction. The Greenland and Nova Zemblan tracks are feebly developed. That towards Norway is well developed, but the depressions tend to turn eastwards instead of proceeding north-eastwards towards the Barents Sea centre. The British track is particularly well developed.

With E – the Icelandic centre is in its normal position, while the Barents Sea centre is between longitudes 10° and 15° E. The Norwegian track is well developed and passes north-east up the coast in a normal manner, while the British track, instead of continuing south-eastwards across the North Sea, turns north-east.

Autumn.—At this season the general contrasts between E + and E – years that have been noted in spring and summer are particularly well developed. Depressions with E – tend to favour the Greenland and Nova Zemblan routes, while with E + the Norwegian and British tracks are more favoured.

From the foregoing it is seen that the Greenland ice-fields produce climatological effects over a wide area, and it had previously been noted that the temperature in autumn over Northern Siberia affects these ice-fields several years later in consequence of the drifting of the ice across the polar basin from Siberia to Greenland. Now, when the cyclonic tracks are displaced southwards (in E + years), the region of ice-formation north of Siberia experiences an increase in the frequency of northerly winds, consequently the state of the ice in the Greenland seas influences the autumn temperature in Northern Siberia during the same year. In this way a series of four- to five-year periodicities is set up, this period representing the time taken by ice in drifting across the polar basin. These considerations may possibly explain the 4·5-year periodicity found by Meinardus with regard to the state of the ice in Iceland, as well as the 4·8-year period in the quantity of Greenland ice found for the years under consideration in the present paper.

Without going exhaustively into the climatological results of the changes in the distribution of pressure brought about by E + and E – conditions, a few may be mentioned which are particularly marked. In E + years rain is below the normal in Ireland and northern England in spring, while in E – years it is above the normal; in autumn E + gives a rainfall notably above the normal in south-eastern England, and E – one equally below normal, the difference amounting to fully 50 per cent. of the normal. Great contrasts of temperature between the two types of year are prevented by the fact that increased supplies of polar air, which tend to produce low temperatures, tend also towards clearer skies and increased solar heating at the

seasons here considered. In so far, however, as E+ years tend to increase the frequency of anticyclonic conditions, they tend to produce the continental phenomenon of big variations of temperature—warm days and cold nights—and this can readily be detected at certain British stations. The increased intensity of the Greenland anticyclone with E+ results in more numerous late spring frosts. Thus at Douglas, Isle of Man, during thirty-two years, temperatures of 32° F. or less occurred seven times in E+ years, once with normal ice conditions, and on no occasion with E−. In Scotland there is even a hint of the *mean* temperature being affected, for in October the mean was found to be nearly 3° F. lower with E+ than with E−. The figures for the whole of autumn give, however, a difference of only slightly over 1° F.

An interesting paper by A. H. R. Goldie, entitled "Circumstances determining the Distribution of Temperature in the Upper Air under Conditions of High and Low Barometric Pressure," appears in the *Quarterly Journal of the Royal Meteorological Society* for January 1923, vol. xlix, No. 205.

The author first refers to a mathematical discussion of certain statistical relationships between temperature and pressure in the upper air published by Hesselberg in 1915 ("Über den Zusammenhang zwischen Druck- und Temperaturschwankungen in der Atmosphäre," *Met. Zeit.*, **32**, 1915, pp. 311–18). In this discussion Hesselberg showed that the changes of temperature up to a height of 9 km. which correspond with various changes of pressure are much larger than would result from the introduction of additional masses of air sufficient to give the changes of pressure, at whatever level the addition may be made. He concluded from this fact that the air masses between 0 and 9 km. after a pressure increase have on the average a higher mean temperature than the masses that were formerly there. He proceeded farther and found a mathematical expression for the increase of temperature per unit of time for a point in space. The final expression arrived at showed that temperature changes as a result of the following causes:

(1) Adiabatic change due to change of pressure.

(2) Change due to introduction—horizontally or vertically—of air of a different temperature from neighbouring regions.

(3) Contribution of heat to or abstraction of heat from the air by radiation, convection, condensation, etc.

Goldie sets out to show the importance of (2), and in particular of the introduction of air of different temperature from more northerly or more southerly latitudes. The temperatures and pressures found on 165 aeroplane ascents made from October to December 1921 were divided according to whether

they were made in "equatorial" or "polar" air, or partly in one and partly in the other. Correlation coefficients were found for each month between pressure and temperature, irrespective of height, for pressures between 950 and 750 millibars, and between 700 and 500 millibars in "polar" and in "equatorial" air, in order to see whether a closer relationship existed between these two variables in the one case than in the other. It was found that in seven out of the eight coefficients the correlation was higher in "equatorial" than in "polar" air, and that in "equatorial" air the correlation was in every case decidedly higher in the region 700–500 mb. than for the higher range of pressure. No such difference existed in the "polar" air. The explanation for this would seem to be that air which has been for some time in polar regions probably reaches a fairly definite state with regard to the temperature for a given height or a given pressure, the same being true for equatorial air, but that whereas the latter is cooled principally in a shallow layer near the surface as it moves towards temperate latitudes, the higher regions being nearly unaltered, in the case of the polar air warming takes place at the surface and results at once in instability and turbulent motion which causes heat to be continually transferred upwards. Thus in polar air the temperature corresponding with a given pressure will vary according to the time taken by the air on its journey, and according to the track followed. In equatorial air, on the other hand, the relationship between pressure and temperature will, except in the surface layers, remain for a long time what it was before it left equatorial regions, whence the higher correlation. When temperature was plotted against pressure, the curves were found to be quite different in the two different supplies of air; the equatorial was the warmer by from 10° to 15° at all heights considered, *i.e.* up to about 5 km., and the difference was greatest for a pressure of 850 mb. This result negatives the theory that cyclones are symmetrical at these levels, and shows that temperature is a function of the source of the air, notwithstanding that there may be no correlation between wind direction and temperature at one place.

The author then considered some cases where the air was partly polar and partly equatorial. One of particular interest may be mentioned. Between 17 h. G.M.T. on October 13th and 10 h. on the 14th, air that was entirely equatorial up to 12,000 ft. was replaced by polar air up to about 4,000 ft., the temperature falling roughly 15° F. in consequence, while above 6,000 ft. the air remained entirely equatorial. It appeared that this equatorial air was lifted over 200 ft. by the wedge of cold air, and pressure rose from 1,020 to 1,031·5 mb. at the surface. In the author's opinion, phenomena of this kind may account

for most of the pressure changes in our latitudes. Investigations were made into the changes of pressure occurring in samples of surface polar and equatorial air as they proceeded over considerable distances, and the following conclusions were reached :

(1) In polar air the pressure falls so long as it is being overrun by equatorial air.

(2) In polar air the pressure rises so long as it is undercutting equatorial air.

(3) In equatorial currents there is as a rule comparatively little change of pressure along a trajectory, at least until it is about to overrun polar air.

The paper concludes with some suggestions as to the probable constitution of anticyclones and cyclones in temperate latitudes.

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PHYSICAL CHEMISTRY. By W. E. Garner, M.Sc., University College, London.

The Constitution of Ampholytes.—A theory of ampholytes has been put forward by Niels Bjerrum (*Zt. Phys. Chem.*, 1923, **104**, 147–73), which, if substantiated by further investigation, will entail radical changes in the theory of the colloidal behaviour of the proteins, especially in the region of the isoelectric point. An amino acid, $NH_2 . R . COOH$, is one of the simplest ampholytes, and according to the current view, a solution of an amino acid contains the two ions, $NH_2 . R . CO\bar{O}$, and $\overset{+}{N}H_3 . R . COOH$, and the undissociated substance $NH_2 . R . COOH$. The "zwitterion," $\overset{+}{N}H_3 . R . CO\bar{O}$, is not believed to be present in considerable amount in such solutions. If these substances be represented by $\overset{+}{A}$, A, and $\bar{A}$ respectively, then the acid and basic dissociation constants are $[\bar{A}][\overset{+}{H}]/[A] = k_a$, and $[\overset{+}{A}][O\bar{H}]/[A] = k_b$. These calculated dissociation constants for both the basic and the acidic groups are much lower than those for the same groups in most of the known amines and carboxylic acids. This result is contrary to what would have been predicted from the mode of ionisation of dibasic acids, where the second ionisation constant is usually 10 to 10,000 times smaller than the first. Since in these acids the ionisation of the first carboxyl group represses that of the second, an intensification of ionisation would be expected to occur when the two unlike groups, amino and carboxylic, were in juxtaposition in the same molecule.

Although the conception of the "zwitterion" has not hitherto been seriously employed in the development of the

theory of the amino acids, it has been found useful in explaining the colour changes of indicators. Thus Kuster (*Zt. Anorg. Chem.*, 1897, **13**, 135) ascribed the change in colour of methyl orange, from yellow to red, to the formation of $\overset{+}{H}(CH_3)_2N.C_6H_4.N_2.C_6H_4.\overset{-}{SO_3}$, and, in acid solutions, methyl orange was considered to give this ion almost entirely. Bjerrum makes a similar assumption for the amino acids which he regards as giving $\overset{+}{N}H_3.R.COOH$, $NH_2.R.CO\bar{O}$, and $\overset{+}{N}H_3.R.CO\bar{O}$ in aqueous solutions, the undissociated acid being present in negligible proportions. This gives the acid and basic dissociation constants the values, $K_S=[A^{+-}][H^+]/[A^+]$, and $K_B=[A^{+-}][OH^-]/[A^-]$, from which $K_S=K_{H_2O}/k_b$, and $K_B=K_{H_2O}/k_a$. The new dissociation constant for the carboxyl group is identical with the hydrolysis constant, corresponding to the old basic dissociation constant. The new values for K_S range from 10^{-2} to $10^{-3 \cdot 5}$, and for K_B from 10^{-4} to 10^{-6}, values much greater than those usually accepted, and somewhat larger than those for the simple carboxylic acids and amines. Bjerrum shows that the relation between the chemical constitution of the amino acids and the ionising tendencies of their carboxyl groups is clearly demonstrated by the new values. Thus lysine and arganine, which contain two amino groups, would be expected to possess strongly dissociating carboxyl groups, and likewise the amino group in asparagine, which has two carboxyl groups, should be strongly dissociated. The new dissociation constants are in agreement with these predictions. Arguments are advanced to show that the undissociated substance $NH_2.R.COOH$ can only be present from 0·1 to 3 per cent., and that the amino acids are almost exclusively present as the "zwitterion." The amino-phenols are exceptional, the concentration of the undissociated electrolyte being apparently greater than the concentration of the "zwitterion."

This argument possesses considerable interest in view of the recent work of Loeb, Pauli, Michaelis, and others, on the colloidal behaviour of proteins. These substances should be largely dissociated at the isoelectric point into the "zwitterion," and $\overset{+}{H}$ and $\overset{-}{OH}$, the colloidal micelle containing an equal number of positive and negative ions, and thus being electrically neutral. Loeb (*Science*, 1922, **56**, 731–41), in a summary of his application of the Donnan membrane equilibrium to the swelling, viscosity, and osmotic pressure of the proteins, emphasises the non-ionic character of isoelectric protein. It is conceivable that the replacement of the undissociated isoelectric protein by the "zwitterion" will not appreciably affect Loeb's work in its

bearing on the Donnan equilibrium, or in regard to the Proctor and J. A. Wilson theory of the swelling of gelatine. According to the latter theory the protein micelle is honeycombed with cells into which electrolyte may diffuse from outside. In acid solution, protein chloride is formed inside the micelle, which cannot diffuse through the cell walls. Acid will diffuse, however, into the protein structure, setting up a Donnan equilibrium, when $[H^+]_1[Cl^-]_1 = [H^+]_2[Cl^-]_2$ represents the relation between the hydrogen and chlorine ion concentrations inside (1) and outside (2) the protein micelle. Since $[\bar{Cl}]_1$ is greater than $[H^+]_1$ and $[H^+]_2 = [\bar{Cl}]_2$, a greater concentration of ions will be produced in the inside solution, and hence osmosis inwards and swelling must occur. In the case of gelatine the differences in osmotic pressure between the two sides of the membrane, calculated from the Donnan equilibrium, give a maximum value at an acid concentration of about $P_H = 3{\cdot}0$, and this coincides with the acid concentration at which the maximum swelling of the gel and the maximum viscosity of gelatine solution occurs.

According to Pauli these maxima correspond with a maximum in the differential ionisation of the protein (*Colloid Chemistry of the Proteins*, p. 63). From the results of Manaba and Mantula on ox-blood serum $[H^+]_1 - [Cl^-]_1$ is a maximum when the viscosity is at a maximum. This, however, does not hold for gelatine, where $[H^+]_1 - [\bar{Cl}]_1$ only slowly approaches a maximum (Loeb, *Proteins and the Theory of Colloidal Behaviour*, p. 175) which lies at a higher acid concentration than that of maximum osmotic pressure. For gelatine it would appear that the Proctor-Wilson theory of swelling, based on the Donnan membrane equilibrium, gives an explanation which is as quantitative as would be expected from the complex nature of the ionisation processes of protein salts. On p. 193 Loeb states that " at the isoelectric point gelatine is practically not ionised, and therefore there can be no Donnan equilibrium. Yet, when dry grains of isoelectric gelatine are put into water of P_H 4·7 a considerable swelling occurs." It is here that the view of Bjerrum on ampholytes will perhaps find application, for if the " zwitterion " state be dominant for amino acids, this will be present in the ionic micelle, and exert an osmotic pressure which will account for the swelling.

The Swedberg and E. R. Jette (*J.A.C.S.*, 1923, **45**, 954–7) have made use of ultraviolet light for the measurement of the cataphoresis of proteins. Previously by the direct measurement of the moving protein boundary has never been possible, since in ordinary light a dilute protein sol can be distinguished from

a colourless solution only with great difficulty. The boundary surfaces can be made visible, however, if the solutions are illuminated with ultraviolet light, for proteins absorb ultraviolet light, and the absorption is accompanied by a strong fluorescence. By means of a quartz mercury lamp of 3,000 candle-power, the effect of variation in $\overset{+}{H}$ concentration on 0·3 per cent. egg albumen has been studied in this manner.

J. Holker (*Proc. Roy. Soc.*, 1923, A **102**, 710) has made measurements of the periodic opacity which occurs when solutions of electrolytes are added to solutions of human serum. The changes in opacity, which are sometimes visible to the naked eye, were measured in an opacimeter. The periodic changes in opacity are considered to be due to flocculation caused by alternate adsorption of cations and anions. This variation in the nature of the ions adsorbed must be associated with some rearrangement of the atoms of the surface of the colloid; the mechanism of this change is as yet obscure.

The Third Law of Thermodynamics.—G. E. Gibson and W. F. Giauque (*J.A.C.S.*, 1923, **45**, 93–104; *cf.* Science Progress, 1921, **59**, 373) have carried out further experimental work to test the inequality contained in the third law, which demands that the entropy of a non-crystalline substance may be greater but can never be less than zero at the absolute zero. Wietzel, for silica, and Gibson, Parks, and Latimer for the alcohols, came to the conclusion that the entropy of the supercooled form is greater than that of the crystalline form at the absolute zero; but difficulties in the determination of specific heats, and heats of crystallisation at high temperatures, leave some uncertainty in these conclusions. By determinations of the specific heats and heats of crystallisation of glycerol, which can be readily supercooled, the deduction of G. N. Lewis and Gibson from the third law has been confirmed. The specific heat of the glass and of the crystals approach one another as the temperature is lowered, and are almost identical below 140° K. The entropy of supercooled glycerol exceeds that of crystalline glycerol by 5·6 ± 0·1 cal/degree per mol at 70° K., and it is concluded that this value will not be appreciably different at the absolute zero. In order to account for this difference on the suggestion of Nernst and Wietzel that the specific heat of the glass may decrease much more slowly than that of the crystals below 1° K., it would be necessary for the specific heat of glycerol glass to remain constant at the improbably high value of 0·25 cal/degree from 1° K. to 1×10^{-100} K. The evidence furnished by glycerol is therefore strongly in favour of the interpretation of the third law given by Lewis and Gibson.

The New Element.—The oxide which was isolated by Scott from a New Zealand sand has been subjected to a searching examination by means of X-ray and by optical spectral analysis by Coster and Hevesy, with the hope of finding in it some of the other missing elements, in particular that of atomic number 75, but without success. Scott (*Trans. Chem. Soc.*, 1923, **123**, 881) has analysed this material, and reaches the conclusion that the very refractory material in the New Zealand sand may be regarded as titanium oxide in which part of the titanium is replaced by silicon.

BIOCHEMISTRY. By R. Keith Cannan, M.Sc., University College, London.

The past few years have witnessed fundamental advances in biochemistry, pre-eminently in problems of nutrition and in our views of vital oxidation and of muscle contraction. The work of consolidation and extension proceeds apace, and its thoroughness gives promise of larger progress in due season; but, at the moment, these and other problems are overshadowed by a notable achievement in the field of carbohydrate metabolism. The measure of success already attained in the therapeutic use of "insulin" in diabetes—a success exuberantly embellished by the daily press—has led to a renewed zeal in the attack on the problems of carbohydrate metabolism, which promises to lead us far towards a better understanding of this difficult subject.

Carbohydrate Metabolism and Diabetes.—Since the classical researches of Von Mering and Minkowski, diabetes has been associated with a disturbance of the internal secretion of the pancreas, but the critical test of this relation could never be brought off with certainty. For years a preparation of the pancreas was sought which, on injection into the depancreatised animal, would allay the characteristic diabetic symptoms—notably the hyperglycæmia and resultant glycosuria. Occasional positive results have been reported, but consistent, and therefore conclusive, success has been reserved for two young Canadian clinicians, F. G. Banting and C. H. Best. Starting from the premises that the islets of Langerhans were responsible for the hormone sought, and that failure hitherto had been due to the destruction of the latter by the external secretion of the gland, Banting and Best (*J. Lab. Clin. Med.*, 1922, **7**, 251) made extracts of fœtal pancreas (in which the islet tissue alone is active). Upon injecting these into depancreatised dogs, a striking reduction in the blood sugar was recorded and the animals showed marked improvement in condition. The next step was the attempt to use normal ox pancreas, of which large quantities would always be available, by extracting

under conditions likely to inhibit tryptic activity. They followed, but with greater success, the method previously used by Scott (*Amer. J. Physiol.*, 1912, **29**, 3) involving the extraction of the minced glands with 50 per cent. alcohol, removal of fat and protein, and precipitation, by raising the concentration of alcohol to 90 per cent., of a fraction which was found to contain the active principle. The results of the trial of this material on dogs was so definite that the treatment of a human diabetic was essayed with equally definite success. The full resources of the Toronto laboratory were thrown into the work, with the result that in a very short time the method was sufficiently standardised for extended clinical trial. (*Canad. Med. Assoc. J.*, March 1922; *Amer. J. Physiol.*, 1922, **42**, 162.) These preparations, which received the name of "insulin," have been in clinical use in Canada and America for a full year (*Proc. Roy. Soc. Canada*, 1922, **16**; *Brit. Med. J.*, January 6, 1923), whilst during the last three months many British hospitals have been able to confirm the general results of the treatment. Satisfactory preparations are already on the market, and "insulin" is assured of a fair trial in this country.

The study of the biochemical function of the pancreatic hormone which is now rendered possible presents many intriguing questions. Insulin lowers the blood sugar of the normal and of the diabetic animal indiscriminately except in so far as the effect is the more pronounced the greater the depletion of the glycogen stores (MacLeod and others, *J. Physiol.*, 1923, **57**, 234), the excretion of sugar and of acetone bodies is reduced or stopped, and the respiratory quotient shows a transient rise. In the depancreatised dog the glycogen of the liver is replenished, but preliminary experiments on normal rabbits suggests that in the non-diabetic animal insulin retards the normal accumulation of liver glycogen after a carbohydrate meal. It is very doubtful if one may argue from the rise in the respiratory quotient that the catabolism of sugar is increased, for there would seem to be no concurrent rise in oxygen consumption, and only a transitory increase in carbon dioxide production which is difficult to interpret (Dudley, Trevan, and others, communication to March meeting of the Physiological Society). There is undoubtedly a fundamental redistribution of the carbohydrate of the body under the influence of insulin, but there is yet no explanation of the pancreatic function of which this is the expression. The problem is elusive but full of promise.

Meanwhile, along rather different lines, interesting speculations are being pursued. It seems to be a convenient and almost unavoidable hypothesis that dextrose prior to utilisation is converted into some more reactive form. The view of

Woodyatt and others, that in the body the glucose molecule is fragmented much as Nef pictured its disintegration in alkaline solution, has much to commend it, but our ignorance of the mechanism of such a change has led to the search for a more definite statement of the biochemical activation of hexoses. Stimulated by the important part phosphates would seem to play in alcoholic fermentation, the thought that a hexosephosphoric acid may be the form in which carbohydrate is catabolised is engaging attention. Embden and his school (*Z. Physiol. Chem.*, 1914, **93**, I, 94, 124) have advanced evidence that just such a compound is the lactic acid precursor in the muscle, whilst several attempts have been made recently to show that phosphates have a specific catalytic function in the oxidation of glucose *in vitro* by hydrogen peroxide. Witzemann (*J. Biol. Chem.*, 1920, **45**, I) has confirmed and extended this view, originally due to Löb, though he is inclined to attribute the catalysis to perphosphate rather than to hexosephosphate formation. It must be recorded, however, that Harden and Henley (*Biochem. J.*, 1922, **15**, 672), in carefully controlled experiments, have been unable to associate with phosphates any rôle in this reaction other than that of regulators of the hydroxyl-ion concentration. It were perhaps sounder progress to become more familiar with the characteristics of the various compounds of sugars and phosphoric acid before staking biochemical claims for them. Levene (*J. Biol. Chem.*, 1921, **48**, 233; and 1922, **53**, 431) in America, and Neuberg (*Biochem. Z.*, 1921, **121**, 326) in Germany, are pursuing a systematic study of the synthetic esters, and we may note that these two workers have also given attention to the sulphuric acid esters of glucose in view of their relation to the glucoproteins. In this country Robison (*Biochem. J.*, 1922, **16**, 809) has recently isolated from actively fermenting yeast juice a hexosemonophosphate which is distinct from the compound obtained by Neuberg from the partial hydrolysis of the fermentation diphosphate. The two latter substances both give a lævorotatory sugar on removal of the acid, whereas the new compound yields a reducing substance of dextrorotation. The properties of this ester have already led to attractive hypotheses in unexpected relations, but a discussion of these must await publication of the experimental results.

Another suggestion as to the form of sugar concerned in metabolism is the outcome of the laborious studies over many years of Irvine and his co-workers on the structure of the carbohydrates. As a result of this work, it has been firmly established that hexoses can exist in their derivatives in a structure other than that of the accepted butylene oxide form. Whether it be finally decided to assign to these "γ sugars"

(an unfortunate name in view of the confusing history of sugar nomenclature) the propylene oxide, ethylene oxide, or other structure, their significance lies in their extreme susceptibility to oxidation and condensation. Hewitt and Pryde were the first to suggest a biochemical importance for these γ sugars from the optical changes undergone by equilibrated solutions of hexoses introduced into the intestine. They offered the suggestion that dextrose and lævulose in contact with the living mucous membrane of the intestine were converted into their reactive isomers (*Biochem. J.*, 1920, **14**, 395). In a later paper Hewitt and De Souza (*Biochem. J.*, 1921, **15**, 667) came to the conclusion that the sugar of the blood was not of the new type. Recently, however, Winter and Smith (*J. Physiol.*, 1922, **57**, 100) have definitely made the suggestion that the sugar of the blood exists in the γ form. This paper was rapidly followed by others advancing evidence to show that in diabetic blood the sugar is the normal stable variety (*Proc. Physiol. Soc.*, 1922, **57**, xxxi), that after the injection of insulin into the diabetic γ glucose appeared in the blood (Forrest, Winter, and Smith, *J. Physiol.*, 1923, **57**, 224), and that in the liver is found an enzyme which, when activated by the pancreatic hormone, converts normal hexose into its reactive variety (*Proc. Physiol. Soc.*, 1922, **57**, xiii). The obvious suggestion that the diabetic fails to metabolise sugar because of his inability to effect this process of activation is undoubtedly attractive, but the technique employed in all these investigations is open to grave criticism. From the properties of the γ sugars reported by Irvine, it would seem almost inconceivable that such unstable molecules could survive the involved chemical manipulation to which the blood filtrates were submitted. After all, processes of activation with which we are familiar all take place within the cell, whilst the blood constituents are generally regarded as a stable circulating currency; there would seem to be no physiological reason for such a flaunting in the blood of reactive material. At the same time, further investigation of the nature of the blood sugar will be very welcome.

So much space has been devoted to these interesting problems that it is only permitted to mention and welcome a few notable contributions to biochemistry in one or two other fields.

Enzymes.—Neuberg has demonstrated in yeast juice the existence of an interesting enzyme capable of building up carbon chains. For example, it condenses benzaldehyde and pyruvic acid to form *l*-acetyl phenyl carbinol, HO . CH . C_6H_5 . $COCH_3$. He has given the catalyst the name of "Carboligase" (*Biochem. Z.*, 1922, **127**, 327; **128**, 610). Turning his attention to the butyric acid fermentation, Neuberg suggests that the

intermediate step in the conversion of the hexose into a fatty acid is the aldol condensation of two molecules of pyruvic acid to form

$$\begin{array}{l} CH_3.\overline{C.CH_2.CO.O} \\ \qquad\ \ | \\ \qquad COOH \end{array}$$

rather than the formation of aldol from acetaldehyde. He was able to isolate small amounts of high fatty acids from the action of *B. butylicus* on starch paste (*Biochem. Z.*, 1921, **117**, 269).

The advent of R. Willstatter into the field of enzyme chemistry will be welcomed in view of his success in other branches of plant chemistry. His project is wide, for he has already published papers on invertase, maltase, lactase, raffinase, emulsin, and peroxidase (*Z. Physiol. Chem.*, 1922, **115**, 186, 199; **116**, 53; **117**, 172; **121**, 183; **123**, 1), but it is yet too early to draw conclusions from his work.

Meanwhile the two views of the nature of enzyme action—the physical and the chemical—appear to be drawing nearer under the influence of the views of Lewis and Langmuir of surface activity (E. F. Armstrong and Hilditch, *Proc. Roy. Soc.*, 1919, A. **96**, 137).

Biochemical Oxidation.—Bach returns to the criticism of Wieland's theories of dehydrogenation (*Ber.*, 1922, **55** B, 3564), though the latter still gain ground and receive support from the notable isolation from tissue by F. G. Hopkins of a thermostable auto-oxidisable peptide to which has been given the name "glutathione" (*Biochem. J.*, 1921, **15**, 286). A second paper dealing with this substance has appeared (*J. Biol. Chem.*, 1922, **54**, 527) in which glutathione is shown to have a real function in cellular respiration. It is associated with certain insoluble thermostable hydrogen donators in the cell together with which it forms a thermostable oxidation system distinct from the enzymic mechanism and having an independent function. This has been a notable discovery.

The progress of investigation in other directions has been real, and a discussion of recent work in protein chemistry, in vitamins, and in the acid-base equilibria of the blood are some of the problems that will demand attention in a future report.

GEOLOGY. By G. W. Tyrrell, F.G.S., A.R.C.Sc., Ph.D., University, Glasgow.

Stratigraphical Geology.—The Coventry region, the geology of which is described by the Geological Survey in a recently published memoir (*Geology of the Country around Coventry*, . . .

Expl. of Sh. 169. London, 1923, pp. 149), contains Pre-Cambrian and Cambrian formations each with igneous rocks of considerable industrial importance. Then there are Carboniferous rocks including the major part of the Warwickshire Coal-field ; and finally Triassic rocks unconformably covering the older formations. A very full account of the stratigraphy of the Warwickshire Coal-field, parts of which fall into contiguous map areas, is given.

Welsh stratigraphy figures prominently in recent numbers of the *Quarterly Journal of the Geological Society.* Miss G. L. Elles describes the structure and rock succession of the classic Bala country after detailed mapping in that complicated and difficult region (*Q.J.G.S.*, **78**, pt. 2, 1922, 132–75). The Lower Palæozoic rocks of the Llangollen district are described by L. J. Wills and B. Smith, with special reference to the tectonics (*ibid.*, 176–226). The structures are complicated, but are elucidated with the aid of an excellent set of serial sections. L. D. Stamp and S. W. Wooldridge have shown that the igneous rocks of Llanwrtyd (Brecon) are spilitic in character, and are mostly of pyroclastic origin (*ibid.*, **79**, pt. 1, 1923, 16–46). The stratigraphical and palæontological evidence proves that they are of the same age as the Upper Igneous Series of Snowdonia, Cader Idris, and the Bala country.

In a paper on the nature and origin of the Pliocene deposits of Cornwall, and their bearing on the Pliocene geography of the south-west of England, H. B. Milner shows that the petrographical characters of the St. Agnes, St. Erth, and St. Keverne deposits are substantially the same, and suggest derivation from a homogeneous distributive province such as that furnished by the Palæozoic rocks of Cornwall (*Q.J.G.S.*, **78**, pt. 4, 1922, 299–347). Since the St. Erth Beds are known to be of Early Pliocene age, the other deposits, although unfossiliferous, must be of the same age. The topographic evidence furnished by the 400′ plateau, in conjunction with the petrographical characters of the deposits associated with it, has been used in reconstructing the Early Pliocene geography of Cornwall.

A complete study of one of the five Devonian regions (Kvamshesten) of western Norway has been made by Prof. C. F. Kolderup ("Kvamshesten Devonfelt," *Bergens Mus. Aarb.*, 1920–1, Naturvid. Raekke, No. 4, pp. 96). The rocks consist of a basement group of conglomerates and breccias, followed by a red and green sandstone series, a green sandstone, and finally a series of red conglomerates and sandstones. The lithological and palæontological similarity of this formation to the Orcadian Old Red Sandstone is convincingly demonstrated.

The sediments are shown to be of terrestrial and fluviatile origin. Prof. W. N. Benson's long paper on Palæozoic and Mesozoic seas in Australasia (*Trans. N.Z. Inst.*, **54**, 1922, 1–62) is really a very full account of the geological history and palæogeography of Australasia, and is illustrated by many informative maps. The tectonic relationships of Australasia and Antarctica are also discussed.

Sedimentary Rocks.—Dr. R. H. Rastall discusses a number of points arising from recent practice in sedimentary petrography, and from his own earlier unpublished work (*Geol. Mag.*, **60**, 1923, 32–9). He calls attention to the utility of large-scale panning of sediments previous to the use of heavy liquids. He is justifiably sceptical of the value of the elutriation method of separating grades in the case of mixed sediments. The value of dark-ground illumination in the identification of mineral particles is emphasised. The sources of some heavy minerals, notably kyanite and rutile, which are abundant in British sediments, are still unknown. Prof. P. G. H. Boswell further discusses some of the points raised by Dr. Rastall (*ibid.*, 165–8), and indicates the character and utility of the petrographic methods devised for the investigation of sediments of economic value.

Dr. R. Thiessen has published an admirable study of the microscopic structures of Palæozoic coals, in which a great amount of new information as to the composition and intimate structure of coal is given (*Bull.* 117, *U.S. Bureau of Mines*, 1920, pp. 296, 160 pls.).

The oil shales of the Mepale basin of Southern Burma are described by Prof. J. W. Gregory as probably Pliocene lake deposits, formed in a basin surrounded on most sides by Eozoic gneiss and schistose quartzite (*Geol. Mag.*, **60**, 1923, 152–9). The rich material consists of a clay base with a yellow or brown organic material which serves as the kerogen. No organic spherocrystals are present as in Scottish and Australian shales; but this may be due to the recent age of the deposits, too little time having been available for the crystallisation of the organic matter.

In a paper on dolomitisation in the Carboniferous Limestone of the Midlands, L. M. Parsons shows that there are two contrasting types of this change (*Geol. Mag.*, **59**, 1922, 51–64; 104–17). One is wholly subsequent in the main limestone mass of central Derbyshire; the other is almost wholly contemporaneous in the marginal deposits north of the Leicestershire Coal-field. In the first case magnesian ground waters appear to have been the agent of metasomatism. In the other case there are no lateral transitions to ordinary limestone, and selective metasomatism does not occur. The dolomitic

limestone here is believed to constitute a shallow-water facies formed during long-continued subsidence.

The massive chert formation in the Carboniferous of North Flintshire has been petrographically studied by H. C. Sargent (*Geol. Mag.*, **60**, 1923, 168–83). He believes that it is an original chemical sediment of inorganic origin, laid down as an off-shore deposit in a largely landlocked basin which was undergoing subsidence. The silica was probably derived from the disintegration of a granitic region lying to the north.

The work by C. L. Dake entitled "The Problem of the St. Peter Sandstone" (*Bull. School of Mines and Metall. Univ. of Missouri*, **6**, No. 1, 1921, pp. 228) is an exhaustive stratigraphical and petrological study of a sandstone formation. The extraordinary purity, uniformity, and degree of rounding of the grains in the St. Peter sandstone have been taken as indicative of its æolian origin. Prof. Dake, however, adversely criticises all these criteria, and argues that the formation is of marine origin. The importance of the work lies in the thorough discussion and criticism of those features of sandstones usually taken as indicating desert or æolian origin.

Vulcanism and Plutonism.—Prof. R. A. Daly has given a preliminary account of his volcanic studies on Ascension and St. Helena (*Geol. Mag.*, **59**, 1922, 146–56). "Ascension presents an ideal case of a complex volcanic island in its constructional stage." Widespread basalt flows form most of the island, but one-eighth is composed of younger domes of trachyte. St. Helena is essentially an older Ascension submaturely dissected by streams ancient and modern.

Discussing the recent volcanic activity at Lassen Peak, California, Dr. A. L. Day shows that there is evidence of powerful horizontal blasts of hot gases and sand, which have levelled long avenues of tree trunks (*Jour. Franklin Inst.*, **194**, 1922, 569–82). Many of the stumps have been completely rounded, and grains of volcanic sand have been driven an inch into the wood. The cause of these blasts is believed to be the release of gases during the eruption at the weakest point of the chamber below the sealing plug of the volcano.

G. Meyer discusses the origin of the low shield volcanoes of Iceland (*Neues Jahrb. f. Min.*, 1919, 51–68); and A. Bergeat describes the eruptions and collapse of crater walls in Vulcano (*ibid.*, 1920, 89–103).

Dr. H. S. Washington discusses the origin of plateau-basalts as exhibited in the enormous basalt floods of the Deccan, Columbia region of the United States, the Atlantic-Arctic region, and others, in a paper containing numerous new analyses of these petrographically neglected rocks (*Bull. Amer. Geol. Soc.*, **33**, 1922, 765–804). Chemically and mineralogically, plateau-

basalts are remarkably uniform. They are usually high in iron, especially ferrous iron; but relatively poor in magnesia and lime as compared with the basalts emitted from the central cone type of volcano. The high iron content is regarded as the cause of their great fluidity, since slags rich in iron are much more liquid than other types.

C. L. Baker has found that the great basalt region of the Parana basin, covering parts of South Brazil, Paraguay, Uruguay, and North Argentina, is probably the largest in the world (*Jour. Geol.*, **31**, 1923, 66–79). The area of basalt flows is at least 300,000 square miles, and their volume is estimated at 50,000 cubic miles. A further area of 75,000 square miles along the eastern margin of the field is believed to have been formerly covered with basalt. Hence the total area of basalt is 375,000 square miles. The sedimentary rocks beyond the basalt region are riddled by dolerite dykes and sills. The age of this enormous eruptive episode is most probably Jurassic.

W. A. Richardson critically reviews Prof. R. A. Daly's theory of the origin of granitic batholiths (*Geol. Mag.*, **60**, 1923, 121–8). He considers that the facts of isostasy are fatal to the view of the injection of abyssal wedges of basalt, which would increase the density of the crust under orogenic regions. Richardson then outlines a theory of batholithic origin by mechanical stoping which harmonises with isostatic conditions. Slight oscillation after the main upward movement over an orogenic belt would result in the intrusion of the postulated granitic shell, and would give all the characteristic features of batholiths.

From his studies of granite massifs in South Africa, Bavaria, and elsewhere, H. Cloos (*Neues Jahrb., Beil. Bd.*, **42**, 1918, 420–56; *Naturwiss.*, **11**, 1923, 7–10) concludes that these masses, while preserving all the characteristic features of batholiths, are not bottomless as generally assumed, but rest on a non-granitic foundation with a more or less horizontal junction plane.

In his work on contact phenomena between gneiss and limestone in western Massachusetts (*Jour. Geol.*, **30**, 1922, 265–94) P. Eskola shows that there has been considerable assimilation of limestone by igneous gneiss, with the production of lime-bearing silicates, especially clinopyroxene (diopside-hedenbergite) and titanite. It is also shown that the relative amount of magnesia to ferrous iron in the chief mafic minerals increases with the total quantity of those minerals. Thus these rocks exhibit a parallelism to the results of differentiation in an igneous series. This comparatively rare type of contact action seems to occur in regions where gneissic magmas have been intruded in connection with mountain folding.

C. E. Tilley has investigated the natural assemblages of minerals belonging to the system magnesia-alumina-silica under thermal metamorphism, and has compared them with the assemblages obtained in the pure dry melt system $MgO-Al_2O_3-SiO_2$ studied by Rankin and Merwin (*Geol. Mag.*, **60**, 1923, 101–7). The chief differences are due to the partial replacement of magnesia by ferrous iron in the natural rocks, and to the appearance of other phases such as plagioclase, orthoclase, biotite, etc. Enstatite appears in the rocks instead of clino-enstatite, and often andalusite in place of sillimanite.

Glaciation.—Prof. W. H. Hobbs has published further studies of the cycle of glaciation (*Jour. Geol.*, **29**, 1921, 370–86). He deals with the cycle of mountain glaciation in moderate latitudes, the transition between the mountain glacier and the ice cap, and the glacial cycle on the margins of the continental glacier of Antarctica.

As a result of a study of Spitsbergen glaciers, the writer (*Trans. Geol. Soc. Glasgow*, **17**, pt. 2, 1922, 1–49, 13 pls.) has been led to propound a new classification of glaciers based on the different development of glacier form, motion, accumulation, and wastage, in regions of high relief as compared with those of low relief. Other topics dealt with are the incidence of glaciation in Spitsbergen in relation to meteorology and topography, advance and recession of Spitsbergen glaciers, the formation of boulder clay, the earlier (Pleistocene) glaciation, and some erosional effects of past and present glaciation.

C. J. Gregory describes the parallel roads of Loch Tulla (*ibid.*, 91–104), of which there are five, the uppermost three forming a closely connected group. The explanation given follows that accepted for the better known and better marked case of Glen Roy. During the retreat of the ice the district about Loch Tulla was occupied by a lake held up by an ice barrier, which was lowered in two minor and at least three major stages, forming beaches during each pause.

PLANT PHYSIOLOGY. By Prof. Walter Stiles, M.A., Sc.D., University College, Reading (Plant Physiology Committee).

Carbon Assimilation.—Considerable advances in our knowledge of photosynthesis have been made since this subject was last reviewed in these pages three years ago, the most noteworthy contributions emanating from the Botany School in Cambridge and from the Institute of General Physiology in Strasbourg. In the former institution the researches on vegetable assimilation and respiration carried out under the general direction of F. F. Blackman have been continued by A. J. Wilmott and G. E. Briggs. Wilmott (" Assimilation by Submerged Plants

in Dilute Solutions of Bicarbonates and of Acids : an Improved Bubble-counting Technique," *Proc. Roy. Soc.*, B, **92**, 304–27, 1921) has described an apparatus by means of which the rate of assimilation of water-plants can be determined satisfactorily by counting the bubbles of gas given off from the immersed plant. This method, the " bubbling method," dates from the work of Sachs, but has always proved most unsatisfactory for quantitative work. By means of a specially constructed glass cap or " bubbler " Wilmott has succeeded in obtaining a continuous stream of bubbles of uniform size from the assimilating plant, and so has eliminated one of the greatest drawbacks of the bubbling method, namely, the lack of uniformity in the size of the bubbles. By the use of his improved technique Wilmott showed that the apparent increase in rate of assimilation observed by Treboux, when acid is added to the water in which a water-plant is immersed, is due to the liberation of carbon dioxide from calcium carbonate present on or in the surface of such plants growing in chalky water. Some data were obtained relative to the relation between carbon-dioxide concentration and rate of assimilation, and it was found that the relation conforms to that demanded by Blackman's Law of Limiting Factors.

Briggs has followed the development of photosynthetic activity of leaves during the growth of the seedling (" The Development of Photosynthetic Activity during Germination," *Proc. Roy. Soc.*, B, **91**, 249–68, 1920). He found that in the seedling leaves of *Phaseolus vulgaris*, *Vicia faba*, and *Avena sativa* the photosynthetic activity is zero or very small for some time after the appearance of the leaves, and then increases day after day. Even when further development of chlorophyll is inhibited, the photosynthetic activity continues to increase. The conclusion is therefore drawn that there is present in the assimilating cells some factor other than chlorophyll which is necessary for assimilation to proceed, and this factor develops with age of the leaf but lags behind the development of chlorophyll.

In a later communication (" The Development of Photosynthetic Activity during Germination of Different Types of Seeds," *Proc. Roy. Soc.*, B, **94**, 12–19, 1922) Briggs extends his observations to a number of other species. From the point of view of their germination the species considered fall into four groups. The first group contains plants in which the cotyledons are storage organs and subsequently become the first assimilating organs of the seedling ; to this group belong *Helianthus*, *Acer*, and *Cucurbita*. In this group of plants it is found that there is no such lag between the development of chlorophyll and photosynthetic activity as is shown by *Phaseolus* ; with greening

of the leaf its assimilatory apparatus appears to be complete. In the second group the cotyledons are storage organs, but do not turn green and assimilate; to this group belongs *Phaseolus*, the photosynthetic behaviour of the seedling of which has already been discussed. In a third group, which includes *Ricinus*, the food reserve is stored as endosperm while the cotyledons become the first assimilating organs. Plants of this group behave as *Phaseolus* in regard to the development of photosynthetic activity in the seedling. Finally, in a class which includes cereals such as maize and barley, the food reserve is stored as endosperm, but the first foliage leaf is the first assimilating organ. In this group also the development of assimilatory activity is similar to that in *Phaseolus*. There are thus two types of seedling in regard to development of assimilatory activity: the "*Helianthus* type" where the first assimilating organ is also a storage organ, in which the photosynthetic activity is fully developed at germination, and the "*Phaseolus* type" where the seedling develops a specialised photosynthetic organ different from the storage organ, and in which photosynthetic activity is not developed until some time after germination.

In a further contribution Briggs records the results of experiments on the effect of deficiency of mineral salts on the rate of assimilation ("The Characteristics of Subnormal Photosynthetic Activity Resulting from Deficiency of Nutrient Salts," *Proc. Roy. Soc.*, B, **94**, 20–35, 1922). Plants of *Phaseolus vulgaris* were grown in a series of water culture solutions devoid of one or other of the necessary nutrient salts. It was found that plants grown under such conditions always possessed a lower assimilatory activity than plants grown in a full nutrient solution but otherwise under the same external conditions, nor did it matter whether light or temperature was the limiting factor. A second point of interest is that the assimilatory activity of a plant grown in full nutrient solution was found to be less than that of a plant grown in soil, but otherwise under the same external conditions.

The fact that the same results are obtained whether light or temperature is limiting is particularly interesting, as it indicates that both the chemical and photochemical phases of the assimilatory process are depressed in an incomplete culture solution, as when light is the limiting factor the photochemical phase limits the rate of assimilation, and when temperature is the limiting factor the chemical phase limits the rate of assimilation. Still more interesting is the fact that both phases show the same amount of depression under the same conditions. This result can be explained if it be supposed that the seat of the photosynthetic process is the surface of the

chloroplast, a very credible supposition, or, rather, what Briggs calls the "reactive surface" of the chloroplast, which, bearing in mind that we are dealing with heterogeneous systems, need not necessarily be identical with the visible surface of the chloroplast. Any change in this reactive surface would result in a change in the rate of assimilation whether light or temperature were limiting, as it would result in change in the light-absorbing surface, and equally a change in the extent of the reactive surface over which the chemical action would proceed. This view is not only credible, but is probably the one generally held by plant physiologists.

Contradiction of the correctness of the theory of limiting factors comes from R. Harder ("Kritische Versuche zu Blackmans Theorie der 'begrenzenden Faktoren bei der Kohlensäureassimilation,'" *Jahrb. f. wiss. Bot.*, **60**, 531–71, 1921). The assimilation of *Fontinalis*, *Cinclodotus*, and *Cladophora* was measured using as a source of carbon dioxide potassium bicarbonate in the water containing these water-plants. Experiments were performed in which only the concentration of carbon dioxide was varied and in which only the intensity of illumination was varied. In either case it is stated that the rate of assimilation increases with increasing value of the factor according to a logarithmic relation. When two factors are varied, the rate of assimilation is stated to depend on the value of both factors, and not simply on the one present in the minimum, but that a change in the value of the factor relatively in the minimum produces a greater effect than an equivalent change in the value of the relatively stronger factor. These experiments should be repeated with other methods for measuring assimilation.

The important work of Wurmser deals mainly with the effect of light of different wave-lengths on photosynthetic activity. Thus the rate of bleaching of chlorophyll was compared quantitatively in different regions of the spectrum ("Action sur la chlorophylle des radiations de différentes longueurs d'onde," *Comp. rend. acad. sci.*, **170**, 1610–12, 1920) and the absorption of energy in the different regions measured with a thermopile. It was concluded that for equal absorption of energy, red and blue lights are equally active in bringing about bleaching of chlorophyll. With changing wave-length the photochemical sensitivity and coefficient of absorption change in the same direction.

In another communication ("L'action des radiations de différentes longueurs d'onde sur l'assimilation chlorophyllienne," *Comp. rend. acad. sci.*, **171**, 820–2, 1920) the rate of assimilation by green and red algæ in different-coloured lights was examined, the intensity of incident illumination being measured by its

rate of destruction of a chlorophyll solution, the amount of energy absorbed by the spectrophotometer, and the rate of assimilation by indicator changes in the sea-water containing the marine algæ used. It was found that in the green alga the secondary maximum of assimilation occurred in the blue, while for the red alga this secondary maximum is in the green. In relation to the quantity of energy absorbed, photosynthetic activity appears to be greatest in the green, where, of course, absorption is least. The explanation put forward of this is that where absorption is feeble the energy is distributed over a greater depth, and consequently active mass, of protoplasm.

An interesting observation on the photosynthesis of red algæ has been made by Wurmser and Duclaux (" Sur la photosynthèse chez les algues floridées," *Comp. rend. acad. sci.*, **171**, 1231–3, 1920). It was observed that red algal plants growing near the surface of the sea are often greener than those growing at lower depths. The greener forms were found to assimilate at only about one-third the rate of the redder forms, other conditions being the same. Estimation of the pigments, however, showed that the greener forms contain only about one-third of the chlorophyll contained in the fully red forms, the destruction of the red pigment in the stronger illumination being accompanied with a corresponding destruction of chlorophyll, although the amount of the yellow pigments is the same in both the surface and deeper growing forms. The reduced rate of assimilation in the former is thus due simply to the reduction in the quantity of chlorophyll.

Observations on photosynthesis in marine algæ have also been made by B. Moore, E. Whitley, and T. A. Webster (" Studies of Photosynthesis in Marine Algæ : (1) Fixation of Carbon and Nitrogen from Inorganic Sources in Sea-water; (2) Increase of Alkalinity of Sea-water as a Measure of Photosynthesis," *Proc. Roy. Soc.*, B, **92**, 51–60, 1921). These writers performed experiments with *Enteromorpha compressa* which they hold to indicate that in sunlight this alga assimilates more nitrogen than is contained as ammonia, nitrate or nitrite in the sea-water in which the alga is immersed. It is therefore concluded that free nitrogen as well as carbon dioxide is assimilated by this plant in sunlight.

R. Legendre (" Influence de la salinité de l'eau de mer sur l'assimilation chlorophyllienne des algues," *Comp. rend. soc. biol.*, **85**, 222–4, 1921) has also made measurements of the assimilation by some marine algæ, namely, *Ulva lactuca* and *Fucus serratus*. He comes to the conclusion that, with decreasing density of the sea-water, photosynthesis increases until a maximum is reached with a density of 1·010 ; with further decrease in the density of the sea-water photosynthesis decreases.

The significance of this result is not clear, but the author suggests that possibly the content of carbonate and bicarbonate may be of importance in determining this state of affairs.

A number of observations have been made during the last three years on the assimilation of plants growing under natural conditions. Thus F. T. McLean (" Field Studies of the Carbon-dioxide Absorption of Coconut Leaves," *Ann. of Bot.*, **34**, 367–89, 1920) measured the assimilation of carbon dioxide by leaves of the coconut in the field by determination of the amount of carbon dioxide absorbed. He found that the rate of absorption appears to be at a maximum at the beginning of the day, it then decreases during the morning, reaching a secondary minimum about midday, rises in the afternoon, and then decreases to zero in the evening. Detached leaves, on the other hand, exhibit a maximum absorption in the middle of the day, and low values in morning and evening. The author is of opinion that external conditions do not account for the decrease in assimilation in the middle of the day, and is inclined to attribute the result to a change in the internal conditions in the leaf. For the same area of leaf, middle-aged leaves absorb more carbon dioxide than either young or old leaves. Thus young leaves absorbed up to 25 mg. of carbon dioxide per hour, while middle-aged leaves absorbed up to 88 mg. and old leaves up to 28 mg. carbon dioxide per hour.

M. G. Stålfelt (" Ljuset i frukträdkronorna," *Pomologisk Årsskr.*, 125–36, 1920) measured the assimilation in sun and shade leaves in a number of fruit trees. He records a maximum rate of carbon-dioxide absorption in sun leaves of 500 to 600 mg. per sq. metre per hour, and in shade leaves of 300 to 400 mg. per sq. metre per hour, rates of assimilation considerably greater than those recorded by McLean for coconut, but about the same as those found by McLean for sugar-cane. In a later investigation Stålfelt continued his investigations on absorption of carbon dioxide by sun and shade leaves, using different intensities of illumination (" Till Kännedomen om Förhållandet mellan Solbladens och Skuggbladens Kolhydratsproduktion," *Medd. f. Statens Skogsförsöksanstalt*, Stockholm, 221–80, 1921). The chief result of these investigations is to show that with increasing illumination the rate of assimilation increases, so that the maximum rate of assimilation in the open air is only reached with full sunlight.

An investigation on somewhat similar lines has been made by H. Lundegårdh (" Ecological Studies in the Assimilation of Certain Forest Plants and Shore Plants," *Svensk bot. Tidskr.*, **15**, 46–95, 1921). When the carbon-dioxide concentration was that in normal air, it was found that with increasing light intensity the assimilation by shade plants increased more or

less in proportion to the intensity of the light up to an intensity of illumination of about one-tenth of full sunlight, above which intensity of illumination no further increase in illumination took place. In one case, however, that of a species of *Nasturtium*, a curve of more logarithmic form was obtained. When light was kept constant and carbon-dioxide supply varied, it was found that the rate of assimilation increased with increasing carbon-dioxide concentration up to a value determined by the intensity of illumination. In some cases there was found to be direct proportionality between carbon-dioxide concentration and rate of assimilation; in other cases the assimilation increased at a slower rate than the carbon-dioxide concentration. Among shade plants *Oxalis* was found to have the greatest maximum assimilating capacity, which accounts for its ability to flourish in very low intensities of illumination.

Boysen Jensen (" Studier over Stofproduktion i Skov," *Dansk Skovforen. Tidsk.*, Copenhagen, 306–36, 1921) concludes, as does Lundegårdh, that the maximum assimilation of plants growing under natural conditions is exhibited in light intensities well below that of full sunlight, the maximum assimilation of forest trees being reached in an intensity of illumination of 5·35 per cent. that of full daylight.

A number of recent contributions deal with the supposed actions in the leaf that make up the assimilatory process. The formaldehyde hypothesis, as usual, has received considerable attention. The fact that there is no production of any recognisable quantity of formaldehyde in the leaf has received further support from the work of Mazé (" Recherches sur l'assimilation du gaz carbonique par les plantes vertes," *Comp. rend. acad. sci.*, **171**, 1391–3, 1920) and H. Molisch (" Über die angebliche Entwicklung von Wasserstoffsuperoxyd bei der Kohlensäure-assimilation," *Biochem. Zeitsch.*, **125**, 257–61, 1921), neither of whom has been able to find a trace of formaldehyde production in green leaves. An elaboration of the formaldehyde hypothesis has, however, been put forward by E. C. C. Baly and I. M. Heilbron (*Jour. Chem. Soc.*, **119**, 1025, 1921; *Jour. Soc. Chem. Ind.*, **40**, 377–9, 1921, and elsewhere) based on *in vitro* experiments on the production of formaldehyde from an aqueous solution of carbon dioxide kept in a state of agitation in ultra-violet light or in ordinary light in presence of malachite green. The confidence with which Baly (" Photosynthesis," *Rep. British Ass. Hull Meeting*, 1922, 395) states that the photochemical conversion of carbonic acid into formaldehyde " forms the true key industry of all life " is unlikely to be shared by plant physiologists who are impressed with the vast difference between the conditions in

the assimilating cell and those in a test-tube. Another theory of assimilation has been put forward recently by Mazé (" Sur le mécanisme chimique de l'assimilation du gaz carbonique par les plantes vertes," *Comp rend. acad. sci.*, **172**, 173–5, 1921), who suggests that hydroxylamine plays a fundamental part in bringing about the reduction of carbon dioxide in green leaves, and reactions are suggested in which hydroxylamine takes part in the chemical or photochemical reduction of carbon dioxide with formation of glycollic aldehyde, which Mazé has found to be present in green leaves. Such speculations are, of course, only profitable in the present state of our knowledge, in so far as they lead to further experimental work.

ANTHROPOLOGY. By A. G. Thacker, A.R.C.S., Zoological Laboratory, Cambridge.

During recent years there have been several important additions to the anthropological periodicals published in England, and among these newer journals much favourable attention has been attracted by the *Annals of Archæology and Anthropology*, which are issued several times a year (not strictly quarterly) by the Liverpool Institute of Archæology (University of Liverpool). The *Annals* are edited by Mr. T. E. Peet, and have a tendency to specialise mainly on the strictly archæological side of anthropology, the papers dealing mainly with Neolithic or late prehistoric times, or even early historic times. Thus contributions appearing in vol. ix (1922) have treated of such subjects as the " Influence of Egypt on Hebrew Literature," and " Megalithic Architecture in the Western Mediterranean," and the " Oxford Excavations in Nubia." On this last subject there has been a series of contributions by Mr. F. L. Griffith, the last instalment being published in vol. ix, pts. 3 and 4 (December 1922). These articles are, of course, primarily a record of the research which was accomplished, and as such their first interest is for the professed archæologist, but that widening circle of readers who—without being professed archæologists—have some acquaintance with the antiquities and ancient history of North Africa will also find much that is instructive in this account of the investigations. Mr. Griffith's article published in December last deals chiefly with the results of researches at Napata, near Merawi, where remains of the Ethiopian-Libyan line of kings were found, dating from the eighth, seventh, and sixth centuries B.C. Not a great deal appears to be known of Napata in the earlier periods of Egyptian history, though Mr. Griffith states that the town was the " Ethiopian frontier-station " in the Eighteenth Dynasty. But it became prominent later. " Nothing more is known of Napata in history until it comes into pro-

minence in the eighth century B.C. as the seat of the Ethiopian dynasty of kings (Libyan in origin, according to Reisner), who when Egypt was divided against itself first dominated the Upper country, then ruled as Pharaohs in Thebes and Memphis for fifty years, and finally, excluded from the enlightened regions beyond the First Cataract, shrank back into barbarism." Mr. Griffith's account is illustrated by a splendid array of plates.

The *Proceedings of the Prehistoric Society of East Anglia* for 1921–2 (vol. iii, pt. 4) fully maintains the high level of interest reached by earlier numbers of this journal. Of the numerous contributions, perhaps the most significant is a brief article by Major E. R. Collins entitled, "The Discovery of an Early Palæolithic Implement in Yorkshire." It will be remembered that it has always been doubtful how far north in England the typical river-drift implements extend, and the opinion has been put forward that in Early Palæolithic times the climate of the North of England was too inhospitable to permit of occupation by man. This contention can no longer be supported. The implement described by Major Collins is a typical Chellean artefact composed of black chert; it was found in the Upper Nidderdale, just within the border of the West Riding. Other specimens, of less definite form, but almost certainly artefacts, were also found. Major Collins remarks: "The fact that the fauna connected with the Chellean Period has been found in Yorkshire has been apparently overlooked; the best examples of which were found in the famous Kirkdale Cave, and also in Victoria Cave, Settle, proving the climate of that period to be warm throughout England." Another article requiring special mention is by Hazzledene Warren and is entitled, "The Mesvinian Industry of Clacton-on-Sea, Essex." The Mesvinian implements (which, as the reader may remember, are very early Palæolithic) were found in the *Elephas antiquus* bed at Clacton, the associated fauna being of the warm type and including *Rhinoceros merckii.* The flora indicated a climate somewhat warmer than that of the present day. Mr. Warren throws out the suggestion that the Mesvinian industry was the precursor of the Mousterian, but was probably not connected with the Chellean or Acheulean.

The following papers may also be noted:

In the *Journal of the Royal Anthropological Institute* (vol. lii, July–December 1922): "L'œuvre anthropologique du Prince Albert 1er de Monaco et les récents progrès de la Paléontologie humaine en France" (being the Huxley Memorial Lecture for 1922), by Marcellin Boule; also "The Ethnology of Malta and Gozo," by L. H. D. Buxton; also "The Cult of the Dead in Eddystone of the Solomons," by A. M. Hocart. And in *Proc. Prehist. Soc. E. Anglia*, vol. iii, pt. 4: "Prehistoric Cooking

Places in Norfolk" (being the presidential address), by Miss Nina F. Layard; also "A Series of Ancient 'Floors' in a Small Valley near Ipswich," by J. Reid Moir. And in the *American Journal of Physical Anthropology*, vol. v, No. 3 (July–September 1922): "Climate and Race as Factors influencing the Weight of the Newborn," by Helen Brenton.

MEDICINE. By R. M. Wilson, M.B., Ch.B.

The Twort-D'Hérelle Phenomenon.—Great interest continues to be manifested in the discovery which is now known as the Twort-D'Hérelle Phenomenon. This phenomenon consists of the lysis of bacterial cultures, evidenced by clearing of a bouillon culture in which is growing another organism to which D'Hérelle has given the name "microbe bactériophage." In other words, it is assumed that organisms—of an ultra-microscopic type—exist which attack other organisms. The evidence on the point is now considerable. In 1915 Twort published a paper entitled "An Investigation on the Nature of Ultra-microscopic Viruses," in which he described how, in agar tubes incubated at 37° C. for 24 hours, some colonies of a micrococcus cultured from vaccine virus became so altered that subculture was impossible. If kept they grew glassy and transparent. This glassy material consisted only of granules. If, again, a little of it was added to a pure culture of the micrococcus, the growth at the point of contact also became transparent and the transparency spread over the whole culture. This glassy substance remained active when diluted one part in a million and passed through porcelain filters. On cultures which had been previously killed by heat the transparent substance had no effect. It was itself killed by a temperature of 60° C. for one hour.

D'Hérelle was unaware of Tworts' work when he published in 1917 his first paper on the subject. It was evident, however, that he was dealing with the same phenomenon. Since 1917 many other observers have found the phenomenon, but there is by no means general agreement as to its character. Thus it is stated in *Medical Science*:

"Twort was content to make several suggestions but settled upon none. D'Hérelle, from the commencement of his publications, has maintained that the active agent is a living filter-passing 'microbe bactériophage.' Bordet (1921) believes that it is an autolytic enzyme produced by the bacteria. Kabéshima (1920–2) looks upon the active agent as a catalyst that causes micro-organisms to produce autolytic ferments, these in their turn acting as catalysts to other micro-organisms."

The origin, then, of the "active substance" is unknown. It is, however, certain that it proceeds from animal cells. D'Hérelle obtained it from the fæces of patients with dysentery and from

the urine and fæces of a patient with typhoid fever. He also obtained it from the stools of healthy people and found it in the sea-water at Marseilles and Alexandria. He thinks it is normally present in the intestines of many animals. It has been obtained from manure, from samples of earth, from a sewer, and from the River Seine. The active principle has never been known to develop spontaneously in a culture. It increases in bouillon, but only when living bacterial cultures are also present.

Some organisms resist the lytic action. Thus, Gratia, working with a normal culture of *B. coli*, found that there were two forms of bacillus present—those resistant (" R ") and those sensitive (" S ") to the active substance. These forms were subcultured and maintained their special characteristics. " S " grew more quickly in an artificial medium and was non-motile; " R " grew more slowly and was very motile, was less easily dealt with by phagocytes and was more virulent. The question as to whether—granting that the active substance is a living organism—one type only or many different types of bacteriophage exist is also unanswered. D'Hérelle believes that there is only one type; but other workers disagree with him.

Meanwhile, Fleming and Allison have carried through some " Observations on a Bacteriolytic Substance (' Lysozyme ') found in Secretions and Tissues " (*British Journal of Experimental Pathology*, 1922, 3, 252). They find that, with some exceptions, all secretions and tissues of animals and some of vegetables contain a powerful bacteriolytic agent. The action of this substance is specially exerted on certain non-pathogenic bacteria, and " in all probability it is the cause of such bacteria being non-pathogenic." The substance is present in tears, nasal mucus and sputum, and in cartilage. It is present in large quantity in egg white. It differs, however, from the bacteriophage of D'Hérelle in that it cannot be transmitted in series.

The Recurrence of Influenza.—The appearance of influenza at the beginning of March, after a complete absence during the whole winter, furnishes a new and striking confirmation of the suggestion of Brownlee that this infection shows a 33-week periodicity. The epidemic of 1921–2 began about the middle of November, but did not reach its height till the second week of January. The present epidemic was therefore " due " towards the end of February (66 weeks later). The death-rate for influenza in London rose, in point of fact, in the last week of February, reaching 56 per week some weeks later. During January it had not exceeded 10 or 12. The epidemic was mild, as might have been anticipated in view of its occurrence so late in the season. A number of questions, however, force

themselves on the mind: If this periodicity is correct, what are the circumstances which determine it? These cannot be meteorological nor yet entomological, since a 33-week period corresponds to no known weather or insect cycle. Again, is it possible that influenza possesses a cycle of its own which is carried out in the blood-stream? This would account for the fact that warm-weather recurrences are free from the symptoms we usually speak of as "influenza"—coryza, bronchitis, pneumonia—these being superimposed conditions which necessitate cold weather for their development. The subject is one of great interest and importance and merits intensive study.

ARTICLES

INDETERMINATE EQUATIONS OF THE THIRD DEGREE

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A GENERAL account of the more important results, indicating briefly the salient ideas involved in their proofs, and their presentation in such a way, it is hoped, as to be intelligible to those interested in Mathematics generally, will be given in this essay.[1]

§ 1. We shall deal primarily with equations in two variables, and shall consider equations in three variables only in so far as their study has been useful or necessary for our purpose. The numerous equations with which we are concerned can be very conveniently grouped into three classes leading to three types of problems which we can refer to as the A, B, C types and introduce by taking very simple questions in each group.

One, which is of the type B, is to find all the integer solutions of the equation

$$y^2 = x^3 + 17. \tag{1}$$

A number of these, including some which are obvious by inspection, are given by the table

x	-2	-1	2	4	8	43	52	5,234
y	3	4	5	9	23	282	375	378,661

and *suggest* that there are an infinite number of integer solutions.

The next, which is of the type A, is to find all the rational solutions, no longer restricting ourselves to integer values of the unknowns, *e.g.*,

$$x = \frac{19}{25},\ y = \frac{522}{125};\ x = \frac{137}{64},\ y = \frac{2,651}{512},$$

the method of finding them apparently indicating that an infinite number of such solutions exist. It is much easier to

[1] The substance of this paper formed a lecture delivered before the Manchester Mathematical Society in October 1922. Ample references will be found in vol. ii of Dickson's *History of the Theory of Numbers*.

find rational solutions than integer ones, though not apparently so in our particular example.

The third type C is to find all the integer values of x, y, z, where x is prime to y, satisfying

$$z^2 = x^3 + y^3 \tag{2}$$

e.g.

x	2	8	11	65	71
y	1	-7	37	56	-23
z	3	13	228	671	588

It is obvious that any number of *rational* solutions are given by

$$x = \frac{p}{p^3 + q^3},\ y = \frac{q}{p^3 + q^3},\ z = \frac{1}{p^3 + q^3}$$

where p and q are any rational numbers.

§ 2. The most general equation of the type A is to find all the rational solutions of the equation[1]

$$F(x, y) = 0 \tag{3}$$

where F represents a non-degenerate cubic curve, or using homogeneous co-ordinates, to find all the integer solutions of

$$f(x, y, z) = 0 \tag{4}$$

where f represents a homogeneous ternary cubic in x, y, z, so that the distinction beteeen rational and integer solutions is of no importance. Included in this type are the rational solutions of the equations

$$y^2 = 4x^3 - g_2x - g_3 \tag{5}$$

$$ey^2 = ax^3 + bx^2 + cx + d \tag{5a}$$

$$m = ax^3 + bx^2y + cxy^2 + dy^3 \tag{6}$$

and the integral solutions of

$$ax^3 + by^3 + cz^3 + dxyz = 0 \tag{7}$$

including as particular cases

$$ax^3 + by^3 + cz^3 = 0 \tag{8}$$

$$x^3 + y^3 - az^3 = 0 \tag{9}$$

It may be noted that the solution of equation (4) is really equivalent to the question of finding the rational solutions of the equation

$$y^2 = ax^4 + bx^3 + cx^2 + dx + e \tag{10}$$

or in a homogeneous form to find all the integral solutions of

$$z^2 = ax^4 + bx^3y + cx^2y^2 + dxy^3 + ey^4 \tag{11}$$

[1] There is no loss of generality in supposing that all the coefficients of the various equations in this paper are integers.

The most important results are that if the equation (4) is unicursal, *i.e.* the cubic curve given by (4) has a double point, all the integer solutions can be found by expressing x, y, z as rational functions of a parameter.[1] If, however, it is of genus unity,[2] as is the case in general, all the solutions can be expressed rationally in terms of a finite number by a classic process, but we still require a theoretical limit to the magnitudes of the unknowns in the fundamental solutions.

The (B) question is to find all the integer solutions of the equation (3). It is most likely there are only a finite number, and this has been proved for equations (5a) and (6).

The last question (C) is to find all the integer solutions with x prime to y of the equations

$$z^2 = ax^3 + bx^2y + cxy^2 + dy^3 \qquad (12)$$

$$z^3 = ax^2 + bxy + cy^2 \qquad (13)$$

The complete solution of (12) is given by

$$x = f_1(p, q), y = f_2(p, q)$$

where f_1 and f_2 are a finite number of binary quartics in p, q; while for (13) the same result holds if f_1, f_2 are cubics.

§ 3. The present topic, apart from its own great intrinsic interest, is of considerable importance in the History of Mathematics. Various aspects of it have suggested themselves for several thousand years both to the mathematician and to the tyro; not only because of the simplicity of enunciation, but also because it appeals to the natural curiosity of persons who have anything at all to do with numbers. Indeed it seems almost impossible that there could have been a mathematical renaissance in the seventeenth century without a display of great interest in such questions to which, even in recent times, many persons owe their fondness for mathematics.

It is, however, very characteristic of the Theory of Numbers that these questions, which suggest themselves so easily, should prove very difficult. Their intractability may be judged from the fact that it was only several hundred years after the seventeenth century that the principles necessary for progress were discovered, and even then these were not applied till recent years.

In the meantime more communications, mostly unimportant, have been published upon Diophantine Analysis than upon perhaps any other branch of mathematics. In fact, their

[1] This was proved by Hilbert and Hurwitz in 1890, and will not be referred to again.

[2] That is, the curve (4) has no double point.

number has been so great as to embarrass seriously those engaged in compiling a history of the subject.

The rational solution of equation (10) had been proposed as a prize problem in 1865 by the Academy at Rome; and mathematicians had frequently emphasised the lack of any general results. In 1900, at the International Mathematical Congress at Paris, Hilbert noted that the solution of the general Diophantine Equation was still to be found.

Our subject gives rise to several thoughts. It illustrates a great change in mathematical fashions, as it was perhaps as much a part of the Algebras of the eighteenth and early nineteenth centuries, as graphs are at present. But progress was stopped at an early age, and the many epoch-making discoveries of the nineteenth century were sufficient to direct the interest and energy of many mathematicians to more fertile ground.

It also shows the danger of mathematical prophecy, as only two years ago I wrote that " the prospect of any immediate solution of such a problem (A) appears almost as remote now as that of discovering any knowledge concerning the chemical constitution of the stars must have appeared in 1800. Finally, it is very surprising that a step which now seems so simple and natural should have proved so very difficult to find, and that its discovery should be due to one of those fortunate accidents which are only too rare in the history of mathematics.

§ 4. The real history of the subject dates from Bachet and Fermat in the early part of the seventeenth century. Before that time, only isolated results had been discussed, and very few general statements had been either enunciated or proved. One of the oldest results was stated by Diophantus (third century A.D.) without proof; namely, that any number which is the difference of two cubes can be expressed as a sum of two cubes. Then some time before A.D. 972, the Arab Alkhodjandi gave a defective proof that the sum of two cubes could not be a cube. The classical problem of the duplication of the cube suggests, however, an older history for questions involving cubes. Finally, an anonymous Arab MS. of a date before A.D. 972 contains the problem, given k to find x so that $x^2 \pm k$ are both squares. This may have been derived from the Hindus, who were acquainted with Diophantine Analysis from an early age. Some attention seems to have been paid to questions of this kind which were discussed by the Arab Mohammed Ben Alhocain of the tenth century, and Alkarkhi during the beginning of the eleventh century. About 1220 Leonardo Pisano proposed the problem to find a fraction x so that $x^2 \pm 5$ are both perfect squares, and also gave an incomplete proof that $x^2 \pm y^2$ were not simultaneously perfect squares for rational values of x, y unless $y = 0$.

In 1591 Vieta took up the theorem by Diophantus, and stated that if

$$x^3 + y^3 = b^3 - d^3$$

we could take

$$x = b\left(\frac{b^3 - 2d^3}{b^3 + d^3}\right), \; y = d\left(\frac{2b^3 - d^3}{b^3 + d^3}\right)$$

and gave some other results of this kind. Bachet in his edition of Diophantus in 1621 gave also these results by Vieta. If, however, $b^3 < 2d^3$, the value of x is negative, and it was left to Girard and Fermat to indicate how positive values were to be found for both x and y. It was again Bachet in 1621 who noted that from the solution $y = 5$, $x = 3$ of

$$y^2 + 2 = x^3$$

we could obtain other rational solutions.

He put

$$y = 5 - \mathrm{N}, \; x = 3 - \frac{10\mathrm{N}}{27}$$

so that the new equation

$$27 - 10\mathrm{N} + \mathrm{N}^2 = \left(3 - \frac{10\mathrm{N}}{27}\right)^3$$

after removing the factor N^2, is linear in N giving a rational value. He carried out a similar process for the equation

$$y^2 = x^3 + 17.$$

This process, of course, can be repeated with the new values of x and y, so that apparently an infinite number of rational solutions can be found. The geometrical interpretation is very simple. The point P(3,5) can be called a rational point [1] on the cubic

$$y^2 + 2 = x^3.$$

The tangent at P will meet the cubic in another point Q, obviously a rational one. Similarly with the tangent at Q. This method may be called the tangent method.

This brings us to the work of Fermat, the father of the Theory of Numbers, whose theorems, given without proof, proved a very powerful stimulus to his successors. They can be arranged under three divisions, each of which will be considered in turn. He stated that he could prove rigorously that the only integer values of x and y satisfying Bachet's equation

$$y^2 + 2 = x^3$$

were

$$x = 3, y = \pm 5;$$

[1] *I.e.* its co-ordinates are rational.

and that for the equation

$$y^2 + 4 = x^3$$

the only integer values were

$$x = 2, y = \pm 2 ; x = 5, y = \pm 11.$$

In fact, he had proposed the first question as a problem to the English mathematicians. Concerning this he says, "Peut-on trouver en nombres entiers un carré autre que 25, qui, augmenté de 2, fasse un cube? A la première vue cela paraît d'une recherche difficile, en fractions une infinité de nombres se déduisent de la méthode de Bachet ; mais la doctrine des nombres entiers, qui est assurément très belle et très subtile, n'a été cultivée ni par Bachet, ni par aucun autre dont les écrits venus jusqu'à moi." Fermat did not publish his method, which is not known, but it is not unlikely that it was equivalent to the following due to Euler. In

$$y^2 + 2 = x^3$$

put

$$x = a^2 + 2b^2$$

and take

$$x + \sqrt{-2} = (a + b\sqrt{-2})^3.$$

By equating imaginary parts

$$1 = 3a^2b - 2b^3 = b(3a^2 - 2b^2),$$

whence $b = 1, a = \pm 1$, so that $x = 3, y = 5$.

This process can be explained by elementary algebra without imaginaries, but it is of course by no means obvious that all the integer solutions can be found in this way. Now only a few years ago, I proved the rather unexpected result that any equation of the form

$$y^2 + k = x^3, k \neq 0$$

had at most a finite number of integer solutions. It is most unlikely that this result was known to Fermat, so that I do not think that his description of his method, "très belle et très subtile," would hold at present.

§ 5. Fermat's next contribution was what is now known as Fermat's Last Theorem, namely, that if n is a positive integer greater than 2, the equation

$$x^n + y^n = z^n$$

has no integer solutions unless one of the unknowns is zero. He stated that he had discovered a truly wonderful proof, which, however, was never published. No one has yet succeeded in finding a general proof. We are at present concerned with the particular cases

$$x^3 + y^3 = z^3 \text{ and } x^4 + y^4 = z^4$$

especially the last one, which we shall take in the form

$$x^4 + y^4 = z^2.$$

Fermat, himself, left a proof for a closely related theorem. The method is now classical and is referred to as the method of infinite descent. As it involves only elementary ideas, and is peculiar to the Theory of Numbers, where it has been applied with great success, we shall outline the proof as applied to

$$x^4 + y^4 = z^2.$$

Firstly we may suppose x and y are prime to each other, and form a non-zero solution, *i.e.* neither x nor y equals zero; and that x is even, y is odd, as they cannot both be odd. Hence we find

$$x^2 = a^2 - b^2, \; y = 2ab, \; z^2 = a^2 + b^2$$

where b is even, a is odd and prime to b. From the first of this group of equations we have

$$x = p^2 - q^2, \; b = 2pq, \; a = p^2 + q^2$$

where p and q are prime to each other. Hence

$$y^2 = 4pq(p^2 + q^2)$$

so that we have

$$p = x_1^2, \; q = y_1^2, \; p^2 + q^2 = z_1^2$$

or

$$x_1^4 + y_1^4 = z_1^2.$$

Also

$$x = x_1^4 - y_1^4, \quad y = 2x_1y_1z_1,$$

$$z = x_1^8 + 6x_1^4y_1^4 + y_1^8$$

while

$$z > z_1^4.$$

Hence from any non-zero solution x, y, z of the equation we can deduce another non-zero solution $x_1y_1z_1$ with $z_1 < \sqrt[4]{z}$. It is clear that this process can be continued with $x_1y_1z_1$, so that we are led to an infinite sequence of steadily decreasing positive integers

$$z, z_1, z_2 \ldots z_n$$

where ultimately $z_n = 1$ corresponding to a zero solution.

The proof above is one which involves only the mathematics of Fermat's time. Euler in 1753 announced that he had found a proof for the equation $x^3 + y^3 = z^3$, which, however, was incomplete in respect of an assumption, wherein lay the real difficulty of the question, and which was not properly understood until the nineteenth century.

The intractability of all of our equations must have been obvious at an early stage to anyone interested in the subject.

But as in most problems a single solution was generally obvious by inspection, the question naturally arose as to whether this solution could be utilised to find others. Fermat gave a number of results, *e.g.* in the equation

$$y^2 = ax^4 + bx^3 + cx^2 + dx + e, \qquad (10)$$

he showed how to find solutions when either a or e was a perfect square, both of these cases being really included in the more general case where he showed how to find other solutions when one was known. His method was in principle identical with that of Bachet's. Fermat took for example the quartic

$$y^2 = ax^4 + bx^3 + cx^2 + dx + e^2$$

of which we know one rational solution

$$x = 0, y = \pm e.$$

He then put

$$y = px^2 + \frac{dx}{2e} + e$$

where p is given by

$$2pe + \frac{d^2}{4e^2} = c.$$

Eliminating y and dividing out by x^3, a linear equation is found giving a rational value for x.

§ 6. Important extensions of Fermat's results and methods were given by the illustrious Euler, who tells us of his great pleasure in such questions. For example, he found new rational solutions of

$$y^2 + 4 = x^3$$

knowing the rational points (2, 2), (5, 11), by a process which is in effect equivalent to finding the point of intersection, obviously a rational point, with the cubic of the chord joining these points, a method which we refer to as the secant method. He also utilised a quadri-quadratic relation between two variables, for finding new solutions, a method very closely related to the modern results obtained by elliptic functions. The method of infinite descent was repeatedly employed by Euler. More novel were his results on questions B and C. To solve

$$ax^2 + by^2 = z^3$$

in integers, he put

$$z = pa^2 + qb^2$$

and took

$$x\sqrt{a} + y\sqrt{-b} = (p\sqrt{a} + q\sqrt{-b})^3$$

whence he found

$$x = ap^3 - 3bpq^2, y = 3ap^2q - bq^3.$$

This method, however, need not give all the integer values of x, y, so that the results of his applications to equations of this type when y is given, say unity, are not conclusive; and his proofs of Fermat's statements are defective in respect of a very important principle which will be considered later.

The idea above, however, was utilised for other equations by Krafft and Lagrange. Thus to solve in relatively prime integers

$$x^3 + ny^3 = z^2$$

take

$$x + y\sqrt[3]{n} = (p + q\sqrt[3]{n} + r\sqrt[3]{n^2})^2.$$

whence

$$x = p^2 + 2nqr, \; y = 2pq + nr^2, \; o = 2pr + q^2$$

The last equation is satisfied by taking

$$p = 2a^2, \;\; r = -b^2, \;\; q = 2ab$$

giving a solution for xyz in two parameters a, b. Lagrange applied the same method to the more general equation

$$x^3 + ax^2y + bxy^2 + cy^3 = z^2$$

as also did Legendre. It does not give all the solutions which were not found till 1911.

§ 7. In the nineteenth century, considerable interest was shown in the problem, given several solutions of the equation (4), $f(x, y, z) = o$ to deduce new solutions. Various results of this kind were given by Cauchy in 1826, Sylvester in 1847, Lucas in 1878, and Desboves amongst others. Thus from the solution x_1, y_1, z_1, of

$$ax^3 + by^3 + cz^3 + dxyz = o \tag{7}$$

Cauchy derived the new one

$$\overline{x_1(by_1^3 - cz_1^3)} \; \overline{y_1(cz_1^3 - ax_1^3)} \; \overline{z_1(ax_1^3 - by_1^3)}$$

a result repeatedly given when $d = o$, by many writers during the period. Most of these results depended upon the tangent and secant methods, which were, however, first stated explicitly in geometrical form by Lucas, and which we state here. Suppose we know n rational points $P_1(x_1, y_1, z_1)$, P_2, . . . P_n (*i.e.* points with x, y, z rational) on the cubic $f(x, y, z) = o$, then in general an infinite number of rational points can be found as follows. Take any two of these points (they may be the same), then the secant through them meets the curve in a rational point, say P_{n+1}. Repeating the process with the $n + 1$ points $P_1 P_2$ . . . P_{n+1} and continuing it indefinitely, we find in general an infinite number of rational solutions. The analytical inter-

pretation is equally simple, as the co-ordinate of any point on the curve can be expressed in terms of elliptic functions, the idea being due to Jacobi in 1834. Take for simplicity the equation in the canonical form

$$y^2 = 4x^3 - g_2x - g_3 \qquad (5)$$

and write in the usual rotation

$$x = p(u), y = p'(u).$$

Then corresponding to the n points $P_1, P_2 \ldots P_n$ we have n parameters $u_1, u_2, \ldots u_n$ given by $x_1 = p(u_1), y_1 = p'(u_1)$, etc.

The addition formula at once shows that an infinite number (in general) of rational solutions are given by

$$x = p(m_1u_1 + m_2u_2 \ldots + m_nu_n)$$
$$y = p'(m_1u_1 + \ldots\ldots\ldots m_nu_n)$$

where $m_1, m_2 \ldots m_n$ are any integers, positive, negative, or zero. During the present year I showed that all the rational solutions could be found in this way from a finite number.

Results on equation (7) were given in 1847 by Sylvester, who established a correspondence between its solutions and those of

$$x^3 + y^3 + abcz^3 + dxyz = 0.$$

He then showed that if $a = b = 1$, c is a prime and $27c + d^3$ is positive and not divisible by any prime of the form $6n + 1$, all the solutions could be found from a single one. He also gave some results such as, the equation

$$2(x^3 + y^3 + z^3) = (27n - 12)\, xyz$$

has no integer solutions (with none of the unknowns zero) if $27n^2 - 8n + 4$ is a prime.

Equally interesting were his results on

$$x^3 + y^3 = Az^3.$$

Denoting by p and q primes of the forms $18n + 5$, $18n + 11$, respectively, he proposed in 1869 the problem to show that there were no integral solutions when

$$A = p, q^2, 2p, 4q, 2p^2, 2q^2.$$

This result, as also some new cases $A = q, p^2$, etc., were proved by Pepin in 1870. They were also proved in 1878 by Lucas, while Sylvester extended his results in the same year to the cases $A = pq, p^2q^2, pp_1^2, qq_1^2$, where p, p_1 are primes of the form $18n + 5$, and q, q_1 are primes of the form $18n + 11$.

A very elegant method of proof was given in 1917 by Hurwitz, who showed that the equation ($z \neq 0$)

$$x^3 + y^3 + eaz^3 = 0$$

had no solutions where x, y were complex integers of the type $\xi + \eta\rho$ where ξ, η are ordinary integers, ρ is a complex root of unity, ϵ is an unknown complex unit, *i.e.* ± 1, $\pm \rho$, $\pm \rho^2$ and $a > 2$ is a prime $\equiv 2$ or $5 \pmod 9$. The first step in the proof is to show that the congruence

$$x^3 \equiv \epsilon \pmod p$$

where x is a complex integer and p is a prime $3k + 2$ has a solution only when

$$\epsilon^{\frac{1}{3}(p^2-1)} = \epsilon^{(k+1)(3k+1)} \equiv 1 \pmod p$$

so that if k is not divisible by 3, *i.e.* if $p \equiv 2$ or $5 \pmod 9$ only when $\epsilon = \pm 1$.

Consider now an integral complex solution of the equation for which $N(xyz)$ (*i.e.* the product of xyz by its conjugate) has its least value, obviously positive and greater than o.

Put $\xi = x + y$, $\eta = \rho x + \rho^2 y$, $\zeta = \rho^2 x + \rho y$ so that

$$\xi + \eta + \zeta = o$$

$$\xi \eta \zeta + \epsilon a z^3 = o.$$

Writing now δ for the greatest common factor of ξ, η, ζ, we have

$$\frac{\xi}{\delta} \cdot \frac{\eta}{\delta} \cdot \frac{\zeta}{\delta} = -\epsilon a \left(\frac{z}{\delta}\right)^3.$$

No two of the factors on the left-hand side can have a common factor as it would occur in the third factor since the sum of all three is zero. Hence since a is still a prime in the complex theory

$$\frac{\xi}{\delta} = \epsilon_1 x_1^{\,3},\ \frac{\eta}{\delta} = \epsilon_2 y_1^{\,3},\ \frac{\zeta}{\delta} = \epsilon_3 a z_1^{\,3},\ \frac{z}{\delta} = x_1 y_1 z_1$$

whence as

$$\xi + \eta + \zeta = o$$

$$x_1^{\,3} + \eta_1 y_1^{\,3} + \eta a z_1^{\,3} = o$$

where η_1, η_2 are units of which the first by the lemma must be ± 1. Hence we have a new solution (x_1, y_1, z_1, η) of the original equation with

$$N(x_1 y_1 z_1) = N\left(\frac{z}{\delta}\right).$$

But this by the choice of $x_1\, y_1\, z_1$ must satisfy

$$N\left(\frac{z}{\delta}\right) \geq N(xyz) \text{ or } 1 \geq N(xy\delta)$$

which means that $x_1 y_1 \delta$ are units. Hence $x^3 = \pm 1$, $y^3 = \pm 1$, and as $a > 2$, $z = o$ contrary to our assumption, so that the theorem is proved.

4

It is very interesting to note that practically the same method had been used in 1808 or 1809 by Gauss for the case $a = 1$, as was discovered after his death from his notebooks.

§ 8. After Euler there were no important contributions to the study of the integer solutions of equations of the form

$$y^2 + k = x^3$$

until 1869, when a new method was discovered by Lebesgue for proving that some equations of this type had no integer solutions. Take the equation

$$y^2 - 7 = x^3$$

for example. It is clear that x cannot be even, for no number of the form $8M + 7$ can be a square. Neither can x be the form $4n + 3$, for then y would be divisible by 2 but not by 4, so that x must be of the form $4n + 1$. But since

$$y^2 + 1 = (x + 2)(x^2 - 2x + 4)$$

$x + 2$, a number of the form $4n + 3$, is a divisor of $y^2 + 1$ which is known to be impossible. The method was extended by Jonquières, who gave formulæ for k for which the equation had no integer solutions, *e.g.* $-k = (4d + 2)^3 - 1$. More general results of this type were given in 1881 and afterwards by Pepin, who had in 1875 proved Fermat's results and similar ones by justifying Euler's methods.

§ 9. The work of the twentieth century was inaugurated by a brilliant paper by Thué, who showed in 1909 that if $f(x, y)$ is a homogeneous binary quantic which is not the power of a linear form or of an indefinite binary quadratic, then the equation

$$f(x, y) = c \qquad (14)$$

has at most a finite number of integer solutions. The particular cases of this, coming within our province, are the equations

$$ax^3 + bx^2y + cxy^2 + dy^3 = e \qquad (15)$$

$$ax^4 + bx^3y + cx^2y^2 + dxy^3 + ey^4 = f \qquad (16)$$

Thué's proof is very long and complicated, but it depends only upon *elementary algebra*, so that it is quite as remarkable as Tchebychef's proof of the theorem, that one prime at least lies in the interval x, $2x - 2$ if $x > 3$. The important steps in the proof, denoting by ρ a root of an irreducible equation of degree r, are:

(1) Establishing an identity in x of the form

$$(\rho - x)^n[f_1(x)\rho^{r-1} + f_2(x)\rho^{r-2} \ldots] = \rho Q(x) - P(x)$$

where $f_1(x)$, etc. . . ., $P(x)$, $Q(x)$ are polynomials in x with integer coefficients, f_1, f_2 . . . are of degree m, depending upon

the arbitrary integer n, and the coefficients of $f_1, f_2 \ldots$ P, Q are numerically less than K^n, where K is independent of n.

(2) Given integers p and q such that

$$q > 0, |q\rho - p| < 1$$

he showed by repeatedly differentiating the identity above for x, and putting $x = p/q$, that two pairs of integers A, B, and A_1, B_1, with $AB_1 - A_1B \neq 0$ could be found, each satisfying an inequality of the type

$$|\rho B - A| < |(\rho q - p)^{\lambda} q^{\mu} C|^{n-1}$$

or $$= \phi K^{n-1}, \text{ say, where } \phi < 1,$$

and $$|B| < |Dq^{\nu}|^{n-1}$$

where C, $D > 0$ are independent of n, p, q, and λ, μ, ν depend on the choice of m.

(3) If the equation (14) has an infinity of solutions one of which can be referred to as p_0, q_0, while a typical one is $p_1 q_1$, then we can take $q_1 > q_0 > 0$ and

$$\rho q_0 - p_0 = \epsilon_0/q_0^{h}, \ \rho q_1 - p_1 = \epsilon_1/q_1^{h}$$

with

$$0 < |\epsilon_0|, |\epsilon_1| < 1 \text{ and } h > \frac{r}{2}$$

since by factorising the equation for ρ, $\rho q_1 - p_1 = M/q_1^{r-1}$, where M is finite when q_1 is large.

We now select $A/B \neq p_1/q_1$ to satisfy the inequality in 2. Hence

$$p_1 B - q_1 A = -B\epsilon_1/q_1^{h} + \phi K^{n-1} q_1,$$

and it is then shown that as $q_1 \rightarrow \infty$ each of the fractions on the right-hand side is numerically less than $\frac{1}{2}$. As the left-hand side is a non-zero integer the contradiction proves the theorem.

Thué's result has been extended by Maillett, and more especially by Siegel, who has also just recently given a much simpler proof.

§ 10. I now come to my own work. My first paper dealt with the integer solutions of

$$y^2 = x^3 + k$$

in three different ways. The first method was an extension of Lebesgue's and Pepin's results, but the other two were to lead to important consequences. One of them can be explained by considering another problem. Suppose it were required to find integer values of p and r, prime to each other, satisfying the equation $p^2 + r^2 = a$ where a is given. If such values exist, we can find two integers q, s, satisfying

$$ps - qr = 1.$$

Writing then

$$x = p\xi + q\eta, \; y = r\xi + s\eta \qquad (17)$$

the binary quadratic form $x^2 + y^2$ is transformed into the form $a\xi^2 + 2b\xi\eta + c\eta^2$ say. It is well known that the determinant $b^2 - ac$ is an invariant of this form, so that $b^2 - ac = -1$; *i.e.* $c = (b^2 + 1)/a$, and this suggests the reverse process. Suppose a is given and find b so that $b^2 + 1$ is divisible by a, and call c the quotient. We get in this way an infinite number of forms

$$a\xi^2 + 2b\xi\eta + c\eta^2$$

with different b's and c's, but with the same determinant (here -1). It can be proved that all these forms can be derived from a finite number (in this case the single one $x^2 + y^2$) by a linear transformation; and the theory of numbers gives a method for finding them and the corresponding substitution, and hence all the values of p, r.

A cubic form

$$a\xi^3 + 3b\xi^2\eta + 3c\xi\eta^2 + d\eta^3$$

has one invariant, namely its discriminant, and a quartic form

$$a\xi^4 + 4b\xi^3\eta + 6e\xi^2\eta^2 + 4d\xi\eta^3 + e\eta^4$$

has *two* invariants usually denoted by g_2, g_3 or I, J. Suppose we try to find all the quartic forms with given invariants g_2, g_3 and first coefficient a. I found that while for the quadratic form, b was such that $(b^2 + 1)/a$ was an integer; in the present case, we were led to an equation of the form

$$z^2 = Ax^3 + Bx^2y + Cxy^2 + Dy^3 \qquad (12)$$

and that corresponding to any of the required quartics, we had a solution of the above with x prime to y. Conversely corresponding to every such solution we could find a quartic. But the general theory tells us that all these quartic forms can be derived from a finite number by means of a linear transformation, and we are led to the result that all the integer solutions with x prime to y of (12) are given by a finite number of expression of the form

$$x = f_1(p, q), \; y = f_2(p, q)$$

where f_1 and f_2 are binary quartics in (p, q), which are easily found in any case, at least theoretically.

So by considering the cubic form, we find the corresponding solution of

$$z^2 = Ax^3 + Bxy + Cy^3 \qquad (13)$$

given by

$$x = g_1(p, q), \; y = g_2(p; q)$$

where g_1, g_2 refer to a finite number of binary cubics. In particular Thué's theorem for (15) and (16) shows at once that there are only a finite number of solutions of (12) or (13) with $y = 1$, and the proof for equation (12) I have recently given in a much simpler way.

Before I pass on to my other method, I must note another of my results, namely, that all the rational solutions of

$$z^2 = ax^4 + 4bx^3y + 6cx^2y^2 + 4dxy^3 + ey^4 \qquad (11)$$

could be found, if we knew one solution and all the rational solutions of

$$t^2 = 4s^3 - g_2s - g_3 \qquad (5)$$

A simple proof follows by taking the quartic in the form

$$y^2 = x^4 + 6cx^2 + 4dx + e$$

and putting

$$2s = x^2 + c + y.$$

By eliminating y and solving the resulting quadratic in x, we have

$$2x(s + c) = -d + t$$

where

$$t^2 = 4s^3 - g_2s - g_3 \qquad (5)$$

Also s and t are both rational if x and y are, and conversely. A similar result applies to the ternary cubic $f(x, y, z) = 0$, *i.e.* all the rational solutions can be found if we know one, and all the solutions of (5) where now g_2, g_3 are multiples of the invariants of the ternary cubic.

My other method employed ideal numbers and had already been used by Kummer for Fermat's Last Theorem. Writing the equation

$$y^2 - k = x^3$$

in the form

$$(y + \sqrt{k})\,(y - \sqrt{k}) = x^3$$

the question arises, what can be said of $y \pm \sqrt{k}$? Are they, for example, the cubes of expressions of the form $a \pm b\sqrt{k}$, as was thought to be the case by some of the older mathematicians, *e.g.* Euler? The simple example

$$(2 + \sqrt{-5})\,(2 - \sqrt{-5}) = 3^2$$

where $2 \pm \sqrt{-5}$ are neither squares of expressions of the form $a + b\sqrt{-5}$ with a and b integers, nor have any factors of that form, shows that this is not necessarily the case. The theory of ideals enables us, however, to say that we can deduce a *finite* number of equations of the form

$$c(y + \sqrt{k}) = (a + b\sqrt{k})\,(p + q\sqrt{k})^3$$

where a, b, c are known integers and p, q, y are unknown integers. By equating rational terms we see that c must be represented by a binary cubic in p, q. Hence by Thué's theorem there can be only a finite number of solutions, as was proved independently both by Thué and Landau, the latter writer having already considered a special case in 1913.

I next tried to prove the corresponding theorem that

$$y^2 = ax^4 + bx^3 + cx^2 + dx + e \qquad (10)$$

had only a finite number of integer solutions. As the previous method was not applicable, I first of all found another proof for the cubic case

$$y^2 = ax^3 + bx^2 + cx + d.$$

The right-hand side was factorised in the form

$$y^2 = (x - \theta)\,(ax^2 + mx + n)$$

where θ is a root of the cubic

$$a\theta^3 + b\theta^2 + c\theta + d = o$$

while m, n naturally depend upon θ. The theory of ideals enables us to deduce that

$$(x - \theta)\,(x_0 - \theta) = (p\theta^2 + q\theta + r)^2 \qquad (18)$$

where x_0 is one of a finite number of integers, and x, p, q, r are unknown integers. By equating coefficients of $\theta, \theta^2 \ldots$ etc., we have two equations of the form

$$f_1(p, q, r) = o,\ f_2(p, q, r) = m$$

where f_1, f_2 are homogeneous ternary quadratics in p, q, r. It is a classical result that the solution of the first is given by a finite number of expressions of the form

$$p = \phi_1(\xi, \eta),\ q = \phi_2(\xi, \eta),\ r = \phi_3(\xi, \eta)$$

where ϕ, etc., are binary quadratics in ξ, η (integers). If these are substituted in the second equation, Thué's theorem shows that only a finite number of integer values for ξ, η could be found, and hence also for x.

When I applied this method to the quartic (10), I found that

$$(x - \theta)\,(x_0 - \theta) = (p\theta^3 + q\theta^2 + r\theta + s)^2 \qquad (19)$$

Equating coefficients of $\theta, \theta^2 \ldots$ etc., we have two equations in p, q, r, s, from which one of the unknowns can be eliminated. But I could not prove that there were only a finite number of solutions, as I thought I had at one time proved. In trying, however, to remedy the flaw in my proof, a matter upon which

I spent some months in vain, I was led to the following far more important theorem. Suppose we had started from

$$z^2 = ax^4 + bx^3y + cx^2y^2 + dxy^3 + by^4 \qquad (11)$$

where x, y are unknown integers prime to each other. Instead of equation (19), we now have a finite number of equations of the form

$$(x - \theta y)(x_0 - \theta y_0) = (p\theta^3 + q\theta^2 + r\theta + s)^2$$

where x_0, y_0 are known. The process of elimination mentioned above revealed to me that x, y could be expressed rationally in terms of X, Y another solution of (11), where X, Y are practically linear functions of p, q, r, s. Further, as p, q, r, s are of the order of magnitude given by the maximum numerical value of $\sqrt{x}$, $\sqrt{y}$, this is tantamount to saying that the method of infinite descent applies; in other words, all the solutions of (11) can be expressed rationally in terms of a finite number.[1] It was then very easy to apply this result to the ternary cubic

$$f(x, y, z) = 0$$

and to show that all the solutions could be deduced from a finite number by the classical process. There still remains the problem of discovering these fundamental solutions. They could of course be found by trial, if we could find a theoretical limit to the magnitudes of the unknowns.

Finally, I may remark that the theory of ideals seems to promise useful information about the number of integer solutions of the equation (15); and only very recently Daus has announced that there is at most one integer solution of the equation $y^2 = ax^3 + 1$ where a is a given integer.

[1] See my paper in the *Proceedings of the Cambridge Philosophical Society*, vol. xxi, p. 179, 1922 (issued November 1922).

"CATALYSIS"

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CATALYTIC reactions are those whose speed is modified by the presence of a small proportion of a substance which appears to take no part in the reaction, and which can be shown to possess the same chemical composition at the end of the reaction as at the beginning.

The existence of such reactions has been known for over a century. Kirchoff, in 1811, observed that mineral acids promote the conversion of starch into sugar without being themselves changed, and the oxidation of alcohol to acetic acid by the oxygen of the air in contact with finely divided platinum was noted as far back as 1820 by Edmund Davy. Earlier still, Mrs. Fulhame, before the close of the eighteenth century, had noticed that chemical change was greatly affected by the presence of moisture, an acute observation that has waited for a full development till recent years, when the researches of H. B. Baker have revealed that many familiar reactions do not take place in the complete absence of moisture. Dry hydrogen and oxygen unite with difficulty. Dry ammonia does not combine with dry hydrogen chloride, and dry ammonium chloride does not dissociate on vaporisation. In fact, there is probably no substance whose catalytic activities are so widely distributed as water.

Though isolated instances of catalysis have thus been long recognised, Benzelius (*Traité de Chimie*, 1845) was the first to group such phenomena together, and to bestow on them the name "catalytic"—a term which, in itself, conveys little impression to the mind, for it means nothing more than "destructive."

Ostwald has happily compared the action of a catalyst to that of oil on a machine, or of a whip on a sluggish horse, and has expressed his belief that the catalytic body cannot start a reaction, but can only assist one already in progress. It is obviously very difficult to be sure that a reaction which does not appear to take place in the absence of a suitable catalyst is not, in fact, proceeding with an infinitely small velocity, but

the "onus" of proof rests on those who support Ostwald's theory. In any case, the subtle difference seems one for "the cumini sectores" rather than for the working chemist, who is, as a rule, content to believe that "catalytic agents can start, accelerate, or retard the speed of, chemical reactions."

The classification of catalytic reactions is a matter of extreme difficulty. A division into those of Organic Chemistry and those of Inorganic Chemistry is often convenient, but is essentially superficial. It seems undoubtedly more scientific to classify the reactions according to the way in which the catalyst does its work. Here, however, we are arrested by the fact that in the case of many such changes we do not know with any degree of certainty the mechanism of the reaction. A brief consideration of the principal theories advanced to explain the mysterious activity of catalysts will illustrate the difficulty of the problems involved.

In the manufacture of sulphuric acid by the "chamber process" there can be little doubt that the catalytic nitric oxide facilitates the production of sulphuric acid from the mixture of sulphur dioxide, air, and steam by the formation of an intermediate compound, perhaps simply nitrogen peroxide, which again parts with oxygen to the sulphur dioxide, perhaps the nitrosulphuric acid (NO_2HSO_3) which can be isolated in the form of "chamber crystals" when the supply of steam is deficient. Again, in the case of the production of ether from alcohol, sulphuric acid acts as a catalyst through the intermediate compound, ethyl hydrogen sulphate.

$$C_2H_5 \cdot OH + H_2SO_4 = H_2O + C_2H_5 \cdot HSO_4$$
$$C_2H_5 \cdot HSO_4 + C_2H_5 \cdot OH = (C_2H_5)_2 \cdot O + H_2SO_4$$

These and similar reactions encouraged what we may call the "intermediate compound theory," among the foremost living exponents of which is Paul Sabatier. The distinguished French chemist, in his book, *La Catalyse en Chimie Organique*, expresses the belief that all catalytic reactions are essentially similar and are purely chemical phenomena, resulting from the temporary formation of unstable compounds involving the catalyst and one or more of the reacting bodies.

The number of cases where the existence of such an intermediate compound can be proved is fairly large, extending from those where its transitory presence can just be detected to such an extreme case as Weldon and Péchiney's process for the manufacture of chlorine,

$$\begin{cases} (a)\ 2MgCl_2 + O_2 = 2MgO + 2Cl_2. \\ (b)\ MgO + 2HCl = MgCl_2 + H_2O \end{cases}$$

which is really a catalytic reaction slowed down so much that it takes place in two distinct stages. Magnesium chloride, when heated in a current of air, is oxidised to MgO with liberation of chlorine. The MgO is then converted again into $MgCl_2$ by the action of hydrochloric acid, and the process recommences. The intermediate compound is here so easily isolated that the mechanism of the reaction is obvious, but we may imagine that if magnesium oxide were a very unstable body, a continuous oxidation of hydrochloric acid with production of chlorine might take place in presence of an apparently inert and mysteriously efficient magnesium chloride.

It is evident, indeed, that if the rate of destruction of the intermediate compound is greater than its rate of formation, attempts to isolate it are likely to fail, and this is no doubt often the case. Nevertheless, the extension of the theory from reactions such as those considered above to *all* catalytic reactions is of doubtful validity. We may take as an example the use of nickel or platinum to promote the hydrogenation of organic compounds. Sabatier explains these reactions by supposing the formation of an unstable hydride of the metal which liberates its hydrogen in a very active (possibly atomic) condition. Vavon and Husson (1922) have carried out an investigation which casts great doubt on the existence of these hypothetical hydrides. It is a well-known fact that the presence of a small proportion of carbon disulphide will prevent platinum black from exercising its power as a catalyst of hydrogenation. These investigators have found that the quantity necessary to "poison" the platinum varies with the body to be hydrogenated. The addition of ·0004 gm. of CS_2 to ·2 gm. of platinum black is sufficient to render it useless for the hydrogenation of acetophenone, but cinnamic acid, nitrobenzene, and cyclohexene are still reduced. When ·0005 gm. has been added, the virtue of the platinum is gone as far as cinnamic acid is concerned; but ·0008 gm. is required in the case of nitrobenzene and ·0011 gm. for cyclohexene before the catalyst is incapacitated. The authors conclude that these figures cannot represent different hydrides of platinum, and that the phenomenon has a physical explanation.

The action of metallic oxides (such as ThO_2, Al_2O_3) as catalysts of dehydration may be due (as Sabatier believes) to the formation of unstable thorinates, aluminates, etc.; but it seems unwise without further proof to accept unreservedly this explanation, and it is almost incredible that such catalyses as the destruction of phosphine and arsine on glass surfaces are due to the formation of intermediate compounds.

The conclusion most widely accepted at present is that,

whilst many catalytic reactions have undoubtedly a chemical explanation, there are certain cases in which such a solution seems improbable, particularly many of those in which gases react on the surface of a solid catalyst. We must therefore consider briefly the " physical " theories employed to explain the action of catalysts.

The phenomena of adsorption and absorption of gases by metals are well known, and Michael Faraday put forward the theory that the oxidation of hydrogen in presence of platinum was due to the formation of a film of adsorbed gases on the surface of the catalyst. Under such circumstances the number of collisions between molecules would obviously be greatly augmented, and chance of combination so increased. Langmuir has recently investigated the catalytic combustion of carbon monoxide and hydrogen on platinum surfaces, and has demonstrated that the reaction kinetics of these systems can be explained on the assumption that the reactions proceed in a unimolecular film on the surface of the catalyst.

One of Rideal's latest experiments is of interest in this connection. He re-examined the hydrogenation of ethylene to ethane in presence of nickel, and found that, in the case of a mixture rich in hydrogen, the reaction velocity is proportional to the partial pressure of ethylene.

This he explains by supposing that the surface of the nickel becomes almost covered with adsorbed hydrogen molecules and that the reaction takes place whenever an ethylene molecule happens to strike the surface. The number of such collisions would evidently be proportional to the partial pressure of ethylene. An explanation, however, based on the "temporary metal-hydride theory," seems to be also possible.

The promotion of different reactions by different catalysts acting on the same system is often advanced as an argument in favour of a chemical explanation of these reactions. Such effects are sometimes very marked. For example, at 300°, and under normal pressure, the vapours of a primary alcohol are split up by reduced copper into the aldehyde and hydrogen, while aluminium oxide or thorium oxide brings about a decomposition into water and a hydrocarbon of the ethylene series. Again, formic acid in contact with zinc oxide is broken up into carbon dioxide and hydrogen, while titanium oxide gives a mixture of carbon monoxide and water.

While it is true that some such cases may be due to the formation of true chemical compounds with the catalyst, the results are not incapable of a physical explanation. The power of a catalyst to promote a certain reaction may depend on its adsorption of the components or products of that reaction

and the adsorptive powers of bodies vary greatly with the substances adsorbed. It may be then that catalysts tend to produce the system which they adsorb most strongly. Even if we accept this suggestion, our " explanation " is only partial, for we must still inquire why a catalyst adsorbs one substance rather than another. This question we are still far from being able to answer.

The action of " promoters " may also be explained on a physical basis. It has been found that the addition of a trace of another body (the " promoter ") sometimes improves very much the powers of a catalyst. We may take as an example from a number given by Rideal and Taylor, in *Catalysis in Theory and Practice*, a catalyst used in the manufacture of hydrogen from water-gas and steam, in accordance with the equation, $CO + H_2O = CO_2 + H_2$. The catalyst is prepared by dissolving a mixture of 194 parts of iron nitrate, 5 parts of ammonium bichromate, and 1 part of thorium nitrate, evaporating and igniting. It is possible that the main body of the catalyst (Fe_2O_3) adsorbs the reacting gases in a proportion unsuitable for combination on a large scale, and that this is remedied by the different adsorptive powers of the " promoter," a little of which is sufficient to adjust the balance.

One must not disregard the possibility that some reactions are brought about, not by " adsorption," but by " absorption," for it is well known that absorbed gases are generally much more chemically active than the same gases under ordinary conditions. Hydrogenised palladium will reduce an acidified solution of ferric chloride, though ordinary hydrogen (molecular) will not, and carbon monoxide will unite with chlorine in the cold in the pores of animal charcoal to produce carbonyl chloride. Recent work (1922) by Anderson has shown that this increased activity lasts even after the gas has been desorbed. He performed a series of experiments on the reducing powers of hydrogen freshly desorbed from platinum and palladium, and demonstrated that it was capable of reducing copper oxide or of combining with sulphur at a temperature about 30° lower than ordinary hydrogen. The hydrogen was shown to be slightly ionised, but not sufficiently to account for the results obtained, in the opinion of the experimenter, who finds himself unable to offer any satisfactory explanation.

An " electrochemical " theory of catalytic action has also been proposed which partly bridges the gap between the " chemical " and the " physical " theories. In many cases there appears to exist at adsorptive boundaries an electrical double layer, caused either by the electrical orientation of a layer of neutral

molecules or, in the case of ionised substances, by selective adsorption of one of the ions of the molecule. (The presence of ions in many cases of combustion and other chemical reactions has been demonstrated by Sir J. J. Thomson and others.) It is obvious that in some cases a definite orientation of the reacting molecules in the surface film would be a great aid to combination.

Another "electrical" theory is due to H. E. Armstrong, who suggests that an electrolytic circuit is formed by the aid of the catalyst. For example, in the oxidation of hydrogen there would be a circuit, H_2/water/O_2, and it is noteworthy that the purest water obtainable is not an active catalytic agent, perhaps because its electrical conductivity is so low.

Very early in the history of catalysis it was discovered that catalysts rapidly lose their activity in the presence of certain substances by which they are said to be "poisoned." Nickel is rapidly poisoned for most purposes by sulphur and the halogens, and it is incapable of hydrogenating benzene which contains even a trace of thiophene. Platinum is poisoned by a large number of substances, among the most virulent being hydrogen cyanide, iodine, corrosive sublimate, carbon disulphide and carbon monoxide. In the "contact process" for sulphuric acid the gases from the pyrites burners must be freed from arsenical compounds and other impurities which speedily inactivate the platinum catalyst. This "poisoning" is sometimes regarded as a special case of "negative catalysis."

In our preliminary definition of catalysis we spoke of catalytic reactions as those "whose speed is modified" by the presence of the catalyst. This modification of speed generally takes the form of an increase, and the catalysis is considered to be "positive." When, on the other hand, the speed is diminished we have a case of "negative catalysis."

Negative catalysis can often be made to serve the chemist, especially by the stabilisation of substances which tend to decompose, polymerise, or be oxidised at the ordinary temperature. The decomposition of hydrogen peroxide is greatly retarded by the addition of a little hydrochloric acid, and the very dangerous oxidation of chloroform to the poisonous phosgene gas is prevented by the presence of a trace of alcohol. Recently (1922) Mouren and Dufraisse have observed that the spontaneous oxidation of acrolein, benzaldehyde, and many other organic substances can be prevented by the addition of a small proportion of hydroquinone. Many other phenolic bodies have a similar action.

Negative catalysis has been less studied than positive catalysis. It is believed that many cases of poisoning are due to

formation of a stable and catalytically inactive compound on the surface of the catalyst, in which case the action of the poison is not, correctly speaking, catalytic at all, but more work must be done before anything like a general explanation of the phenomenon can be offered.

The chemical composition of a catalyst must, by definition, be the same at the end of a reaction as at the beginning, but this is not necessarily true of the physical state. In fact, magnified photographs of a platinum wire which has been used as a catalyst show that the surface, originally smooth, has become roughened, and is, in fact, covered with a fine deposit of platinum black (*v.* Rideal and Taylor's *Catalysis in Theory and Practice*). This may be due to deposition of platinum from a temporarily formed compound of the metal with one of the reacting substances, but it is not impossible that reactions between gases in the surface layer of the platinum might break it up sufficiently to produce the same effect.

The applications of catalysis to chemical industry are very numerous. The most that can be done in a short article is to touch on a few of the large-scale operations that seek aid from catalysis.

The two principal processes for the manufacture of sulphuric acid have been already mentioned. Both depend essentially on the reaction $2SO_2 + O_2 = 2SO_3$, but in the "chamber process" the facilitation of this change depends on the formation of one or more intermediate compounds involving the catalytic nitric oxide, whereas in the "contact process" the reaction takes place on the surface of platinised asbestos at 400° C. and may have a physical explanation. The latter process was first patented in England in 1831, but it was not until 1901 that the practical difficulties were sufficiently overcome to make it a commercial success. The entering gases must be very rigorously cleansed of dust, sulphur, arsenic, and other impurities or the platinum is soon poisoned.

The fixation of atmospheric nitrogen constitutes one of the most important problems before the chemist of to-day. One means of solving it is by making the nitrogen unite with hydrogen to form ammonia, a reaction which takes place under high pressure in the presence of a suitable catalyst. Several such were suggested by Haber, who found that uranium carbide, which changed into uranium nitride in the course of the reaction without losing its efficiency, gave excellent results. The process was taken up on a large scale by the Badische Anilin und Sodafabrik, who gave up the attempt to employ as catalysts the more recherché elements in the periodic table and came back to iron, used at 600° C. under 200 atmospheres pressure. Recently the French chemist Claude, having ob-

served the great services rendered to Germany during the war by this colossal factory, which turned out, in 1918, 100,000 tons of ammonia, and realising the importance of rendering France independent in this respect, perfected a similar process which employs a pressure of 1,000 atmospheres at 500°–600° C. By this great increase of pressure the practical yield of ammonia for a single passage of the gas over the catalyst is increased from 6 per cent. to 25 per cent.

Natural oils and fats are composed, for the most part, of esters of glycerine with organic acids, and in some cases they contain a high percentage of glycerides of unsaturated acids. These are not only acids of the acrylic series, such as oleic acid, but many which are unsaturated in a much higher degree. For example, the unpleasant smell of fish oils is largely due to clupanodonic acid, whose highly unsaturated nature can be seen at once from its formula $C_{17}H_{27}\cdot COOH$. The melting points of these unsaturated acids and their esters are generally much lower than those of the corresponding saturated acids (oleic acid, $C_{17}H_{33}\cdot COOH$, melts at 14° C., whereas stearic acid, $C_{17}H_{35}.COOH$, melts at 71° C.), and they almost all possess disagreeable odours. When such oils are hydrogenated in presence of catalysts, the hydrogen fixes itself on the double or triple bonds, with the result that an evil-smelling oil is converted into an almost odourless fatty solid (the melting-point of cod-liver oil is —°10C, that of the hydrogenated product 68° C.). An idea of the industrial importance of this branch of chemistry is given by the fact that more than 200 patents have been taken out for the use of various catalysts and various forms of apparatus. Nickel is the most favoured, but palladium is also used, its high price being compensated for by the fact that 1 part of the metal is sufficient to bring about the reduction of 10,000 parts of oil. Oils generally contain a proportion of free fatty acids which attack the metallic catalyst employed, and many also contain sulphur compounds which rapidly deprive it of its catalytic activity. The acids may be neutralised with chalk, and the sulphur compounds fixed by stirring the heated oil with freshly precipitated copper hydroxide. The catalyst can then work undisturbed.

When hydrocarbons are subjected to a high temperature they tend to lose hydrogen with, it seems probable, a temporary setting free of radicals, such as CH_3, CH_2, CH, etc. These groups unite to form new molecules, and the result is the production of a considerable quantity of hydrogen, methane, and low-order hydrocarbons, both gaseous and liquid. Owing to the great demand for gases for illuminating purposes, for petrols to be used as fuel for internal combustion engines and as solvents, and for "paraffin oil," the production of these

hydrocarbon mixtures from oils of higher boiling-point has become an important branch of chemical industry. This "cracking" process, as it is called, can be carried out at a much lower temperature by the aid of catalysts which have been tried in very great variety, both the finely divided metals and the anhydrous metallic oxides such as those of aluminium and zinc giving good results. Reduced copper appears to be superior for this purpose to either iron or nickel, and a litre of American petrol containing only compounds of boiling point higher than 150° C. gave, by a single passage over a layer of copper at 600° C., 120 litres of gas of high calorific power and 225 c.c. of hydrocarbons boiling at temperatures lower than 150° C. The copper becomes gradually inactivated by the deposition of carbon, but this can be removed by heating the exhausted catalyst in a current of steam, when water-gas is produced and the catalyst is regenerated. The liquid obtained by these operations generally contains a certain proportion of unsaturated hydrocarbons of disagreeable odour. These may be hydrogenated in contact with reduced nickel with production of a petrol suitable for use in internal combustion engines.

The production from water-gas of illuminating gas possessing a high calorific power is another interesting and important application of catalysis to industry. Water-gas consists mainly of hydrogen, carbon monoxide, and carbon dioxide, the proportions of the latter gases depending on the temperature at which the water-gas is prepared. Carbon monoxide is completely reduced by hydrogenation over nickel at 250°, giving a mixture of methane and water, and a similar reduction can be practised on carbon dioxide at 300°–350°. It is obvious that a gas rich in methane can be thus prepared. We may take as example the method proposed by Sabatier in 1912, which utilises water-gas prepared at as high a temperature as possible, and consisting almost entirely of carbon monoxide and hydrogen in approximately equal proportions. The water-gas is cooled to a temperature such that about three-quarters of the carbon monoxide is condensed, together with traces of sulphur dioxide and other impurities which would poison the catalyst. The resulting mixture of $CO+4H_2$ is passed over nickel at 200°–250°, when the reaction $CO+3H_2 = CH_4 + H_2O$ takes place, and the issuing gas has the composition $CH_4 + H_2$ and possesses a calorific power approximately twice that of the original coal-gas. The carbon monoxide which was removed by condensation can be used as fuel to heat the reaction tubes.

Not alone do the gigantic operations of industrial chemistry profit by catalytic aid. Many a smaller process is thus rendered commercially profitable. The manufacture of phenyl

ether, which imparts its pleasant odour to many toilet soaps, will serve as example. The dehydration of phenol in contact with thorium oxide at 400°–500° to give phenyl ether was originally realised at the University of Toulouse. Fifty tons of this compound are now being made every year in the same town under conditions which are essentially similar to those under which it was produced in the laboratory.

While emphasising the paramount importance of this branch of chemistry in the commercial world, we must not forget that it also facilitates innumerable reactions which, though not industrially important, are of first-rate scientific interest. Space forbids any attempt to enumerate these at length, but we cannot pass over in silence what has been described by one of its authors as "sans contredit le plus important des travaux que le nickel réduit permet d'accomplir." The hydrogenation of the benzene nucleus was for many years considered one of the most difficult problems of organic chemistry. Cyclohexane itself, resulting from the addition of six hydrogen atoms to benzene, had to be extracted tediously from Russian petrol or prepared by complicated syntheses. In 1901 Sabatier and Senderens showed that reduced nickel could accomplish the hydrogenation of benzene at a low temperature (170°–190° C.) with a theoretical yield, and were thus responsible for one of the most striking triumphs of catalysis.

The catalytic power of enzymes can receive no such attention as it deserves in the present article. They fulfil the ordinary tests for catalysts in a marked degree, for an exceedingly minute quantity of the enzyme can promote to an unlimited extent the reaction which it catalyses, without itself apparently being changed. The familiar operations of nature depend to a very great degree on the catalytic action of these peculiar bodies. The souring of milk, the making of cheese, the production of alcohol and vinegar, the breaking down of protein matter to ammonia, nitrites and nitrates, the hydrolysis of starches and fats during digestion, are only a few of the processes made possible by these organic catalysts. But the study of their activities is a branch of science in itself and they are only mentioned here as a reminder that catalysis is not exclusively a laboratory phenomenon.

On the practical side, catalysis opens to the research worker a field vast in extent and fertile in possibilities. For the theorist its unsolved problems are numerous and fascinating. Although the amount of work done in recent years has been enormous (Sabatier and his co-workers alone have published about one hundred communications dealing with catalytic reactions), much still remains to be done, and we are likely to wait long before the last word on the subject is pronounced.

SOME CHEMISTS OF ISLAM

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There is no more romantic figure in the history of chemistry than that of the great Geber. So much that is legendary, however, has accumulated around him that if we are to get a clear picture of the man himself we must have recourse to the original sources. In the eighth century A.D. there lived at the Court of the Caliph Hārūn al-Rashīd at Baghdad a man whose name was Abū Mūsā Jābir ibn Ḥaiyān. He is said to have been a native of Khurasan, and was on very friendly terms with the great Barmecide family, the story of whose relations with Hārūn al-Rashīd is well known. The Barmecides were themselves deeply interested in chemistry, and in his *Book of Properties* (British Museum MS.) Jābir refers several times to conversations on chemical subjects which he had held with them. Thus he says: " One day—it was the ninth of Ramadhān—Ja'far ibn Yaḥyā summoned me, and we went, together with Yaḥyā and [Al-Fadhl], Ja'far's brother, to buy some slave-girls. When we had finished that, we fell to talking about the properties of different substances," and after the Barmecides had described several experiments they had performed on the effect of plunging heated metals into oil, they said, " O Abū Mūsā, knewest thou all that? Now relate unto us that which thou thyself hast performed thereon."

As was usual in those days, Jābir was a physician as well as a chemist, and he gives a graphic description of a cure he made by means of an elixir he had discovered: " Yaḥyā ibn Khālid had a very valuable handmaiden, unequalled in beauty and perfection and deportment and intelligence and accomplishments. One day she fell ill and drank a certain medicine which, however, failed to cure her, and she rapidly grew worse and finally became delirious. A messenger came to Yaḥyā with the news and he asked me what I advised. I had not seen her and thought she might be poisoned, so I recommended the application of cold water. This treatment was of no avail, so I ordered them to poultice her abdomen with heated salt and to chafe her feet. As she still grew worse,

Yaḥyā at last asked me to go and see her, and I found her at the point of death from some obscure disease. Now I had some of the elixir with me, so I gave her a draught of two grains of it in three ounces of oxymel, and, by Allah, the sickness departed from the damsel and in less than half an hour she was as well as ever. And Yaḥyā fell at my feet and kissed them, but I said, ' Do not so, O my brother.' And he asked me about the uses of the elixir, and I gave him the remainder of it and explained how it was employed, whereupon he applied himself to the study of science and persevered until he knew many things ; but his son Ja'far was cleverer than he."

Jābir was more than philosopher and scientist—he was a mystic as well, and one of the earliest adherents of Ṣūfī-ism. Unfortunately, he occasionally allowed his mysticism to mingle with his science, or perhaps it would be fairer to say that he often used scientific metaphors in his mystical writings, with the result that certain European scholars, mistaking these writings for genuine scientific works, very naturally concluded that Jābir could not possibly have written the books which are known to us in a Latin form and attributed to " Geber." Luckily, however, many other of Jābir's books have been preserved to us and among them are several which prove their author to have been a man of keen intellect and of the true scientific temper. One may go even further, and say that, although so far no Arabic originals of the Latin treatises have come to light, yet many passages of the latter are distinctly " Jābirian," and that the burden of proof lies on those who deny that Geber and Jābir are identical.

In the Arabic works Jābir describes the reduction of metals, the preparation of many compounds such as white lead, cinnabar, and verdigris, the suphur-mercury theory of metals, the similarity between arsenic (sulphide) and sulphur, and many other chemical facts and theories. He also describes methods of dyeing and of waterproofing cloths and mentions the use of manganese dioxide in glass-making. It would be impossible to maintain that the content of scientific knowledge of the Arabic works is less than that of the Latin.

Aidamir al-Jildakī, in his book *Nihāyat aṭ-Ṭalab,* refers to the troubles which beset many of the early chemists, and says : " Jābir ibn Ḥaiyan himself on many occasions narrowly escaped death, and suffered much violence and oppression at the hands of envious and evilly disposed people, on account of his scientific knowledge. He finally had to reveal some of his secrets to Hārūn al-Rashīd and to Yaḥyā the Barmecide and his two sons, Al-Fadhl and Ja'far, hence their riches. When Al-Rashīd became suspicious of the Barmecides and

knew that their aim was to transfer the kingdom to the 'Alids—their great wealth rendering them quite able to do this—he killed them to the last man, and Jābir fled for his life to Kūfā, where he remained in concealment until the days of Al-Ma'mūn, when he came out of retirement."

Jābir is usually said to have died A.H. 160 (A.D. 766–7), but, if Al-Jildakī is correct, Jābir's death must be placed much later, since Al-Ma'mūn did not succeed to the Caliphate until A.H. 198.

Jābir's fame spread throughout the whole of Islam, and there is scarcely a single later chemist who does not quote him. He is mentioned by the apostate Leo Africanus (died A.D. 1552), who says: " In this citie of Fez there are great store of Alchymists which are mightily addicted to that vain practice: they are most base fellowes, and contaminate themselues with the steam of Sulphur, and other stinking Smels. In the evening they vse to assemble themselves at the great temple, where they dispute of their false opinions. They have of their arte of Alchymie many bookes written by learned men, amongst which one *Geber* is of principall account, who lived an hundred yeeres after Mahumet. . . . This *Geber* his works and all his precepts are full of allegories or darke borrowed speeches."

When in Kūfā, Jābir lived in the street known as the Damascus Gate, where he had a laboratory. It is related in the *Kitāb al-Fihrist* (tenth century A.D.) that when some houses in this quarter were demolished, a mortar of gold, weighing about 200 pounds, was discovered on the site of Jābir's laboratory. The king's chamberlain took possession of it.

About a hundred years after Jābir lived Abū Bakr Muḥammad ibn Zakariyya al-Rāzī, known to Europe as Rhazes. He was primarily a physician, but studied chemistry in addition, and wrote several books on the subject. Most of these have been lost, but one, entitled *The Book of Secrets*, is preserved in Arabic in the Leipzig City Library and in a Latin translation in the Bibliothèque Nationale at Paris. This important work is being edited by Prof. J. Ruska at Heidelberg. It is severely practical in character, and deals with the recognition, purification, and properties of the chemicals commonly employed, and of the various operations and forms of apparatus in use at the time. It shows throughout the influence of Jābir, whom al-Rāzī often acknowledged as his master.

Al-Rāzī divides minerals into six classes: stones, bodies, spirits, salts, boraxes, and vitriols. Of *stones* there are 13 kinds, including marcasite, magnesia, tutia, lapis lazuli, alum,

antimony sulphide, talc, gypsum, and glass. The (metallic) *bodies* are gold, silver, copper, iron, steel (?), lead, and tin. The *spirits* are sulphur, arsenic (sulphides), sal-ammoniac, and mercury. The *salts* include cooking salt, sweet salt, bitter salt, calcined salt, bituminous salt, *qali* (crude sodium carbonate), and salt of ashes (crude potassium carbonate). The *boraxes* are bread borax, natron, bone-ash, tinkar (borax), and others. The *vitriols* are the black, white, yellow, red, and green.

Most of the list of apparatus has already been given in SCIENCE PROGRESS (October 1922). There are a few utensils, however, of the exact form of which I was at that time ignorant. These are the " crucible son of a crucible," the " ṭābistān," and the " 'amyā." Fortunately my friend, Ḥajji 'Abdu'l-Muḥyī, of the Mosque at Woking, had met several present-day alchemists in Persia and Afghanistan and kindly enlightened me. The *crucible son of a crucible* consists of a crucible with a conical bottom, at the apex of which is a hole. This crucible rests in another, and the whole arrangement is called *baut ibn baut* (crucible son of a crucible). It is used for the reduction of metals. The calx of the metal is mixed with natron and the mixture made up into a paste with olive-oil. The paste is then placed in the upper crucible and the whole heated strongly in a furnace. The fused metal obtained in this way passes through the hole into the lower crucible, the dross remaining behind.

The *ṭābistān* consists of an earthenware vessel, somewhat more than a hemisphere. It contains the reacting mixture and is closed by a bowl containing cold water. Fire is lit beneath it and any sublimate collects on the bottom of the bowl, whence it may be removed as desired.

The *'amyā* consists of two more or less hemispherical vessels. The substance to be treated is placed in one of them, which is then closed with the other, the two being firmly luted together by means of " philosopher's clay." A hole is then dug in the ground and a wood fire kindled in it. When the fire has become a mass of red-hot embers the 'amyā is thrust in and the hole covered over with turf. The 'amyā is removed when cold.

Al-Rāzī was a Persian, born at the town of Ray, near Teheran. He was, says the *Fihrist*, a man with a big head, like a casket, and used to sit in his consulting-hall surrounded by his pupils. The most advanced of these sat nearest him in a ring, and in an outer ring sat the more elementary. When a patient came he described his symptoms first to the elementary students, and if these understood the case they dealt with it. If not, the man passed on to the advanced students,

and if the case was too difficult for them, then Al-Razi dealt with it in person. He was of a kind and gentle disposition, and sympathetic with the poor and infirm. He was continually writing books or making notes, and no doubt this was one of the causes of the blindness which afflicted him towards the end of his life—although the author of the *Fihrist* says it was caused by his excessive consumption of broad beans!

Al-Rāzī died in the early years of the tenth century A.D. To his position of pre-eminence in chemistry succeeded Abu'l-Qāsim Maslima ibn Aḥmad al-Majrīṭī, who lived at Madrid and flourished in the reign of Al-Ḥakam II (961–76). He studied philosophy, mathematics, astronomy, and chemistry in the East, and was brought into contact with the celebrated encyclopædists of Islam, the "Brethren of Purity," whose *Letters* have been published by Dieterici. These *Letters* are anonymous, and it seems to be quite possible that Maslima al-Majrīṭī himself wrote several of them, including that on chemistry. He certainly claims to have done so, and makes many references to them in his great work, the *Rutbat al-Ḥakīm.*

The *Rutba,* which treats of chemistry, is largely theoretical, but contains sufficient exact descriptions of experimental work to show us that the author was a chemist of no little manipulative skill and ingenuity. He begins as follows: "Know, O thou that seekest after the secrets of Nature, that to every effect there is a cause. The cause which led me to compose this book was that I saw my contemporaries in ignorance and error, reading what they could not understand and searching for they knew not what. . . . They were so conceited that they considered themselves to have acquired all possible knowledge and therefore regarded it as unnecessary to read the books of the ancients, and neglected the study of philosophy, which is the Light of the Faith."

He goes on to say that no one who has not studied chemistry can have any claim to the title of *savant.* To acquire a good knowledge of chemistry one should begin by a thorough training in mathematics, preferably by reading Euclid. Some acquaintance with astronomy is also desirable; for this the Almagest is recommended. Logic is essential; this is best studied in the book which Al-Kindi translated from Aristotle. Aristotle's works *De Cœlo* and *De Generatione et Corruptione* are very valuable; so are the works of Democritus and Apollonius, although these may be dispensed with if one has Aristotle. Coming to the "moderns," Al-Majrīṭī says that the books of Jābir ibn Ḥaiyān and of Al-Rāzī are indispensable. Finally, the student must have extensive training in practical work, and must learn to observe keenly and to exercise his

mind about the phenomena of Nature. As to transmutation, there can be only one proof of that, namely, experimental.

The section of the *Letters* of the Brethren of Purity which deals with chemistry opens with an account of the sulphur-mercury theory of metals first definitely formulated by Jābir. This is followed by a description of the metals and of common substances used in chemical operations, and by a scheme of classification. The whole section is characterised by its precise language and clear statement of theory. The debt of its author to Jābir and to Al-Rāzī is apparent throughout, although neither is mentioned by name.

Maslima al-Majrīṭī is typical of "Arab" chemists in his careful exclusion of astrology from chemistry. He believed, like most of his contemporaries, in magic, astrology, and the talismanic art, but, in common with practically all of the great chemists of Islam, he does not allow this belief to interfere with his chemical theory and practice. That this has not always been recognised is probably due, as I have suggested above, to the fact that the mystics of Islam often used the terms of alchemy in a symbolic fashion and their books were later regarded as manuals of chemistry instead of poetic visions. Thus the great Ṣūfī Ibnu'l-'Arabī († A.D. 1240) uses the name *Al-Kīmiya* to express knowledge of the Deity, and calls one of his books on mysticism *Al-Kibrīt al-Aḥmar*, the Red Sulphur, or Philosopher's Stone. I believe that this book, which is nothing more than a manual of Ṣūfī-ism, is still considered by the alchemists of Persia to contain sufficient information to enable them to make gold, if only they could understand it!

A last point of interest in regard to Maslima al-Majrīṭī has to do with one of Berthelot's criticisms of the theory of the identity of Geber and Jābir. Berthelot points out that Geber modestly says that alchemists are nothing more than ministers of Nature, and remarks that this is quite unlike the extravagant claims of the Arabic works. In Part I of section 3 of the *Rutbat al-Ḥakīm*, however, Maslima al-Majrīṭī adopts exactly the same view as Geber, and illustrates his argument by a comparison with the physician, who is "merely a servant of Nature"; he diagnoses the complaint and administers a remedy, but it is Nature who acts. This is not the only case in which Maslima al-Majrīṭī's outlook resembles that of the author of "Geber's works"—a fact which becomes of importance when one remembers the close acquaintance which he had with the books of Jābir ibn Ḥaiyān. He says, "I know of no chemist more skilful or eloquent than Jābir, and although he and I are separated by more than 150 years, yet I regard myself as a true pupil of his on account of my great admira-

tion for his works, all of which I have gathered together . . . and of which I have given the names in my *History of the Arabian Philosophers* [this work has unfortunately not come down to us]." He says further that, in the time of Khālid ibn Yazīd and his predecessors, no one had any doubt of the possibility of transmutation, but that Jābir was confronted with the difficulty that scepticism had arisen. This passage seems to dispose effectively of Berthelot's contention that disbelief in the transmutation theory did not arise until the days of Ibn Sīna (Avicenna), and that therefore the Latin works ascribed to Geber, which contain an elaborate refutation of the sceptics, could not have been written earlier than the tenth century.

The chief Muslim chemists of the later period (A.D. 1200–1400) are Abu'l-Qāsim Muḥammad ibn Aḥmad al-'Irāqī and 'Izz ad-Dīn Aidamir ibn 'Alī ibn Aidamir al-Jildakī. Abu'l-Qāsim al-'Irāqī is said by Hajji Khalifa to have lived in the sixth century after the Flight, but in the preface to his work *'Uyūnu'l-Ḥaqā'iq* (Br. Mus. 1837 2) the names of the reigning Sultan al-Malik al-Zāhir Rukn al-Dīn, of his vizier Bahā al-Dīn, and of his son, Barakah Khān, are mentioned. Al-Malik al-Zāhir Rukn al-Dīn reigned A.H. 658–76, so that we may place Abu'l-Qāsim al-'Irāqī in the seventh century, and not the sixth century A.H. He probably died somewhere about A.H. 700 (A.D. 1300). His most important work is entitled *Knowledge acquired concerning the Production of Gold*, of which three or four manuscripts are in existence. The author begins by "proving" the possibility of transmutation and proceeds to discuss the nature of the Elixir and its preparation. He then supports his arguments by quotations from earlier alchemists, and concludes the book with an explanation of the "dark sayings" used by the sages. As usual, the instructions given for the preparation of the Elixir are incomprehensible, but the work is nevertheless of great interest and importance for the history of chemistry, since it gives a very clear and logical account of the chemical theory of the time. There are also many references to experimental work which the author has carried out, and, in general, the book is remarkable for its clarity of thought and expression, and for its freedom from any taint of magic or astrology.

In Part I, section 1, the author says: "Know that the materials used in the Art of Chemistry are of one species essentially. They are called the metallic minerals and are subdivided into six sorts, varying in form and in properties, but not immutable as are individual animals and plants. They are gold, silver, copper, iron, lead, and tin. Each of them is marked off from the others by accidental distinguishing

properties, and it should be possible to effect the necessary removal of these since the specific nature is constant.

" We say and maintain that two species of natural things which differ radically and essentially cannot be changed and converted one into the other by Art, as, for example, man and the horse. But these six bodies can be mutually converted: thus lead may be converted into silver, for if you place a pound of lead in the fire, it rectifies it and matures it, and most of it is burnt away, leaving a small part as silver—about a quarter of a dirham of pure silver from every pound of lead.

" Now since it is possible for a part of the lead to be thus changed into silver, there is nothing to hinder the conversion of the whole. In the same way silver may be converted into gold, by the refinement of the smelting fire only. For it is tinctured by the fire and strengthened and transmuted and behaves like gold with the touchstone. Thus it is possible to effect a certain transmutation, since the specific nature is constant; but if silver differed from gold in species it would not be possible to convert it into gold, just as it is impossible to convert a horse into the human species by Art, because they differ radically and essentially.

" Another indication of that, and more complete than the first, is that in gold-ores the gold is sometimes found perfect and at other times imperfect. The imperfect can be purified by the fire and separated into silver and gold. In the same way, silver is found in its ores mixed with lead, and can be refined and separated from it. Now the cause of the occurrence of silver in gold-ores is that the heat [of the earth] matures those parts of the ore which are near it and converts them into gold, if the ore is a gold ore, or into silver if it is a silver ore. But it does not mature what is distant from it, by reason of the low temperature and little heat.

" It appears, therefore, that these six metallic bodies are all of one species, distinguished from one another only by differentiating *accidental* properties; their extreme limit is reached when they become gold. Now that which is free from any accidental property is gold, while what possesses these becomes either silver, lead, or tin, if it has the quality of coldness, or copper or iron if it has the quality of hotness. And these six forms of a single species are like fever in an essentially healthy man. When he is treated so that the fever leaves him and he returns to freedom from disease he regains the most perfect state of health."

In the following section he says: " The moistness and dryness of which minerals are composed are nothing but 'watery' steam and 'earthy' smoke; if compounded together

in right proportion they give rise to the six metals, while if the dryness, that is, the smoke, is in too great proportion, then are formed brittle stones such as the marcasites, magnesias, tutias (and the like). If the moistness, that is, the steam, is in too great proportion, mercury, and nothing else, will result." [1]

Abu'l-Qāsim al-'Irāqī's book *Knowledge acquired concerning the Production of Gold* was commented on by Aidamir al-Jildakī in his *Nihayat aṭ-Ṭalab*. Al-Jildakī was a prolific writer on scientific subjects who lived in the fourteenth century and died at Cairo about A.D. 1360. It is difficult to gauge accurately his attainments as a practical chemist, but he was certainly well acquainted with contemporary scientific thought and possessed a deep and extensive knowledge of the work of earlier scientists. His habit of lengthy quotation is very valuable, as the works from which he quoted are in many cases no longer to be found; this is especially true of certain works of Jābir ibn Ḥaiyān. A study of his books shows that much advance in chemistry had been made by the fourteenth century both in theory and in practice. Thus he lays down, as a fundamental principle, that substances react only by definite weights, and mentions the use of nitric acid to separate the silver from a gold-silver alloy. His main defect is his delight in interminable discussions after the manner of the scholastic theologians, but he has the merit of never being dull. Although he himself believed in the possibility of transmutation, he is sufficiently broad-minded to give a full account of the arguments against it. Avicenna, for example, considered that the six metals, instead of being different varieties of one species, were different species of the same genus and therefore could not be transmuted. He agreed that it was possible to colour copper by suitable substances, such as realgar, until it looked like silver, but maintained that it was still copper although "dyed" a different colour. Al-Jildakī admits this, but regards density, colour, and similar properties to be "accidental," that is, superimposed upon the metallic essence, the latter being the same in all metals. He argues that by removing its accidental properties and substituting those of gold, it should be possible to transmute silver into gold. Avicenna's objection, then, would apply only to those "transmuted" metals whose colours were not "fixed," and could be removed by smelting or cupellation. It is interesting to note that the chief contributions to chemistry in the Middle Ages were made by men who believed in the possibility of transmutation. In spite of its many and grave defects, and of the incubus which, long after, it proved to be, we must admit

[1] Quoted from the writer's edition of *Al-'ilm al-muktasab fi zira'at al-dhahab* (Paris, Geuthner).

that in these early days the transmutation theory correlated facts, inspired research, and unified chemical thought where lesser theories would probably have signally failed.

The evolution of chemical theories is a fascinating subject, and the development of one particular theory can be followed with great accuracy by a study of the chemistry of Islam: I refer to the theory of combustion. This arose out of the theory of the constitution of mineral bodies, especially the metals. An early Greek theory, which we find revived in Abu'l-Qāsim al-'Irāqī (see above), was that minerals are composed of "watery steam" and "earthy smoke." Jābir ibn Ḥaiyān modified this into the sulphur-mercury theory, the mercury corresponding to the watery steam and the sulphur to the earthy smoke. Calcination of a metal was then explained by supposing that the metal lost part of its sulphur. Further investigation showed that the literal sulphur-mercury theory could not be upheld, and we find that an oily constituent of metals is postulated; this oil is the cause of combustibility, and is present in abundance in sulphur. From this to the *terra pinguis* of Becher and thence to the phlogiston theory is but a short step.

A very remarkable feature of the Arab chemists is the thoroughly sound scientific outlook which most of them possessed. It is remarkable in this sense, that in general the Oriental mind will seize on one isolated point and discuss it endlessly without any attempt at correlation; it seems incapable of evolving a theory which will cover a wide group of facts. Men like Jābir and Al-Rāzi, however, were the very antithesis of this, and their theories were firmly based upon observation and experiment. To them chemistry was a serious science, to be undertaken only after long and careful training of the mind and hand and eye. What an advance upon the Founder of Chemistry, Hermes Trismegistos, who is reported to have said, "I taught my son chemistry in three days and used no apparatus—not even an alembic"!

THE PRESENT POSITION OF THE DARWINIAN THEORY

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"And there arose another king that knew not Joseph."

It must be admitted that the Darwinian theory of evolution by natural selection has fallen upon evil days. Prominent literary men like G. K. Chesterton have allowed themselves to talk of the "dead dogma of Darwinism." Although the connotation of "Darwinism" in the minds of such emotional and inexact writers is somewhat obscure, nevertheless they reflect an impression which they have imbibed from scientific acquaintances, *viz.* that our views on many things connected with life have changed since Darwin's time, and that most of Darwin's arguments will no longer hold.

They cannot be blamed for conceiving this idea, for to a hearer of the discussion on evolution which took place last September at the meeting of the British Association at Hull,[1] it must have seemed clear that many zoologists and botanists were inclined to deny that natural selection was the efficient agent in the formation of new species. Prof. Stanley Gardiner from the standpoint of zoology, and Prof. Johannsen on the botanical side, both emphasised the "inadequacy of natural selection," whilst if we turn to Dr. Willis's *Age and Area*—a book which really gave rise to the discussion—we find the statement (p. 215): "It is clear that it is no longer safe to consider that advantage to the species has had anything to do with the actual evolution of that species." Most serious of all, if we refer to Dr. Bateson's address to the American Association for the Advancement of Science on the occasion of their Toronto meeting in 1921,[2] we find the statement: "That particular and essential bit of the theory of evolution which is concerned with the origin and nature of species remains utterly mysterious. We no longer feel as we used to do that the process of variation, now contemporaneously occurring, is the beginning of a work which needs merely the element of

[1] Reported in *Nature*, December 2, 1922. [2] *Nature*, April 29, 1922.

time for its completion." It is within the recollection of all that Bateson's address was made the pretext for a widespread and virulent attack on Darwinian teaching, an attack which was largely taken up throughout the United States and Canada, and very nearly led to the passing of a law prohibiting the teaching of evolution in the state of Kansas.

If we now endeavour to form a judgment as to how far this very general dissatisfaction with the Darwinian theory is justified, the first step must be to turn to the authoritative presentation of that theory contained in the sixth and last edition of the *Origin of Species.* It is boldly outlined in the first five chapters, the remainder of the book being occupied with answers to innumerable objections. In the first chapter the great variability of our domestic animals and cultivated plants is described; in the second Darwin deals with the variation of wild species. He points out that systematists who believed in the origin of each species as a distinct act of creation, nevertheless recognised subspecies and varieties which they regarded as derived by descent from the main species; and that no line could be drawn between subspecies and species, because what some naturalists regarded subspecies others regarded as true species.

He admits that the word "variation" is of vague and uncertain import. It may mean a monstrosity, *i.e.* a considerable deviation from normal structure, or it may indicate the slight differences which distinguish the offspring of the same parent from one another. He expresses a doubt as to whether monstrosities such as frequently occur in domestic productions are ever permanently propagated in a state of nature. The second type of variation, on the contrary, he regards as of the highest importance: he states that such variations lead, by insensible steps, to the differences separating slight varieties and through these to the differences between well-marked varieties, and so to the differences separating subspecies and species. In the third chapter Darwin describes the struggle for existence which necessarily results from the tendency of each species to increase its numbers in a geometrical ratio, and points out that this struggle will inevitably lead to the survival of any individual a little better fitted to its surroundings; and for this struggle he introduces the term NATURAL SELECTION. In the fourth chapter he develops the idea of natural selection still further. He points out that in order that any considerable modification should be effected in an incipient species, a variety once formed must again, perhaps after an interval of time, vary or present individual *differences of the same favourable nature as before*, *i.e.* must present further variations in the same direction as that in which the first variations, which ensured its survival,

occurred. This assumption is absolutely vital for the validity of Darwin's theory in its original form. On this subject Darwin says: "Seeing that individual differences of the same kind perpetually recur, this can hardly be considered as an unwarrantable assumption. But whether it is true, we can judge only by seeing how far the hypothesis accords with and explains the general phenomena of nature. On the other hand, the ordinary belief that the amount of possible variation is a strictly limited quantity is likewise a simple assumption." We shall return later to the examination and criticism of this point.

Darwin next develops his theory of sexual selection, or the contest between males for the possession of the female. He points out that if males vary in their secondary sexual characters—and he produces much evidence to show that they do—then the most vigorous and attractive male will win the female and transmit his qualities to posterity, and so the special organs and colours connected with this contest will be perpetuated. He then goes on to point out that once a distinct variety has been formed, the more it diverges from the parent species the better chance it will have of maintaining itself, for it will be enabled to occupy a slightly different place in nature to nourish itself with slightly different food, and thus the struggle between it and the parent species will be mitigated.

The fifth chapter of the *Origin of Species* is in many ways the most valuable in the book, for it contains a summary of Darwin's extensive observations on the kinds of variation which actually occur. He attempts to deduce "Laws of Variation," and utterly denies the idea that he regards variations as due to chance. His general conclusion is that "variability is generally related to the conditions of life to which each species has been exposed during several successive generations." "Changed conditions act directly on the whole organisation or on certain parts alone, or indirectly through the reproductive system. The direct action of changed conditions leads to definite or indefinite results. In the latter case the organisation seems to become plastic and we have much *fluctuating variability*. In the former case the nature of the organism is such that it yields readily when subjected to certain conditions, and all or nearly all the individuals become modified in the same way." Darwin then proceeds to formulate certain laws, which may be enumerated as follows:

(1) There can be no doubt that use, in our domestic animals, has strengthened and enlarged certain parts and disuse has diminished them, AND THAT SUCH MODIFICATIONS ARE INHERITED.

(2) Species can become acclimatised to quite different climates from those which were originally native to them: this

may be due to natural selection of hardy varieties having different innate constitutions; but (inherited) habit has some influence, because all agricultural books urge caution in transporting animals from one district to another. It is unlikely that man should have succeeded in selecting many breeds with constitutions fitted each to its particular district; THEREFORE ACCLIMATISATION AND THE INHERITANCE OF ITS EFFECTS MUST HAVE PLAYED A PART.

(3) When slight variations in one part of the body occur and are accumulated by natural selection, other parts become modified in consequence. This is termed by Darwin "correlated variation." Thus homologous parts tend to cohere, like the petals of a flower, hard parts affect the form of adjoining soft parts, and so on.

(4) If nourishment flows to one part or organ to excess, it rarely flows, at least in excess, to another part. This is termed by Darwin the law of compensation or balance of growth. It is explained by him thus: If under changed conditions of life a structure before useful becomes less useful, its diminution will be favoured, for it will profit the individual not to have its nutriment wasted in building up a useless structure.

(5) Multiple, rudimentary, and lowly organised structures are variable. By multiple is meant metamerically (or radially) repeated in series; and such organs are for the most part "lowly organised," *i.e.* not differentiated, and therefore, as Darwin remarks, used for many functions, so that slight deviations in any one direction are not accumulated by natural selection. Rudimentary (vestigial) structures are variable because, having ceased to be useful, slight deviations from the normal in their structure do not determine the survival of their possessors.

(6) Any part developed in one species in an extraordinary manner compared with its development in other species tends to be variable. Thus secondary sexual characters, frequently used to discriminate one species from another, are extremely variable in their development within one species. Again, the valves of the sessile barnacle *Pyrgoma* differ markedly from the type common to most sessile barnacles, and the number and arrangement of the plates of the valves are extraordinarily variable within the single species of *Pyrgoma*.

(7) The diagnostic characters of species are more variable than those of genera; thus, as noted above, secondary sexual characters are extremely variable.

(8) Distinct species often present analogous variations, so that a variety of one species assumes a character proper to an allied species or reverts to some of the characters of an early progenitor. As an instance of reversion he gives the tendency

of all breeds of pigeon to produce slaty-blue birds with two bars on their wings like the original *Columba livia*. The tendency of horses and asses to have stripes of colour on their legs and spinal stripes is likewise adduced as an instance of analogous variations.

If we read over this list of "laws," we must first exclude the assumed action of natural selection in accumulating favourable variations, because our endeavour is to discover not the results of natural selection, but the material on which it has to work. Then we find that Darwin's laws boil down to two, *viz*. (1) the inheritance of the effects of use and disuse, in a word of habit, which is the Lamarckian doctrine of evolution; (2) an indefinite tendency to vary to an unlimited extent in all directions, each individual variation being small, which Darwin attributed to the action of climate in making the organisation of the species "plastic."

In a former article to SCIENCE PROGRESS I pointed out that the truth of the Lamarckian doctrine was assumed as a matter of course by the earlier champions of Darwinism such as Haeckel; it only fell into disrepute through the teaching of Weismann, and his dogmatic assertion of the impossibility of the germ-cells being affected by the soma, assertions which now sound quite as inconclusive as the futile experiments by which Weismann sought to support his uncompromising rejection of Lamarckism. I shall deal briefly with this subject a little later, pointing out the additional evidence in favour of Lamarckism which has accumulated since I wrote my first article.

The Lamarckian explanation having thus been discredited, the supporters of evolution fell back on the law of "indefinite variability." The chief objection to this law was that it was too facile. As already pointed out, it was not merely necessary to assume that organisms could vary in all directions, but that the offspring of any one individual would continue to vary further in the same direction as that in which its parent had departed from the normal condition of the species. It is obvious that, if we can make that assumption, any conceivable change can be "explained." As every change consists of an accumulation of favourable variations, so every change must be assumed to have played a part in enabling the individuals which showed it to survive. It was the fashion some thirty years ago to make somewhat crude guesses as to the utility of structures, guesses which those who made them never thought it worth while to put to the crucial test of experiment. This kind of procedure disgusted those who were anxious to come to closer grips with the problem, and led to a reaction against the whole idea of the accumulation of small favourable variations. But to this general criticism of loose thinking on the part of the

older Darwinians several notable exceptions must be made. The late Prof. Weldon, of Oxford, noticed that the common shore-crab varied in the breadth of its carapace—a variation which involved the width of the gill chamber and the width of the orifice leading to this chamber. By subjecting crabs to the action of sea-water made turbid by china-clay, Weldon showed that only crabs with narrower carapaces survived, and that these alone were able to prevent the clay from entering the gill chamber and so to maintain their respiration unimpaired. Prof. Bumpus, after a blizzard in New England, collected the sparrows which had succumbed and, measuring their chest dimensions, found that these were narrower than the average in a random sample of the surviving population.

But the greatest difficulty in the way of accepting the doctrine of indefinite variability was furnished by the results of what have been called "pure-line" investigations. By this term is meant investigations which have had for their object the determination of the inheritance of variability amongst the offspring of a single parent. There are three ways in which such a population can be raised, *viz.* (1) we can self-fertilise the eggs of an hermaphrodite animal or plant, or (2) we can rear the progeny of a parthenogenetic female, or (3) we can isolate the offspring of a single Protozöon reproducing itself asexually by transverse fission.

All three ways have been employed. Prof. Johannsen raised a crop by sowing a single bean. When the bean plant had grown up, he self-fertilised the flowers with their own pollen and thus secured his first crop of beans. He sorted these beans into lighter and heavier lots and sowed them, again self-fertilising the plants which he thus raised. He found that on an average the offspring of a lighter bean were just as heavy as those of a heavy bean, and that it was impossible, by practising selection through several generations, to effect any change in the hereditary characters of the stock. The apparent change produced by selection in a mixed population, the offspring of many parents, he explained by supposing that such a population was a mixture of various strains—each strain with its own unalterable hereditary tendency. When the cultivator selected through several generations a heavier bean, only the strains producing the heaviest beans were left, all those giving rise to lighter beans having been eliminated, and so the average of weight amongst the population was raised. When, however, all but one strain had been weeded out, further selection had no result.

Agar experimented with the parthenogenetic Crustacean *Simocephalus*, which carries its eggs in a brood pouch on its back, from which they emerge in a form not unlike the parent.

Fully adult size is attained in three or four moults. Agar found that these Crustacea varied in the length of their carapace, and he tried, by selection from amongst the offspring of a single individual, to produce strains with longer and shorter carapaces respectively. He utterly failed to do so; in fact, he states that the offspring of an individual with a short carapace showed on the average a rather greater carapace length than the offspring of an individual with longer carapace. It is to be noted, however, that the length of carapace tended to vary with the temperature of the water in which the Crustacea were living, and that Agar had to introduce a correction for this variation.

Jennings produced offspring by the repeated division of single specimens of the slipper-animalcule *Paramecium.* He arrived at the same result as Agar. The offspring of a single individual varied in length about a fixed mean: if from amongst them specimens were selected to carry the culture farther, it was found that the mean length of the progeny of a short individual was exactly the same as the mean length of the descendants of a longer individual.

The concordance of the results of these three investigators is very remarkable. One could hardly choose three organisms more remote from each other in the scale of life than a flowering-plant, a Crustacean, and a Protozöon. We are driven, I think, to the conclusion that there is no evidence of the existence of that variability to an indefinite extent in all directions which Darwin postulated. In a word, the minute differences dividing members of one litter from one another are not inheritable. It is usual nowadays to attribute such differences to accidental variations in nutrition either pre-natal or post-natal, and they are termed FLUCTUATIONS. If we reject both the inherited effects of habit and indefinite variability, there remains only a third alternative to fall back on, *viz.* the strongly marked variations formerly denominated sports or monstrosities, which sporadically occur not only amongst our domesticated breeds, but also amongst wild species, and which are known to be strongly inherited. It is no part of our purpose to weary the reader with a recital of the well-known laws of Mendel, which have been discussed *ad nauseam* in this journal; but we should like to remind him of two things: first, the laws of Mendel deal with the hereditary behaviour of these sports when crossed with the normal type, and not with their origin; and, secondly, the sports are relatively rare in their appearance, and as a rule are found only in the proportion of one to many thousands of normal individuals born.

It was on this rarity that Darwin fastened when he rejected these variations, or "MUTATIONS" as they are now termed, as the possible material for evolution. The chances of a sport,

if it survived, mating with its like he considered infinitesimal, and if it mated with the normal, he believed that its peculiar character would be swamped. It is true that Mendelian laws somewhat mitigate this objection of Darwin's; we know that the sport, when crossed with the normal, though apparently obliterated in the first generation, reappears in the proportion of one in four in the second generation; but still the main force of this objection incontrovertibly remains. The fortuitous destruction of young in all stages is so great that the chance of a favourable variation which occurred at rare intervals seriously influencing the next generation must be considered *nil*.

But this rarity is by no means the only difficulty which we encounter when we endeavour to account for evolution in terms of "mutations." It will scarcely be denied that the differences between closely allied species give us some idea of the initial steps of evolution. If, however, we compare these differences with the most frequent mutations, we are struck with the entire dissimilarity between the two.

Bateson, in his *Materials for the Study of Variation* (1894), collected and figured a large number of these mutations, and suggested that in their abrupt divergence from the normal they give an explanation of the apparent discontinuity of species. Wallace, in criticising this conclusion, pointed out that none of Bateson's mutations could have played any part in the evolution of vertebrates or arthropods.

Allen and Sexton[1] have investigated the mutations in the eye of the fresh-water shrimp *Gammarus*; they have shown that when these mutations are crossed with the type the progeny behave in a normal Mendelian manner. The eye of the type has dark brown pigment, but a sport with red eye pigment turned up, and later one with colourless eyes. In some of these sports the lenses tended to be dissociated from one another.

Now Schneider[2] has examined the eyes of the *Gammarus* living in the subterranean waters of Clausthal. He finds that, compared with the eyes of the type, the eyes of these subterranean forms are larger but shallower and that the pigment is somewhat darker. At the periphery of the eye, however, the eye elements are beginning to be dissociated from one another. This dissociation Schneider attributes to a diminution in quantity of the pigment embracing these elements and holding them in place.

Vejdovsky has examined the eyes of two genera allied to *Gammarus*, *Bathonyx*[3] and *Niphargus*,[4] which live in

[1] "Eye-colour in *Gammarus*," *Jour. of Genetics*, vol. ix, No. 4, 1920.

[2] "Die unterirdische *Gammarus* von Clausthal," *Sitzberichte König. preuss. Akad. Wiss.*, Berlin, 1885.

[3] "Ueber einige Sasswasseramphipoden," *Sitzungb. König. böhm Gesell.*, 1905.

[4] *Ibid.*, 1900.

comparatively deep water, and finds further stages in eye-reduction of the same kind as those described by Schneider. Thus in *Bathonyx* the eye elements, or ommatidia, are greatly reduced in number and quite separated from one another: each is still immersed in pigment. The crystalline cones are degenerate, consisting of oval, clear vesicles. Finally, in *Niphargus* the pigment has disappeared, but so has all trace of eye-structure, a lengthening of the hypodermic cells in the eye-area alone remaining. We see that the natural reduction of the eyes of Gammarids due to loss of function has pursued a totally different course from that indicated by the pathological mutations described by Allen and Sexton.

Another difficulty in regarding mutations as the material for evolution lies in the demonstration by Bateson and Punnet that when two strains are crossed one of which is dominant to the other, the recessive strain is distinguished from the dominant by the absence of some factor or ingredient present in the dominant. Further, when the wild ancestor of the domesticated form can be identified, as in the case of the pea, all the dominant "factors" are found conjoined in this ancestor, so that apparently all mutations which have given rise to the domesticated breeds have been of the nature of losses. The recognition of this fact led Bateson in 1914 to suggest that the evolutionary process had consisted in a series of losses, and that the qualities of Shakespeare were innate in the original Amœba, but were prevented from manifesting themselves by inhibitory factors which were subsequently dropped. The facts which suggested to Bateson this paradoxical conclusion should lead us rather to conclude that mutations are not the raw material of evolution, a conclusion which, oddly enough, was adopted by Bateson on other grounds in his Toronto address and in a speech to the Genetical Society at Wimbledon in 1922.

It is not true, however, that all mutations are recessive to the type. Indeed, the mutations of the combs of fowls, which led Bateson and Punnet to formulate their "presence and absence" theory, are dominant to the "single-comb" of our game-breeds, which is also characteristic of the wild jungle-fowl ancestor *Gallus bankiva.* Leaving these mutations on the one side for the moment, let us look at other cases of dominant mutations.

Bateson, in his book *Mendel's Principles of Heredity* (1909), gives a list of dominant mutations in Man. These include (*a*) brachydactyly, the fusion of two joints so as to produce short, stumpy fingers; (*b*) early congenital cataract, *i.e.* the loss of transparency in the lens of the eye; (*c*) *Tylosis plantaris* and *palmaris, i.e.* the abnormal thickening of the skin on the palms of the hands and soles of the feet; (*d*) *Epidermolysis*

bullosa, liability of the skin to blister for trifling causes; (*e*) *Hypotrichosis congenita familiaris*, *i.e.* the complete loss of hair at birth. We are at once struck with the monstrous and pathological character of these, and this impression is heightened when we survey other cases of dominant mutations. No animal has been more thoroughly investigated for its hereditary factors than the little banana-fly *Drosophila melanogaster*. Sturtevant estimates that 10,000,000 specimens of this fly have been raised for experimental purposes, and no less than 250 distinct mutations have appeared amongst these laboratory animals. Of these the vast majority are recessive when crossed with the wild type, but some few are dominant. These dominant mutations are plainly monstrous, and when crossed with one another are not fertile. Gates has produced evidence to show that this is also true of the apparently slight and comparatively harmless mutation, brachydactyly in man. When two brachydactylous people marry they have no children.

We have noted above that the types of comb in fowls known as rose and pea are dominant to the single-comb characteristic of the wild ancestor. We might be challenged to prove that these types are monstrous variations, for the barnyard fowls showing them are not unhealthy. But these aberrations are members of series of increasing divergences from the normal type; more extreme types, such as the Breda comb, show a few feathers intervening between the lobes of the comb, and the series culminates with the Polish fowl, in which the comb is represented by a soft swelling carrying a large tuft of feathers beneath which there is a large vacuity in the skull. We can see, I think, that both dominant and recessive mutations belong to the same category—in each case we have to deal with a *failure of regulation*. We now know that in the growing and developing embryo, the development of any part controls and is controlled by the development of other parts, and in a healthy animal there is a balance between the growth of the various organs *which determines the specific features of the animal*. It is this balance which we term REGULATION, and it is the disturbance of this balance which gives rise to a Mendelian mutation. If the disturbance is relatively slight, so that, when present in one parent only, the child appears to be normal, then the mutation is said to be *recessive*; but if it is of a more fundamental character, so that its appearance in one parent results in visible consequences to the child, then it is said to be *dominant*.

A very small acquaintance with wild species teaches us that each has its own REGULATORY BALANCE; and so the ultimate problem of the origin of species resolves itself into

the answering of this question: When one species is derived from another, is the original regulatory balance changed into a new one, by the chance occurrence of a series of small disturbances (mutations), or is the new regulation a direct adaptation of the daughter species to the new conditions?

Sturtevant[1] has endeavoured to answer this question by examining all the wild species of Drosophila found in North America. His conclusion is as follows: "The species usually differ slightly from each other in innumerable characters, but are not strikingly different in any character. The mutant races, on the other hand, are alike in most details of their structure, but often differ strikingly in a few characters." Nevertheless, Sturtevant endeavours to persuade us that specific differences are of the same kind as mutations. This he does by trying to find analogies between *Drosophila* mutants and the characters of other species. In this search he seems to us to be singularly unsuccessful. He only succeeds in getting "suggestions" of similarity, and to do this he has to search amongst different genera, sub-families, and even sub-orders. Even granted that he was successful (which he is not), it seems to us that the Darwinian difficulty in accepting mutations as the material for evolution would be intensified a thousandfold. For if these mutations occur very rarely (which is the case), and if allied species differ from one another in hundreds of small independent characters, then each of these divergent characters must have arisen as an independent mutation; and the chances of an individual combining two mutations each of which appears in one of a thousand cases would be one in a million!

There is another objection to considering mutations as analogous to specific differences which is strongly insisted on by Bateson. He points out that all the mutants of *Drosophila*, however divergent in character, are fertile when crossed with the parent stock, but it is only possible to make one interspecific cross in the case of *Drosophila*, and in this case the hybrids are completely sterile.

We shall deal later with the question of interspecific sterility, but the difficulty that species differ from one another in many characters has been recognised both by Darwin and by De Vries. As Darwin was fully convinced that the variations on which natural selection worked must be of a kind that frequently occurred, it was obviously a reasonable supposition to make that natural selection first favoured the intensification of one character; and then of another, that, in a word, many of the differentiating characters separating species had been evolved one at a time. Then Darwin pointed out that the acquisition

[1] "The North American Species of *Drosophila*," Carnegie Institution of Washington, 1921.

or intensification of one character entailed modification in others either by mutual pressure or correlation of growth.

When, however, the inheritability of Darwin's "indefinite variability" was questioned, this explanation would no longer hold. Then De Vries announced that he had discovered the origin of genuine new species in the case of the evening primrose, *Œnothera lamarckiana.* Selecting three plants from a potato field, he transferred them to his botanical garden at Amsterdam and raised from them thousands of plants. Amongst these there appeared in small proportions (usually one in a thousand, rarely one in two hundred) sports of various kinds, which, when fertilised with their own pollen, bred true. To each of these sports, which he regarded as incipient species, he gave a special name, and as each differed from the type not in one but in many small characters affecting stem, leaves, and flowers, he came to the conclusion that *Œnothera lamarckiana* was a species undergoing a fit of mutation and throwing off new species which arose fully equipped with all their differentiating characters, like Minerva from the head of Jove. It was, of course, embarrassing to fail to find any other species in a similar "fit": this rather raised the suspicion that there must be something peculiar in the constitution of *Œnothera* to account for the phenomenon. Further inquiry has confirmed this conclusion. *Œnothera* belongs to the North American family of the Onagracea, only one genus of which (*Epilobium*) is native to Europe. The species *Œnothera lamarckiana* is not found wild in America; it has been used as an ornamental garden plant in Europe for a century and a half, and it seems very probable that, like so many other garden plants, it owes its origin to the art of the gardeners in hybridising two wild forms. This supposition is supported by the fact of the relatively great sterility of the pollen, only about half of it being good. The mutants might, then, be regarded as Mendelian segregates.

Davies and Cleland[1] have investigated other species of *Œnothera,* notably *Œnothera franciscana* and *Œnothera grandiflora.* They find that in these forms nearly all the pollen is good and a very large proportion of the seed germinates, and that no mutations appear. The culture of these forms was carried on for eight years.

The results of these workers seem to us to justify the dismissal of the mutants of *Œnothera* as throwing any light on the origin of species. Since, then, we cannot rely on mutations as the material for evolution, we are driven to ask whether the effects of use and disuse may not after all furnish us with what we require. The primary question is, of course, whether these

[1] "The Reduction Divisions in the Pollen Mother-cells of *Œnothera franciscana,*" *Amer. Jour. Bot.,* vol. ix.

effects are in any degree inherited. In my previous paper to SCIENCE PROGRESS I pointed out that there existed records of experiments performed in Vienna extending over eighteen years, which, if true, definitely proved that alterations in structure due to use were to a certain extent handed on to the next generation. The usual answer to this is to say that the results rest on the word of a single experimenter (Kammerer), and, therefore, cannot be accepted until confirmed. Of course, all discoveries in biology must remain doubtful until confirmed again and again, but those Mendelians who make this criticism of Kammerer's work show no inclination whatever to repeat his experiments and so test the truth of his conclusions for themselves. Until they do so, their criticisms are out of court and the experiments remain a prima facie evidence of the inheritability of acquired characters. Quite recently, however, confirmation of Kammerer's correctness has come to light. Przibram has publicly asserted that all Kammerer's work is *bona fide* and was done under Przibram's supervision, and Przibram's reputation as one of the leading physiological biologists of Europe is sufficient to crush at once the insinuations of dishonesty and fraud on Kammerer's part in which leading Mendelians have indulged.

In my previous paper I mentioned that one ground for the suspicion alleged against Kammerer was his statement that, by inducing the midwife toad *Alytes*, which normally pairs on land, to breed in water, he had succeeded in causing the male to develop a horny nuptial pad on the hand, similar to the pad developed by the males of other frogs and toads during the breeding season. Bateson visited Vienna in 1910 and challenged Kammerer to produce a modified male *Alytes* and Kammerer failed to do so—the reason being, as Kammerer explained later, that he was keeping all the modified males in his possession for breeding tests and that the nuptial excrescence was cast off when the breeding season was over, and that therefore no toad showed this excrescence at the time of Bateson's visit.

This year (1923), however, Kammerer visited England as the guest of the Cambridge University Natural History Society, and he delivered an address to the Linnean Society at which his principal British critics were present. He exhibited a modified male *Alytes*, and also sections through the skin of the acquired nuptial pad. In the subsequent discussion Bateson generously and unreservedly withdrew all charges against Kammerer's bona fides and frankly accepted his results as genuine. The only question therefore remaining was as to the interpretation of these results. Bateson made three objections which were easily answered. These were: (1) that the horny skin on the hand of the male *Alytes* could not be a

nuptial pad, because the male clasps the female with the dorsal surface of the hand and the horny pad was on the ventral surface.

(2) That the sections through the pad differed from the sections through the nuptial pads of other Anura in that the characteristic papillæ were absent.

(3) That if a change in hereditary potency were really effected, he could not understand why duration of time was supposed to be effective, any more than it was necessary when a definite chemical change occurred in a non-living substance.

The answer to (1) is that all Anura have the nuptial pad on the ventral surface of the hand or forearm, and presumably, therefore, the male clasps the female with this surface—and that *Alytes* does not differ from other Anura in this respect.

To (2) it may be answered that the section which Kammerer exhibited at the meeting of the Linnean Society showed all the characters of a nuptial pad. The papillæ were not so much developed as they are in the nuptial pad of a frog, but they were obviously of the same nature. Kammerer stated that every species of toad which he had examined showed a nuptial pad of different but characteristic structure. Bateson's criticism was largely based on sections which Kammerer had sent him some time before in which the characteristic papillæ were not so strongly marked as they were in the section shown at the meeting.

The third objection raised by Bateson has its origin entirely in his own crude conceptions of the nature of hereditary change. He evidently regards it as a process similar to that by which, for instance, sulphur dioxide (SO_2) is converted into the trioxide (SO_3). The sudden appearance of Mendelian sports, and the persistence with which, once they have appeared, they transmit their characters to posterity, doubtless have suggested this view. But if acquired characters—or, in simpler words, the modifications of structure due to modified habit—are really inherited, then we must take a different view of the process. We must imagine that there is a slow accumulation in the germ-cells of substances produced by the modified structures, and the accumulated work of generations may be necessary before the new tendencies can entirely override and suppress the ancestral ones.

Kammerer gave a brief description of some as yet unpublished work on the Ascidian *Ciona intestinalis*, which he—we think with good reason—regards as absolutely decisive on this question. As these experiments require a relatively short time for their execution compared with experiments on salamanders and toads, there is hope that they will be soon repeated in this country and that critics will consequently be silenced. Briefly

these experiments amount to this. *Ciona* has long transparent inhalent and exhalent siphons. If these be cut off they are soon regenerated. If they be again cut off and this process repeated several times, the animal eventually produces siphons much longer than the normal ones. If it be allowed to reproduce (and as it is a hermaphrodite animal it can be "selfed," so that complications of crossing are excluded), then the young from the beginning of their growth produce siphons very much longer than those produced by the young of control specimens which have not been subjected to this process of amputation.

It is difficult to find in the work of any other author observations exactly parallel to those of Kammerer, for the reason that no one else has had the patience and perseverance to carry out difficult experiments extending over a course of eighteen or nineteen years.

Nevertheless, some work has been done which indirectly supports Kammerer. We have stated that if the effect of habit is to be handed on to the offspring, the altered body tissues must in some way affect the germ-cells, and that we can only conceive of this happening through the emission by the tissues into the blood of some chemical (hormone) which acts on the germ-cells. Now Guyer and Smith, by injecting the pulped-up lens of a rabbit into a fowl, produced a cytolysin which, when injected into a pregnant rabbit, checked or inhibited the development of the lens in some of the embryos in her womb. These embryos when born developed into rabbits with imperfect eyes, and these when bred transmitted the defect for six generations, and the defect was inherited through the male as well as through the female. Guyer's conclusion is that the lens of the defective eye emitted a hormone which was absorbed by the germ-cells.

We have no a priori right to assume that if an animal is exposed to conditions to which it fails to respond adequately, and in consequence of which its structure and functions undergo degeneration, this degeneration should be handed on by inheritance. Nevertheless, if we should find that this is actually so, it would strengthen our belief in the inheritance of the effects of habit, which are, after all, only the structural and functional effects of exposing an animal to conditions to which it *can* respond.

In *Science* for December 15, 1922 (New Series, vol. lvi, No. 1480), Griffiths records the results of a series of experiments made on white rats. These animals were confined in small circular cages mounted on vertical axles, which were kept in constant uniform rotation day and night. The rate of rotation varied in different cases from one a second to three times in two seconds.

After a sojourn of some months in these cages some of the rats were removed to terra firma. These rats showed permanent changes in bodily movement and posture. The animals continued to move round and round their nests and to twist the head to the right or to the left. Occasionally swellings and discharges in the region of the ear were discovered, and frequently there was a loss of tone in the body-muscles ending in death. The pathological features usually showed themselves a week or two after the rats had been removed from the cages. Before this occurred, some individuals were mated with normal control white rats. These unions gave rise to offspring some of which showed the same "disequilibration" symptoms as did their parents which had been in the whirling cages. These symptoms have appeared in some rats of the fourth generation! Altogether, out of a total progeny of various generations of 500, 60 disequilibrated individuals have been found. This is perhaps the most striking evidence in favour of the inheritability of the effects of the environment which has as yet come to light.

There is, then, good prima facie evidence that continued exposure to new conditions induces changes in animals which are to a certain extent handed on by heredity. If we accept this evidence, then, we can understand why so many species of plants and animals are divided into local races. Incidentally, we may remark that the word "race," like the word "variation," has been used in various and mutually contradictory ways. It is used, for instance, for the progeny of a pair of selected individuals and for the members of a domesticated breed or strain. In the sense, however, in which we use it, it is equivalent to what Lord Rothschild has called a subspecies. Darwin uses the word "variety" in the same sense. In the *Origin of Species* (p. 264) he says: ". . . if we lay on one side . . . all mere temporary variations such as size and albinism, the more permanent varieties are generally found inhabiting distinct stations, such as high land or low land, dry or moist districts, etc."

The investigation of these local races is a difficult problem far beyond the means of the average zoologist. It can only be carried out by great museums with large funds at their disposal, and yet it is vital to the solution of the problem of species. Lord Rothschild, with the help of his admirable curators in his museum at Tring, has done great service in determining the occurrence and distribution of these races, and his experience—which has involved the collection of hundreds of specimens of the same species from each region where it occurs, all over the globe—leads him to assert that all species except migratory ones can be divided into local races, and that no line can be drawn between the distinguishing marks of the

distinct races of a species and those of closely allied species living in distinct regions.

Similar conclusions have been arrived at by Schmidt in dealing with the common eel and the so-called viviparous eel. The common eel is found in all the fresh-water lakes and streams of Europe and North Africa, and it migrates to the deep waters of the North Atlantic to spawn. As eels arrive in the same place from all quarters, and as the spawn deposited by those of one district is continually fertilised by the milt of the males from another district, it is not to be wondered at that no local races of the eel exist. On the other hand, a contrasting case is afforded by the so-called viviparous eel *Zoarces*, which is an elongated Blenny. The young of this fish come into the world with habits like those of their parents; they are not migratory, but tend to form small semi-permanent communities wherever they are found. As a result the local races of this fish are legion in number—distinct races occur at the upper end and at the mouth of the Lijm Fiord, in Jutland, for example; and the differences between these two are correlated with greater sluggishness in the first case and greater mobility in the second.

A beautiful example of these local races was given to the writer by Lord Rothschild. A certain species of crested lark is found in Algeria. As one traces it from the seaboard to the edge of the Sahara Desert it changes in colour from brown to sandy grey. But scattered over the fringe of the desert and breaking up the sandy waste are patches of red earth. On these patches in the breeding season are to be found pairs of larks of a reddish-brown colour contrasting strongly with their grey companions which make their nests on sand. Another example, taken from lizards, has been described by Boulenger,[1] and is based on an examination of British Museum material. The lizard *Chalcides ocellatus* extends from the Canary Islands through Morocco, Algeria, Tripoli, and Egypt to Somaliland. As one traces it eastward it changes its shape from a short, thick-bodied form with stout legs to a long, slender form with thin, weak limbs. When the two extreme forms are contrasted with one another, they seem to belong to distinct species, and yet an unbroken continuity can be traced between them by intermediate forms.

But we now encounter the objection raised by Bateson: that whereas local races are fertile when crossed, "pairs of species" are almost always separated by the barrier of sterility. He further states that to explain species we must await proof of the occurrence of mutations amongst the progeny of the same parent, which should render some of them unfertile when crossed

[1] "On Some Lizards of the Genus Chalcides," *Proc. Zool. Soc.* (Lond.), 1920.

with the rest. The point of view of Willis in *Age and Area* is somewhat similar. He arrives at the conclusion that "endemic species," that is species confined to small areas, are not the last lingering remnants of species on the verge of extinction, but new species which have lately originated by sudden mutations and which have not as yet spread far from their point of origin. Willis bases this extraordinary conclusion on the statement that the older a genus is the more species it possesses.

It will suffice to say that Willis's arguments have been riddled with destructive criticism which has descended on them from all quarters. It has been pointed out that a genus is a perfectly arbitrary conception totally incapable of definition, that its extent depends on whether the "lumpers" or "splitters" have been in the ascendant at its formation, and that its age is a highly speculative affair to which no palæobotanist will commit himself. Some of the special instances put forward by Willis have been shown to be entirely misleading. Thus he states that one species of Labiate [1] covers the lower grounds and rises to a certain height on a mountain, whilst a distinct but allied species occupies the summit. This latter he regards as a new species. Further investigation has shown that the supposed new species is closely allied to others occurring on distant mountain-tops, and that this group, so far from being new formations, are the remains of an older flora on the verge of extinction.

But in the light of what we know of the origin and distribution of local races, the reply to both Bateson and Willis is easy. "Pairs of species," whose territories adjoin, are not closely allied and have not arisen by a process of sudden mutation from a common ancestor, but are usually extreme terms in a process of variation, the intermediate terms of which are either extinct or are to be found in distant regions. Innumerable instances leap into the memory; a few are subjoined, taken from the special group studied by the writer. Thus the two common species of the starfish *Asterias* found in the English Channel are *A. vulgaris* and *A. glacialis*, which are extremely distinct from one another. The nearest ally of *A. rubens* is not *A. glacialis*, but is *A. vulgaris*, which is found on the east side of North America.

Two "sun-stars" with eleven to thirteen arms of the genus *Solaster* are found side by side in the Firth of Clyde. *Solaster papposus* has rather long tufts of spines and is of a colour which fluctuates between straw and orange. *Solaster endeca*, on the other hand, has shorter tufts of spines and is of a magnificent purple-red colour. Close allies of both species are found in the San Juan de Fuca Strait between Vancouver

[1] *Coleus barbatus* and *Coleus elongatus*. *Vide Age and Area*, p. 151.

Island and the mainland; and these Pacific species are separated from one another by much the same differences of colour as are the two Clyde representatives. The two common sea-urchins of British waters are the large organge-red *Echinus esculentus* and the much smaller green "burr" *E. miliaris.* The nearest ally of *E. miliaris* is *E. microtuberculatus* of the Mediterranean.

But again there is great difficulty in discriminating between races and species, and the test of mutual sterility has been applied in only a very few cases. The red grouse of Scotland and the willow grouse of the Continent have always been regarded as two distinct species. Yet when a Scotch landowner imported willow grouse into Scotland and allowed them to run wild, in two or three generations their progeny became indistinguishable from the red grouse; and when red grouse were taken to Norway, they reverted to the form of willowgrouse in a few generations. Part of this result may be attributed to intercrossing, but it is contrary to all "Mendelian principles" to assume that one race is dominant to another in Scotland and recessive to the same race in Norway.

Like Darwin, we may fairly assume that sterility is a by-product of increasing divergence of organisation, and like this divergence, that it is of all degrees of intensity.

But the doctrine of the survival of the fittest implies that all organs of an animal shall be useful—that is to say, shall have some function—and this also forms a ground of attack on the theory by Willis, who points out how apparently useless are some of the discriminating features of species. Thus he asks: How can some trifling peculiarity of the calyx of the flower of a tree determine its survival when the flower is only produced after the tree has lived thirty-six years and has successfully undergone its struggle for existence? It might be replied that the form of the calyx might have some close relation to the dispersal of the seeds or even to the fertilisation of the flower, and that this could not be ascertained unless a close study of the life of the tree had been made—*and in most cases this has not been done.* But if we recall the principle that all the features of an organism, whether plant or animal, are the outcome of its REGULATORY BALANCE—that is, of its reaction as a whole to the conditions in which it lives, to the heightening of some functions and to the inhibition of others—then it will be seen that we are under no obligation to explain the definite utility of every specific feature. Some of them are by-products or indirect results of the functions of other parts, and both the objection and its proper answer were given by Darwin, only what we have called regulatory balance he calls correlation of growth and compensation of growth.

Natural selection will, then, mean the survival of those individuals which respond most quickly and fully to the stimulus of the environment, that is, of those which are of a healthy and vigorous constitution. Darwin viewed as a great support of his theory the occurrence of embryonic rudiments such as the gill-slits of the embryo chick which resembled permanent structures in other forms. These he interpreted as due to the tendency of inheritance to produce corresponding structures at progressively earlier periods of life. Shortly after the publication of the first edition of the *Origin of Species* his leading continental supporter, Haeckel, enunciated his "Fundamental Law of Biogenesis," *viz.*, that the embryo in its development tends to "recapitulate" the history of the race. This law has been the inspiration of the study of embryology ever since his time. It has been criticised again and again; but nature is stronger than criticism, and its very opponents admit it in practice whilst denying it in theory, for every one of them, when seeking to explain mutual affinities, seeks eagerly for indications given by the structure of young forms. We have no space for a general discussion of the validity of the law, but we may briefly notice the objections raised by the latest critic, Prof. Garstang, which merely demonstrate that he does not understand it. Prof. Garstang, in a recent paper to the Linnean Society,[1] raises two objections: first, that in evolutionary progress, an animal does not grow up to adult life as one form, and then add on a new spell of life during which it becomes another form; and, secondly, that the supposed ancestral stage in the life-history does not *exactly* resemble the ancestor from which, on other grounds, we may suppose the present race to have been derived. But does Prof. Garstang seriously suppose that Haeckel did not see these two obvious objections? Indeed. the very word "recapitulate" is intended to indicate only a general resemblance between larva and ancestor, whilst it is sufficiently clear that progress in evolution would not necessarily mean increasing length of life. But what "recapitulation" records is the successive assumption of different habits and the corresponding structures by the race in its development, and the successful assumption of new habits can be accomplished only by young and plastic individuals, as every schoolmaster knows, so that the point at which the new life must be entered on is at the beginning, and not the completion, of adolescence; it is the *young adult* whose structure is "recapitulated," not the fully developed one. Once new habits have been acquired, they tend with constant and successful repetition to be more and more deeply engrained,

[1] "The Theory of Recapitulation: A Critical Restatement of the Biogenetic Law," *Jour. Linn. Soc.*, vol. xxxv, September 1922.

i.e. to appear ever sooner in the life-history, and the ancestral larval stages are correspondingly relegated to ever earlier periods of development.

To sum up, we find that in its general outlines the Darwinian theory of evolution is as securely based as ever it was, and that it has withstood all the assaults of "Mutationism" and "Mendelism," the relevancy of which to evolution has been given up by their principal exponents. "Natural selection" determining the survival of the functionally efficient, and inheritable variability bringing about changes in function and the structure related thereto—these are the corner-stones of Darwinism, and nothing has transpired which can shake them. We have pointed out that Darwin regarded "conditions of life," *i.e.* environment, as *the* cause of variability, and that he conceived of the environment as acting in two ways, *viz.* (1) indirectly by inducing a general plasticity, and (2) directly by alteration of habits and consequent modification of structure, and the inheritance of these.

The progress of research has thrown considerable doubt upon the action of the first cause; but Darwin's belief in the second cause has been vindicated, and it is seen to be the key to the riddle of evolution. In his great book, to which the *Origin of Species* was intended to be an introduction, *Variation of Animals and Plants under Domestication*, Darwin introduced his theory of Pangenesis to explain inherited variability. This theory, which has been ridiculed as altogether fanciful, supposed that "gemmules" were produced by every part of the body and thrown into the circulation and accumulated in the germ-cells, and so alterations of structure caused by the environment were handed on to the next generation. If we substitute for the word "gemmule" the word "hormone," is not this theory amply justified by the most recent work? Kammerer, indeed, has expressly accepted this theory as giving an explanation of his own results.

Doubtless Darwin was mistaken in drawing too close a parallel between the monstrosities of domesticated breeds and the adaptational peculiarities of wild races exposed to the fierce struggle for existence; but nothing would have horrified him more than the idea that every detail of the argument set forth in the *Origin of Species* should be regarded as the last word on a subject of which, as he himself saw, our knowledge was only beginning.

He would have welcomed the new knowledge, and so long, as Darwinists, we take our evolution from Darwin and not from Weismann, our position is impregnable.

THE EVOLUTION OF THE FUNCTIONS OF BLOOD

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At the present day it will hardly be denied that the most significant event in the history of biology during the last half-century was the rise of the conception of evolution. But perhaps the most interesting fact which presents itself, as one examines it, is the quite insignificant part which it has played in physiological thought. Fourteen years ago, Keith Lucas published a paper in this journal, entitled, "The Evolution of Animal Function," in which he discussed the probable reasons for this remarkable isolation of physiology from the evolutionary outlook. "The Evolution hypothesis," he said, "has not affected students of physiology, functional relationships have not influenced taxonomy; nor has the idea of evolution interpreted known facts of function. . . . The exclusion of functional considerations from classification has robbed physiology of that compelling stimulus which has made morphology a comparative science." The exclusion of phylogenetic considerations from physiology is certainly as much to be deplored from the physiologist's standpoint as from that of the zoologist, for the greatest ignorance exists at the present day as to the evolution of the various functions of the body. By the evolution of function is here meant, not the information to be gained by such studies as to the position or classification of any group of organisms, but the actual series of steps by which a function grew from its simplest to its most complex form. Respiration in the amœba is a very different thing from respiration in the mammal; but when did the differences arise and how did they develop?

Now this conception of physiological evolution includes two lines of development; that of minute structure both histological and chemical, and that of dynamic function in its strictest sense. Theoretically, the evolution of histological form should be dealt with by the morphologist, but it is so difficult to separate in thought minute form from function that such a proceeding is impossible. And, indeed, it is notice-

able that the morphologists, in their consideration of the evolution of animal form, have left histological structure almost entirely alone. For many years, they left histology altogether to the physiologists, but of late they have claimed it again as their province, refusing to remain mere anatomists concerned only with gross structure. It is difficult enough to separate histological form from function, but it is still more so if we descend further, to molecular and atomic dimensions. For example, we can hardly think of the chemical make-up of a nerve-cell without thinking of the work it has to do. In fact, at the level of the atom, the morphologist and the physiologist blend into one.

If, then, we propose to follow out some such line of thought as that sketched above, it will be well to give a detailed consideration to one particular physiological system; and the blood is suitable for this purpose, both on account of its great importance and because it, or an analogous system, is plainly traceable throughout the animal kingdom. But, owing to the greatness of such a subject, we shall not be able to do more than follow up certain aspects of its manifold activities. Let us begin with the red blood-corpuscle. We are to inquire if anything can be made out, with the facts at our disposal, concerning the history of the function of the erythrocyte, and firstly its statically functional aspect, *i.e.* the histological and chemical differentiation of its structure. (The word "erythrocyte" is here used to mean the red blood-corpuscle itself, and not its progenitor in the marrow or elsewhere.) It may be said that we are in possession of certain facts indicating directions of this evolution, if nothing more.

In the first place, the underlying assumption of the view here propounded, or rather, supported, is that in the erythrocyte we have to deal with a case of symbiosis. It is not, of course, intended to imply that this is a case of symbiosis at the present day, but simply that indications exist that the ancestors of what we know to-day as the red blood-corpuscle entered the multicellular organism, to live symbiotically with it, in the days before animal life came to live on land. The mechanism of symbiosis has been worked out in detail by Keeble on two small planarian worms, *Convoluta paradoxa* and *Convoluta roscoffensis*. It seems that the driving power behind the symbiosis which they show is the nitrogen-hunger occurring in the sea. Small algal cells, possessing in one case Chlorophyll, are driven, by the difficulty of procuring enough nitrogenous material for their needs from the sea, to live symbiotically within these planarians. They feed on the end-products of the nitrogenous metabolism of the larger party to the contract, and, in return, supply it with carbohydrate produced

by their photo-synthetic activity. Now the working hypothesis here put forward is that at some stage in the development of the vertebrate a symbiotic union occurred between it and certain freely swimming unicellular organisms, destined eventually to give rise to the erythrocytes of the blood. Possibly the entering cells were of a kind not now in existence, though perhaps there is no reason for thinking them to have been any other than ordinary amœboid, probably ciliate protozoa.

Excellent evidence exists for the thesis that all animal pigments were originally excreta. In all probability, pigments either are now, or were once, resistant to the metabolism, possessing properties of protection against the processes tending to break them up. Quite *à priori*, one might well suppose that all animal pigments were originally end-products of different lines of metabolism having no function and only acquiring one after a considerable time. But, as a matter of fact, such processes can be seen going on at the present day. The Melanins, for example, are almost certainly produced as the end-products of one particular kind of metabolic treatment of Tyrosin and other aromatic amino-acids, and it has been shown that their deposition in the negro's skin is really a manner of excretion. As another example, one might adduce the case of Turacin, a copper-containing pigment found in the coloured feathers of certain birds. A case which more obviously illustrates the excretory significance of pigment-production is that of the cabbage-white butterfly. It was found by Hopkins in 1894 that the white pigment in the scales of *Pieris brassicæ*, and the yellow pigment under its wings were both derivatives of uric acid.

The relevancy of this conception of pigments as once excreta is seen if we consider the unicellular organism driven by nitrogen-hunger, as it enters into symbiotic relationship with the invertebrate ancestor of the higher animals. One of the substances it would naturally ingest would be Hæmoglobin or more probably some ancestral form of it present at the time, whereupon would be formed the prototype of the modern erythrocyte.

Other evidence supports this view. The majority of invertebrates have nothing corresponding to the vertebrate red blood-corpuscle: what respiratory or other pigments are found in them are present freely dissolved in the vascular fluid. In some cases it is said that the respiratory needs of the animal are satisfied by a colourless protein circulating in the plasma. This lack of red blood-corpuscles should be correlated with the fact which we gain from embryology; namely, that the heart of the vertebrate begins to beat some time before the

blood contains any formed elements. For a time it is filled with plasma alone, but eventually the erythrocytes are formed from their precursors and true blood is produced. Further, Maximoff has shown that the appearance of the lymphocytes precedes that of the erythrocytes by a considerable interval. The Law of Recapitulation leads us to contrast this with the invertebrate condition. But a further fact must not be overlooked. The manufacture of the red blood-corpuscles by the erythroblasts and megalocytes of the bone-marrow, which goes on throughout life, produces a cell containing its proper share of Hæmoglobin; but also in possession of a nucleus. At varying periods after it is produced, the nucleus disappears; presumably the cell extrudes or autolyses it. Perhaps we have here another indication of what the ancestor of the erythrocyte looked like: it had a nucleus; so have modern red blood-cells, but only for the first part of their lives. Now Lillie found that the Indophenol test for oxidative activity was given much more strongly by nucleated than by non-nucleated erythrocytes. And this may be correlated with the fact that the auto-oxidation of blood containing nucleated corpuscles is much greater than in blood containing non-nucleated ones, so that in all probability the degeneration of the erythrocyte brings with it a diminution, if not a cessation, of individual metabolism. Now to return a moment to the Planarians. Symbiosis is seen in them to be a very efficient system, for the worm supplies the algal cells with the nitrogen which they could not obtain in the sea, and they in turn provide their host with carbohydrate nutriment. But it is important to notice here that after a time the exchange usually becomes insufficiently rapid, and the worm has either to fend for itself as it did before the algal cells came upon the scene, or to feed on the algal cells themselves. It chooses the latter course and thereby commits suicide. One wonders if any parallel could be drawn between the degeneration and absorption of the algal cell in the planarian worm and the degeneration and loss of nucleus on the part of the red blood-corpuscles.

Another fact bears on this point. Oliver showed in 1914 that, given the correct conditions, namely, prolonged survival in ·85 per cent. NaCl solution, erythrocytes will show amœboid movement, and will put forth ciliate and flagellate processes by means of which they will become actively motile. This has not apparently been confirmed, however.

Since we are considering the evolution of the chemical as well as the histological structure of blood, it is important to deal next with the origin of Hæmoglobin and other related pigments. This is a subject about which our present knowledge is appallingly fragmentary, and it is only within very

recent years that we have come to know anything about Hæmoglobin itself. Perhaps the reason for this is that the early workers in this domain were either zoologists or physiologists of the older school, possessing only a very slight acquaintance with chemistry in general and biochemistry in particular. Perhaps, also, it is true to say that researches on this subject were held up for lack of efficient methods and technique. Whatever the reasons, the fact remains that this branch of study is in a bad way at present. On the above hypothesis, the inclusion of respiratory pigment in the symbiotic ancestors of the erythrocyte occurred much later, in all probability, than its first appearance. In what localities, then, is Hæmoglobin and its relations found among the lower animals ?

A red pigment, Helicorubin, has been found in the bile of that museum of curiosities, the snail *Helix pomatia*; it is a relation of Hæmoglobin though with a simpler molecule. Hæmatoporphyrin is found in the skin of certain worms and in that of the slugs, *Limax variegatus*, and *Arion ater* ; it also occurs in the salivary glands of certain members of the snail group. In addition it has a very wide distribution among the marine invertebrates, and in most cases it almost certainly has no respiratory function. Other pigments of probably near related chemical constitution are the Chlorocruotins, Echinochromes, and Histohæmatins, as well as Hæmocyanin, Myohæmatin, Neurohæmatin, and Hæmerythrin. It would not be difficult to extend this list, but what is most remarkable about these pigments is their extraordinarily sporadic distribution among forms, and their apparently haphazard distribution in the organisms themselves. As regards the former aspect, there seems to be no law governing their appearance ; any one of them may be found in a few of the members of one genus and then only very far away again in the members of some totally unrelated genus. All the species of one genus may have different pigments. As an example of this one might take the Echinodermata, among which the following pigments are all to be found : Echinochrome, Hæmatoporphyrin, Hæmochromogen, and Hæmoglobin itself. With regard to the second peculiarity of distribution, the differences are very strange indeed. Pigments may occur free in the vascular fluid, or in the muscular tissue, nervous tissue, anywhere and everywhere, in fact. The skin, it is to be noted, is a favourite place. A significant observation is that in the Diptera, Hymenoptera, and Lepidoptera, those insects which are known to be most active in the use of their muscles, contain the greatest quantity of Myohæmatin. Von Fürth alleges another example of this, the actively boring *Solen*,

and is inclined to agree with Ray Lankester's view that the presence of Hæmoglobin and its relations in such cases is due to the need for efficient oxidation under such conditions as poor circulatory systems in stagnant ponds. The other hypothesis is that the pigment appears as a side-phenomenon of some particular line of metabolism (perhaps that of Proline) and in greatest quantity wherever that is most active. The oxidation-necessity theory is not very satisfactory, because of the many examples of pigment containing forms which neither are very active nor live at the bottom of stagnant ponds. And it is also rather difficult to put much weight on the greater quantity of pigment occurring in active animals, because of the case of some which contain a great amount of Neurohæmatin in their nervous system; we should have to conclude that these animals were given to hard thinking! On the whole, the theory which best fits the facts is that these pigments are end-products of the metabolism, which at first are functionless, and resistant to it. The organism may leave them where they are formed, or it may excrete them into its skin.

The distribution of Hæmoglobin itself is important, as it seems to be the final form of this class of pigments. Among invertebrates, its distribution is wide, and like its relations, is to be found in every kind of tissue. In the saccular fluid of the annelids, *Lumericus eunice* and *Hirudo*, it is found freely dissolved; as in the perivascular fluid of many molluscs and lower crustacea. It is found free in the blood-plasma of *Daphnia*, *Cheirocephalus diaphaneus*, *Planorbis corneus*, and the larvæ of *Cheironomus plumosus* and *Musca domestica*. Wherever it occurs in invertebrate blood, it is found free in the plasma, with only four exceptions, *Solen*, *Glycera*, *Capitella*, and *Phoronis*. In their bloods, it occurs in nucleated corpuscles. In other invertebrate bloods, where it is free, colourless nucleated cells are also found, resembling erythrocytes in their non-amœboid nature. This condition is also met with in *Leptocephalus*, which, with *Amphioxus*, forms the only known exception to the rule that in vertebrates the respiratory pigment is always contained in cells and never free in the vascular fluid.

The position can now be summed up as follows. It is probable that the ancestral forms of Hæmoglobin were end-products of the metabolism, subserving at first no respiratory function, though they may have done so subsequently, before the symbiotic invasion. This is not to say that they were of no other utility. The subject of protective mimicry, and similar artifices is too large a subject to do more than mention here; perhaps it may account for the preserved production

of these pigments. Then before the vascular system of the invertebrate closed up and lost connection with the external medium of the sea, unicellular organisms of some kind were drawn into symbiotic union with it by reason of the nitrogen-hunger in the sea. Amongst the nitrogenous end-products which they ingested, the pigments were included, whereupon there was produced the ancestor of the modern erythrocyte, and the potentiality of a gas-transporting mechanism of marvellous efficiency. A point which indicates the possibility of this ought to be mentioned here. Macallum has shown that the salt-content of the plasma of present-day vertebrates is, with certain reservations, identical with that of the sea-water of pre-Cambrian times. At the end of the Cambrian period, when vertebrates with a closed circulatory system took to the land, they took with them blood-plasma of the same composition, as regards salt-content, as the sea-water they had left.

The possibility that pigments were once resistant to the metabolism involves a few further considerations. The habit of excreting pigments into the skin forms a notable tendency throughout the whole subject. But the space available for such disposal of excreta would naturally not be unlimited, and at some time or another the organism may have been faced with the problem of how to get rid of its surplus pigment. In such circumstances, metabolic machinery may have been evolved for dealing with coloured substances which previously had been immune from further catabolism. Certainly, in the higher animals studied by the physiologist there is a small though continuous catabolism of respiratory pigment, and perhaps the reason for its smallness is that the pigment is almost as much out of the way of the metabolism in the blood as if it were in the skin.. A point which ought to receive more emphasis than it does is that, to all intents and purposes, the blood is outside the body. The organism takes the most minute precautions to prevent it coming into contact with the tissues ; even the smallest arterioles and capillaries have walls which the erythrocytes may not pass through. The only places where the circulatory system really does blend with the tissues are the spleen, the hæmolymph glands, and the bone-marrow. And these, be it remarked, are the very places where manufacture and destruction of the red blood-corpuscle—and therefore synthesis and catabolism of Hæmoglobin—are actively going on. It looks as if Hæmoglobin was well protected from metabolic dangers when in the blood, and free to carry out its respiratory duties ; but as soon as it came into contact with the tissues it also came into contact with the metabolism and was broken down. Possibly the steady

catabolism of Hæmoglobin is a relic of the time when it had no respiratory significance, and was a burden to the organism. Possibly, also, this isolation of the blood from the tissues is a relic of the time when the circulatory system, or its prototype, was open to the exterior. And there is always the possibility that the invading cells brought pigment with them, though this is very unlikely, in view of the fact that pigments of this sort are not found as low down as the protozoa in the scale of life. In the case of the planarian the algal cells bring their chlorophyll with them.

So far we have been considering only the evolution of the statical aspect of the hæmato-respiratory function. It will be interesting to apply the evolutionary outlook to its function considered dynamically.

Considerations of the physical nature of protoplasm, such as its surface energy and internal friction, show that the size of a unicellular organism is strictly limited, and therefore, if any advance is to be made on the protozoal condition, the cells must cease to compete and begin to co-operate. This implies the rise of colonies of cells, surrendering the advantages of independence to the greater advantages of cohesion, and can be seen at the present day in such organisms as *Carchesium polypinum*, *Eudorina elegans*, or *Magosphæra planula*. This metazoal stage is a true cell-republic; each of the individuals is capable of leaving the assembly and leading an independent life. The more that differentiation and specialisation proceed, the less is this possible. Now, when the organism has reached a certain size, the cells in the interior of its body will be subjected to an entirely different environment from those on the surface. Two ways of overcoming this difficulty are seen in *Spirogyra* and the *Confervæ* respectively: the former arranges its cells in long threads, the latter in flat masses. These methods, however, were not of universal application, so it came about that the cells in the interior became functionally modified from those on the surface, thus, perhaps, laying the foundations of specialisation. But, as the organism grew in size, some mechanism became essential whereby the cells in the interior could get the oxygen and foodstuffs necessary for maintenance of life. Processes of diffusion through the outer cells which had sufficed for smaller organisms were found to be of no use whatever for larger cell-communities. And simultaneously with these demands there grew up the need for some co-ordinative mechanism, by which the cells at one part of the body could communicate with and influence cells at other parts of the body. The more differentiation and specialisation proceeded, the more urgently necessary became a co-ordinating mechanism, and, later, a centralisation of administration. In

Eudorina, for example, very little communication between the cells was needed, and what was necessary was probably carried out by diffusion of chemical substances from cell to cell.

Simultaneously with these needs, there had been growing up, in connection with other requirements, a body-cavity, such as the Cœlomata present to-day, in more advanced forms. This cœlum gradually came to be enclosed from the exterior, and out of it there grew a tubular system containing cœlomic fluid, the precursor of the plasma. Later, contractile chambers in the tubes gave rise to a circulation. If the erythrocyte infection took place, it took place before the closure of this system.

The arrangement of a blood-system at once supplied the solution to all the problems with which the organism was faced. By reason of the pigments, into the history of which we have already gone, and the precursors of the red blood-corpuscles, it was provided with a mechanism for gas-transport of maximum efficiency, ready to be brought into operation as soon as the increasing size and complexity of the organism should demand it. Then, from the nutrient point of view, it placed the buried cells in full possession of their food-supply. But, as well as all this, the formation of the blood also supplied an administrative system which brought all the parts of the body into co-ordination ; the nervous system was a later development ; not until later were special paths of protoplasm differentiated along which physical changes could be transmitted to produce peripheral effects. Those chemical mechanisms of co-ordination, which the highest organisms have not wholly abandoned, were then the only means at the disposal of the animal.

It has been suggested that in the sympathomimetic action of Adrenalin we see the sympathetic nervous system (itself perhaps older than the central nervous system) taking over the functions once entirely performed by Adrenalin. Another point in connection with the evolution of the hormonic function of blood is possibly not irrelevant here. The majority of the endocrine glands seem to have a close connection with the gut. The Pituitary is partially developed from the buccal ectoderm, the Parathyroids, Thyroid, and Thymus are derived from the gill-clefts, and the Suprarenal glands have at least an indirect connection with the gut. All are known to have originally possessed ducts, some of the vestiges of which remain ; these opened into the intestine, as the liver and pancreas do now—two glands which are also glands of internal secretion. In view of the close connection between cœlom and circulatory system, these

facts are of some interest, though their meaning is anything but clear.

Further research on invertebrate blood might furnish us with indications as to whether the hormonic preceded the respiratory function of blood in evolutionary time, or vice versa.

If it be granted that the symbiotic origin of the erythrocyte is a possibility a very great difficulty is the perpetuation of the red blood-cells in succeeding generations of hosts. In so typical a case of symbiosis as that of the planarian worms, reinfection by the algæ is a necessity for each generation. How could the erythrocyte type of cell have been perpetuated from an infected organism to its descendants? As far as one can see, the only answer to this very just criticism is that, as long as the organism inhabited the sea, with an open circulatory system, reinfection probably took place. But when that became impossible, owing to the conditions of life on land, the long-accustomed presence of the red blood-cells must have influenced the germ-plasm which was to produce the immediate offspring in such a way that in its development cells were set apart for the work—a process made all the more imperative by the respiratory needs of land life. At any rate, the symbiotic hypothesis does explain how the Hæmoglobin got inside the erythrocyte.

Another criticism, of a different kind, but no less just, is that the whole treatment of the problem is too speculative to be of any value. Its speculative nature is freely acknowledged, and it is rather to be considered as an attempt to illustrate the kind of line along which a section of modern biochemical thought might profitably go. Physiology and the evolutionary outlook have much to gain from one another. The study of function must not be limited to the examination of its expression in man, or that "animal kingdom of the physiologist," the cat, the frog, the dog, and the rabbit. Its ultimate physicochemical analysis must, of course, be carried on in whatever tissue or organism best suits the methods employed; but the problem of Life is wider than that, for it has a past as well as a present. A question of terminology brings this out in an oddly appropriate way. Morphology is, in its widest sense, "the science of the structure of living things, and all that that implies"; but as usually used it means, "the study of structure in relation to its development and expression in any given organism." Anatomy (leaving apart etymological considerations) merely means "the description of facts gained through dissection." Similarly, physiology is "the description of facts gained through experiment and observation." With "the study of function in relation to its development

and expression in any given organism," there is nothing at present to correspond.

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THE FUTURE MOULDED BY SCIENCE: A VISION

Translated from the French of MARCEL SEMBAT, with an Introductory Note by Sir J. G. FRAZER, F.R.S., D.C.L., LL.D.

[*Introductory Note.*—The author of this paper, Marcel Sembat, was an eminent French politician of the Socialist party and represented one of the Montmartre divisions of Paris in the Chamber of Deputies. A man of wide culture, both literary and artistic, he was passionately and disinterestedly devoted to the highest aims, to the advancement of knowledge and to the progress of humanity. Widely read in English as well as French literature, he was deeply interested in philosophical questions and animated by a firm faith in science which he conceived to be destined to exercise the most commanding and beneficent influence on the future of our species. To this faith the fragment, of which an English translation is here offered to readers of Science Progress, bears eloquent testimony. It was found among his papers after his sudden and tragic death at Chamonix in September 1922, and it was read at an impressive commemoration ceremony, which took place on Montmartre in December 1922 and was attended by an immense assembly of his friends, constituents, and representatives of the Socialist party. The ceremony was a double commemoration; for his devoted wife, herself an artist of high distinction, had refused to survive him and had shot herself on the day of his death, after making all her dispositions with the most perfect calm and self-possession. They had been a most loving and happy couple in life, and in death they were not divided. They were buried, amid public demonstrations of sorrow, side by side in the same grave at Bonnières on the Seine, where he had been born, and where he always kept his old family home, to which they regularly retired every week for rest and refreshment from the bustle and fatigue of their busy life in Paris. There they found their principal recreation in long country walks, which they took together in the beautiful scenery of hill and river, endeared to them by its own intrinsic charm and by happy

memories of youth; for she also, though not born there, had been brought up at Bonnières, and it was there that they had first known each other. Their deep love of nature led them in later life to buy a small house at Chamonix, where they spent many happy months, making ascensions of the mountains together; for they were seldom separate except when he was at his duties in the Chamber. I was privileged to know them both intimately and to enjoy their hospitality once at Bonnières and often at their home on Montmartre, where Madame Sembat had a studio, and where on summer days they loved to sit with friends under the shade of trees in their small but secluded garden. He was a delightful talker, his conversation ranging easily over a variety of literary and other topics, and constantly lit up by a play of humour, always gentle and kindly, never harsh or sardonic. During the four years or so that I knew him he never once obtruded his own socialistic views on me; indeed, I do not know what precisely they were, for we never discussed them. We had ample subjects of interest in common without broaching these controversial questions. Nor did his socialism interfere for a moment with his patriotism as a loyal and devoted Frenchman. He rallied to the side of the Government throughout the war and served for a short time in it as a Cabinet Minister; and when, after the peace, the extreme section of the Socialist party accepted the Bolshevist programme, he publicly broke with them. He has left behind him a considerable body of manuscripts, out of which his nephew, M. André Varagnac, expects to publish enough to form several volumes. These, it is hoped, will serve to refresh and prolong his memory among those whose privilege it was to know him, and will convey some idea of what he was to those who never met him. The fragment here presented to the reader is eminently characteristic of the man—of his fervent idealism, of his imagination tinged with poetic ardour, of his unshaken faith in the high destiny of humanity, of his ardent admiration and love of whatever is good, and beautiful, and true.]

SCIENCE must needs go on. We men were born but the day before yesterday. Yes, and the day before yesterday is still too soon. In the seventeenth century? you ask. But you know well enough that I speak not now of Louis XIV nor of Racine. The masterpieces of literature, Versailles, and all that, are matters of indifference to us. The important thing is the beginning of the scientific movement. That was what brought us to the birth. Mechanics, physics, chemistry, electricity, and the instruments, the microscope, the telescope—you would have us remember Kepler and Copernicus? I

am content. In that case our birth must be dated a little earlier. Be it so! The important thing, you see, is the contrast. Versailles, and the tragedies of Racine, and the oratory of Bossuet, why do we put them aside? Because all that existed before; it existed in the age of Pericles. And besides it is finished, it is dead. Therefore it is mortal.

Whereas science is a glimmer of eternity, a glimmer of the dawn. That is a thing unheard of. It had never existed before. And now it appears before our eyes; it promises to last for humanity. A chance is offered us—ah, still uncertain, hazardous, but yet a chance! in all this ephemeral world a glimmer of the eternal!

You understand? Choose, for example, the great philosophic outburst of Kant, Fichte, Schelling, Hegel, Schopenhauer. What brains! With them the world remade all its ideas. Immense, was it not? But was the intellectual impulse less in the India of the Vedantas? and in the Alexandria of Proclus and the Gnostics? And Greece rears for us a whole chain of mountains, dominated by two peaks, Plato and Aristotle, summits that have never been surpassed.

Yet Greece has perished and is gone. Hindoo philosophy has ceased also. To-morrow, if there were none to save us but Kant and Hegel, we too would disappear in our turn.

But we shall not disappear.

All has existed—splendours of literature and splendours of philosophy—painters whom we no longer attempt to equal, Tintoretto, Rembrandt, the epics of Homer, and the trinity of Æschylus, Sophocles, and Euripides. All has existed, and all has disappeared, and all has begun again.

On the contrary—a violent contrast—the scientific movement, prepared from the time of Chaldea, of Egypt, of Greece, began in the seventeenth century. It is no longer a question of such and such a science, as geometry or astronomy. It is a question of a sheaf, of a confluence of sciences, which all support each other and push each other on.

Renan spoke of a firmament of blue crystal, of a great vault stretching to infinity. In this vault—the canopy of the universe—there is but one aperture, a solitary little aperture no larger than the palm of the hand, and a butterfly flutters and beats its wings against the crystal of the infinite vault. You think it will never find the aperture? Wait! The butterfly is eternal.

Humanity is not sure of being eternal. It believes itself sure of the opposite. Has it twenty thousand years behind it? and how many in front of it? The glaciers will return. Herbert Spencer spoke of a thick cap of ice that will yet cover

London and England. The sun will one day be extinct. We are not the eternal butterfly.

Well, we have nevertheless found the narrow passage. We shall pass to the other side of the vault. We are passing now.

Science does for men what neither philosophy nor literature could do. It gives us power. In proportion as it grows, our power increases. If its growth has no limits, our power has no limits.

I know, of course, there is the possible catastrophe.

But there will be no catastrophe. I wager there will not. I made Pascal's wager in the bottom of my heart; but nay! it was made of itself in me since eternity. There will be no catastrophe.

You will not accept the wager? You argue? Be it so, then let us *suppose* that the catastrophe will not take place, and that men have before them—oh, not so very much. So little time is needed nowadays. Three centuries have sufficed to bring us to the point where we stand. Three centuries will suffice to free us from the earth.

Three centuries have sufficed? Nay, a single century sufficed. It was in the nineteenth century that they laboured, and now, the express once started, we are travelling at ever increasing speed. Whither? Listen. In three centuries we shall be able to quit, if need be, the planet. We shall have colonies elsewhere. We shall have learned to traverse the interplanetary spaces and to scour the universe.

Then what matters the earth and all that happens to it? The race of men, the race of conscious beings, who speak and know, will be free to choose, instead of it, the point of the universe whereon to lay (provisionally, for a hundred thousand years) the courses of the work that is carried on.

One thing alone interests me: it is that which endures, which is eternal. For myself, I shall die in ten years, in ten days. Yet I am not like one of those good folk who are plunged in grief when they are told that the sun will be burnt out in a hundred thousand years. Oh, indeed? the sun will be burnt out? But that will be terrible! then there will be darkness on the earth!

But you will be dead and buried long before that. So much the worse. What matter? They suffer at the thought of it to-day.

For myself I am happy from to-day at the thought of the conquest of eternity by man, at the thought of the conquest of eternity by man to-morrow, after my death. Nothing interests me which does not endure. I am in love with the forever.

Why? In each of us there is an individual self and a

group self. There is the social element in me beside the personal. There is a communion which, on the social side, links me to all that men have done since there have been human tribes and languages, and which links me to all that men will do.

Since my twentieth year that is the dream which has lit my path—a future moulded by science—a humanity triumphant, that extends its dominion and its mastery without limit and without end.

What will men do? Humanity has its programme marked out. By what? By the myths, by the religions! All that our tribes have thus projected above themselves and deified, is the depth of our own souls, the quintessence of our desires.

The last of these myths, the living residue of these religions, is the projection by humanity of a deity into the sky, a deity eternal, who has created everything, who knows everything, who can do everything.

That is the programme. Naturally it is modified in the application. It is always necessary, says Maurice Barrès, to abate something of our dreams.

Who knows? There are moments when I feel that our wildest dreams will be surpassed. What? After all, these dreams are only the dreams of savages. Has not the reality of to-day—the railway, the aeroplane—already surpassed the imagination of primitive man? In three centuries our dreams will be petty and childish beside the realities.

But let it be. We shall not be gods all at once; yet one thing is certain, that the greater our knowledge; the greater will be our power. Practically we are acquiring omnipotence. God is coming into being. We are in process of creating Him.

Our existing horizons are cracking.

POPULAR SCIENCE

POLITICS AND SANITATION

By Colonel W. G. King, C.I.E., I.M.S. (ret.), M.B., D.P.H.

Mens sana in corpore sano may be trite, but is a truism which the modern psychologist and the politician alike admit ; politics cannot disregard political economy, nor political economy sanitation in its broadest sense—as applicable to man and beast. The success of politics depends on the making and enforcement of appropriate laws, whether these be applicable to observance of the Eighth Commandment by individuals, or the protection of continents from avaricious nations. If sanitation be an integral part of politics, equally must laws be evolved for restraint of the selfish instincts of individuals which endanger the health of their neighbours, and their corollary—multiples of communities and nations.

When individuals of Western nations find themselves irritated by legal restraint in the interests of sanitation, the public opinion of the average community recognises that the minority may reasonably be required to suffer inconvenience in the interests of the majority.

In India, however, where various races possess heterogeneous doctrines ruling relations with their fellow-beings, which must be provided for by harmonious blending in the warp and woof of administration, there is demanded of the politician not only recognition in practice of the broad truths of these aphorisms, but the faculty of judging where and when each suggested departure from limits inculcated by caste or creed is likely to be tolerated. Study of the tenets involved engenders useful discrimination ; but, even when thus forearmed, opposition to reforms—the result of ignorant interpretation of suggested sanitary methods and of the doctrines relied upon in support of their rejection by the more conservative members of an Indian community—may be encountered. Enforcement of the most well-meant measure of philanthropy, which cannot be defended as in harmony with these doctrines, risks either the flight of a panic-stricken population or its resort to mob-law.

Trained primarily as administrators of Districts of huge areas in immediate contact with Indian populations, selected members of the Indian Civil Service ultimately fulfil the rôle of politicians in the Indian Empire, under the Imperial and Local Governments. A proportion of the officers of the Indian Medical Service, detached from military duty for the purpose, has from the early days of the Honourable East Company provided them with sanitary advisers.

The ruling of the India Office by party government, and through it of the Government of India, has not favoured a continuity of policy. It has, however, never been possible to neutralise absolutely the demands of sanitation; "globe trotters," meddlers in statistics, and Press notices would at intervals furnish disagreeable tales of mortality, which could not be ignored when fostered by a Member of Parliament at a loss for "Questions." To meet such spasmodic symptoms of interest, it has sufficed for the Government of India to report that great schemes for improved sanitation were "under consideration"—a stage of progress non-committal and, as history shows, indisputable—and with hopes expressed of a bright future, an extension of the period of official equanimity has been obtainable.

At the present moment, there is reason to believe that the sanitary function of the Indian Medical Service as applied to the civil population may cease; hence, at the parting of the ways, it seems desirable to note what progress has been made up to date by this Coalition—representing Politics and Sanitary Science. To do this in detail would demand the writing of volumes, but a sufficient knowledge of conditions may be gained by a review of the more important results. If, therefore, it be found necessary to judge whether the Indian Medical Service might have influenced progress more, or the Indian Civil Service have exhibited a firmer belief in life-saving measures, there should be held in mind not only the inhibiting factors just referred to, but their repercussion on the psychology of both advisers and advised. Much progress has undoubtedly been made in spite of many difficulties, but, as a result of these, it has been of a halting nature.

In 1859 the heavy mortality of the troops in India awakened attention in Great Britain. A Royal Commission was accordingly "appointed to inquire into the sanitary state of the Army in India," which issued a Report in 1863. Information gathered by the Commission showed that the mortality of the European Army was 69, and that of the Indian Army, probably, not less than 20 in 1,000; that officers with British and Indian troops died at the rate of 38 per mille per annum; that expectation of life at 20 years for male Europeans resident in

India was shortened by 21·8 years, as compared with the rate established by Life Tables[1] for lives in England; that the annual death-rate for European women "in married quarters" varied from 44 to 276 per mille; that, taking Calcutta as affording a representative Indian population, statistics compiled by Dr. Strong for a period of 11 years disclosed an annual death-rate fluctuating between 37 and 81 per mille; that 23 per cent. of deaths in Calcutta were due to small-pox; and that in civil jails the annual death-rate varied from 84 to 120 per mille.

Under orders by the Secretary of State for India, the first stage of the reforms advised was put into effect in 1864, by the appointment of Sanitary Commissions for Madras, Bengal, and Bombay. The Commissions of Madras and Bengal[2] at once advised the establishment of Public Health Services *throughout Municipal and District Board areas.* The first act of these Commissions was, therefore, to expound the fact that, in dealing with a free population, necessary information as to the presence of disease can only be ascertained and be correctly utilised by permanently embodied and technically trained staffs (constituting a Public Health Service) in direct contact with the people; that to attempt to combat disease by staffs hastily assembled when epidemics arise may impress the "gallery" but is futile, and that to expect the death penalty of disease to be thwarted in the 1,152,000 square miles of India by making Municipalities the sole points of defence equally would be so.

In both Presidencies, the recommendation for the formation of a Public Health Service was pigeon-holed, with the intimation in one case that funds must be looked for from provincial sources. Nevertheless, the Madras Commission did excellent work in drawing up plans for new barracks and jails and for remedying the water-supply, drainage, and structural defects of those existing. The sanitary care of festivals also was undertaken. *Within the short period of four years, the saving of life effected by these various measures was readily demonstrable.*

Sanitary Commissions possessed of this amount of vitality,

[1] According to Dr. Farr.

[2] The first annual Report of the Bengal Sanitary Commission (1864–5) contained the following statement: "We have recommended the establishment in all Districts of Local Boards for carrying on the Municipal and Sanitary Service, the appointment of Health Officers and the adoption of other measures for a proper supervision *throughout the country* [italics not in the original] of the sanitary condition of the people. The proposals we have made are now under the consideration of Government." The proposals by the Madras Sanitary Commission embodied in the so-called Ellis scheme—after the name of the President—also dealt with both Municipal and District Board areas.

however, could not be tolerated by politics; not only did they represent an "unproductive" Department, but threatened increased expenditure both from Local and Imperial funds. Moreover, they added to the work and responsibility of District officials, and foreshadowed trouble on caste and creed problems. A policy of devolution was thereupon inaugurated by the Government of India, *which became traditional with successive Governments.* Faced with caste and creed problems, they early sought "the line of least resistance," embodied in the belief that the desire for sanitary advance would follow the acceptance of education. The Education Department responded by providing University graduates—latterly at the rate of about 2,000 per annum. Neither primary education nor the education of women received the proportionate attention necessary; and the mother-in-law still defends social customs leading to penury, whilst caste and widowhood disabilities, which condemn thousands to degradation of mind and body, still exist.[1] In the presence of undeveloped resources of the country, and the consequent lack in industry and commerce of suitable employment for skilled intellects, this education obsession of the Government of India has reacted upon local politics on lines which are self-evident in to-day's position in India.

In 1866, in accordance with a recommendation made by the Government of India to the Secretary of State for India (for which sanction was not awaited), in each of the Commissions four members were dispensed with, and only one member—a medical officer—was retained as Secretary. By 1869 Presidents of the Commissions (who drew the desirable pay equivalent to that of Judges of Indian Civil Service origin) were substituted by a medical officer—thereafter styled the Sanitary Commissioner—aided by a second as Secretary. In the same year, on the death of the Secretary attached to the Sanitary Commissioner of the Madras Presidency, the appointment was abolished; and, subsequently, the same tenuity of the personnel of the carefully constituted Commissions of 1864 was secured in the other Presidencies.

In 1869, transfer of the appointment of Sanitary Commissioner from the Military to the Civil Branch of Government was effected. Up to 1886 the Sanitary Commissioner held the rank of Deputy-Surgeon-General (corresponding to the present rank of Colonel) when—covered by an "axe" committee—the Indian Medical Service was deprived of one of its administrative appointments, and the status of the Sanitary Commissioner was lowered; it was asserted that an officer

[1] Under the caste system over thirty millions of Indians are classed as "untouchables."

of the rank of Major or Lieutenant-Colonel would suffice for the duty expected.

Subsequent events demonstrated that, whilst the position as heads of Departments directly under Governments—albeit solely advisory in function—when held by Sanitary Commissioners of administrative rank, could be tolerated, so unorthodox a condition as connoted by the holding of independent appointments within the areas of their commands by officers many years their juniors, was more than the autocratic tendencies of Surgeon-Generals possessed of a leaven of militarism could bear with resignation.

By 1895–6 a Surgeon-General with the Government of India officially advocated that officers of the rank of Captain, who should serve under the Surgeon-Generals of the areas concerned, would suffice for the duties of Sanitary Commissioners. His proposal was rejected by the Madras and Bombay Governments, but was accepted by that of Bengal, where it was carried into effect in 1898. The efficiency of the system involved of allowing the opinion of officers dealing with curative medicine to dominate those who had made sanitation their speciality was shortly put to a severe test. The Surgeon-General with the Government of India who had made the proposal dealt with plague on its introduction to the city of Bombay, and, if records of the period are to be trusted, had but a dim remembrance that a Sanitary Commissioner for the Government of Bombay existed. How far India is to be congratulated on his acting on the assumption that sanitarians are born, not made, is a matter of opinion; the facts evidently did not appeal to the Indian Plague Commission of 1898–9, which explicitly advised a return to independence of Sanitary Commissioners. Nor did it appeal, as a result of later experience, to the Government of Bengal (Sir Andrew Fraser being their Governor) nor to the Government of India, both of which, in 1904, agreed that their Sanitary Commissioners should be directly subordinate to them, and "not to the Medical Department."

In 1911, on the death of the officer holding the appointment of Sanitary Commissioner with the Government of India,[1] the then Surgeon-General with that Government (by this time styled the Director-General of the Indian Medical Service) pursued the policy of his predecessor of 1896, and after failing in the effort to secure a complete abolition of the

[1] This is again in contemplation, in accord with the advice of the Inchcape Commission. If this be accepted, the Central Government of what is rightly styled an Empire will revert to conditions found in Great Britain in 1830—the Lords of the Council dealt with sanitation without the advice of a Sanitary specialist. An epidemic of cholera awoke the nation to the fact that international politics should embrace Public Health problems.

appointment, succeeded in rendering the future Sanitary Commissioner subordinate to him, and in wresting from him the right of protection of research in the interests of Preventive Medicine. As was possibly anticipated, after this event any semblance of independent action by Sanitary Commissioners with Local Governments, who are now known as Directors of Public Health (a terribly ironic title unless it were possible, as is not yet the case, to add the word "Service"), has become nominal. Under the arrangements made, far from the Sanitary Department being considered as of sufficient importance in administration of the country to merit the creation of a Ministry of Public Health, as urged by the Indian Medical Congress of 1894, sanitation was now placed under the care of its financial rival—the Educational Department.

Schemes for a Public Health Service were forwarded by Major Malleson, S.C., direct to the Government of India (1867); by Surgeon-Major Ranking (1868), and by Surgeon-Major H. King (1875–6) to the Government of Madras. In 1868 the matter was the subject of inquiry in a dispatch by the Secretary of State to the Government of India as follows: "I have expressed my approval of the appointment of special Medical Sanitary Inspectors for each Government immediately subordinate to your Excellency in Council; but I desire now to be informed of the organisation by which it is proposed, under the Medical Sanitary Inspector's supervision, to secure the health and cleanliness of towns and villages under each Government. In large towns, it is presumed, the Municipalities will be rendered available for this duty; but I should wish to be informed of the mode in which each Government will satisfy itself that the Municipality of any given town performs its duty in this respect. I would ask further what arrangements will be made for attaining the same object in small villages."

In 1894 the first Indian Medical Congress represented the subject to the Government of India by deputation.[1] In December 1895 that Government gave belated attention to the matter by requesting Local Governments to furnish suggestions. The Sanitary Commissioner for Madras (W. G. King) forwarded a scheme in detail and, presumably, the other Sanitary Commissioners for Local Governments acted likewise. In 1904 Sir Andrew Fraser, then Lieutenant-Governor of Bengal, urged the Government of India to organise a Public Health Service *throughout India*. In 1905 the College of Physicians (England) made the same representation. In 1907 the Secretary of State for India again called attention to the subject. All

[1] At this Congress Prof. W. J. Simpson, C.M.G.—then Health Officer, Calcutta—made the useful suggestion that there should be with the Government of India a Ministry of Health.

efforts remained fruitless. But by 1881 Surgeon-General (then Surgeon-Major) Cornish, C.I.E., as Sanitary Commissioner, had made the first successful endeavour towards the evolution of a Public Health Service, by securing sanction of the Madras Government for attachment of a Civil Assistant Surgeon to the Civil Surgeon of each District: these Assistants were selected as of professional capabilities which would ensure satisfactory medical work during the absence (in the interests of sanitary duty) of the Civil Surgeon on tour within his District. Thereafter, Civil Surgeons in the Madras Presidency were termed "District Medical and Sanitary Officers." In 1891 Colonel (then Surgeon-Major) W. G. King, C.I.E., when "on special duty," advised the reorganisation of the Vaccination Department, the erection of a Vaccine Institute combined with bacteriological and pathological laboratories, and the giving of facilities for young medical officers being trained as specialists—the latter detail being inspired by the results achieved by Cunningham and Lewis; by 1903 much of this scheme had materialised.[1] In 1893 the same officer, in his then function of Sanitary Commissioner, recommended, and before the end of 1894 secured sanction for, compulsory employment by all local bodies in the Madras Presidency of none but certificated Sanitary Inspectors who had undergone special technical training and examination of the nature formally approved by Government. Over 1,174 men of sound general and technical education and of superior castes and social status have thus qualified, at their own expense. All Municipalities were soon provided with these men, and District Boards made appointments for them with increasing frequency. United with the Cornish scheme, there therefore was in process of formation a complete Public Health Service. *But*, then came the proof—if proof there need be—that the Sanitary Commissioner should be subject solely to the Government he serves and "not to the Medical Department"[2]; in short, that *a Public Health Service must be self-contained.* The Surgeon-General with the Local Government of 1895–6 did not appreciate the attention of officers under his discipline being utilised with ever-increasing frequency for sanitary work; he skilfully trammelled their movements in the interest of curative medicine, with the inevitable sequel that the Assistant Surgeons of the Cornish scheme became merely Clinical Assistants at headquarters. The obvious remedy was to duplicate the Assistant Surgeon by

[1] India now possesses the following laboratories: Pasteur Institute of India, Kasauli; Pasteur Institute of Southern India, Coonoor; Pasteur Institute of Burma, Rangoon; the Bombay Bacteriological Laboratory; the King Institute, Madras; the Central Research Institute, Kasauli.

[2] Government of India, Home Department, No. 223 of June 4, 1886.

the addition of a Civil Assistant Surgeon subject to the Sanitary Commissioner; to be attached by him to the District Medical and Sanitary Officer as solely liable to orders for sanitary duty. The Madras University[1] had in 1889 undertaken to grant the qualification of Licentiate in Sanitary Science (now held equivalent to the D.P.H. of Great Britain), and the taking of this within a defined time could be rendered compulsory. In the meantime, Civil Assistant Surgeons were obtainable who had undergone courses in Hygiene and Bacteriology; to these could be added a full course in Minor Sanitary Engineering. All Sanitary Inspectors above the grade of Assistant Sanitary Inspectors had also received full training in the latter requirement. With these two classes of subordinates available, therefore, it was evident that what hitherto were mere abstract proposals for minor and petty sanitary engineering works could henceforth reach the District Medical and Sanitary Officers accompanied by plans and estimates; and that, by attaching to each of these officers a Public Works subordinate who could aid them in scrutinising these proposals, it was rendered possible to forward such as met with their approval to District Boards for financial sanction; with that rapidity of action which is essential when dealing with the frequently simple yet urgent measures necessary for conservancy, the protection of rural water-supplies and the carrying out of anti-malarial works. The scheme (W. G. King) secured the full support of the Madras Government (1903–4); *but* the Surgeon-General of the period protested that, as the Head of the Medical Department, the Assistant Surgeons should be nominated by him and be subject to his discipline, whilst District Boards held that, as they paid the salaries, they had the legal right to retain them under their orders. The Sanitary Commissioner was effectually "blocked out." This scheme went lamely forward with no authority to dry-nurse it. Subsequent Sanitary Commissioners pleaded for its completion in the interest of life in rural areas. Nevertheless, in 1921, of seven pioneer Sanitary Assistants (there should have been at least twenty-two) the District Boards retained only two. As it happens, this scheme, excogitated twenty years ago[2] (only to be wrecked, as the proposer prophesied in a published official Report would be the case, if the Curative

[1] At the suggestion of Surgeon-Major McNally, I.M.S.

[2] Since this was in type, the Minister of Public Health of the Government of Madras has announced that a scheme of a similar nature, ensuring a complete Health Service, will be put in force shortly. Unfortunately, no mention is made of the pivot-man in rural organisation, namely, a Public Works subordinate attached to the Chief Sanitary Officer of each District. If this scheme escape the official conservatism which doomed its predecessor—notwithstanding it possessed the imprimatur of the Government—the effort cannot fail to redound to the credit of recent administrative changes.

branch of Medicine were allowed to dominate the Sanitary), has been adopted in principle, as a result of large and independent experience, by the Health Department of the Panama Canal, as stated by Colonel Fisher in his Report for 1921: "About three years ago, the Health Department undertook to do all its minor engineering work within its own forces. The Sanitary Inspectors were provided with levelling instruments, and such as required it were taught to use them." In the meantime, the Government of India scheme of 1911-12 which took cognisance solely of the population of Municipalities—thereby excluding 227,000,000 of the inhabitants of British India from its operation—has received more or less attention throughout India, but with far from complete results. So far as the Madras Presidency (pop. 41,402,026) is concerned, the judgment as to progress passed by the Ministry of Local Self-Government [1] in 1921 was as follows:

"The reluctance of professional men to accept service under Municipal Councils is a menace to the future of sanitation of this Presidency. Of the two appointments sanctioned for first-class health officers, one is at present vacant, and of the fourteen posts sanctioned for second-class health officers, only three are filled. No candidates have appeared for training as health officers for the past three years. Even men qualified as sanitary inspectors avoid service under local bodies if they can get other employment."

In the Bombay Presidency (pop. 23,081,064), where there are 157 Municipalities, only 11 have as yet appointed Health Officers of any class, and 26 towns share 50 qualified Sanitary Inspectors. The latter in respect to their employers complain they have difficulty "in securing genuine co-operation." In Bengal (pop. 52,656,461) there are—excluding Calcutta—115 Municipalities; only 22 of these have Health Officers, and the whole share 90 Sanitary Inspectors.[2]

What, then, has resulted from the efforts of the Indian Medical Service,[3] in spite of inhibitory conditions, during the

[1] G.O. No. 80, P.H. January 26, 1921.

[2] Reports of Director of Public Health, Bengal, and of the Sanitary Commissioner for Bombay for 1920.

[3] In regard to curative medicine, hospital statistics sufficiently demonstrate that multitudes have been spared suffering and life rendered tolerable. It is worth remembering that hospital organisation was evolved in consequence of the private efforts of the early civil and military surgeons of the East India Company, and that the Government which supplied these officers for the care of troops and civil officers gave no extra remuneration for their philanthropic and fast-expanding labour. Hospital returns for 1919 show there were treated in that year 35,078,305 patients, and that 1,368,614 operations were performed. The efficiency of medical training by members of the Service is testified to by the position assigned to graduates of Indian Universities by the Medical Council of Great Britain.

sixty years which have elapsed since the Royal Commission of 1859–63 laid down the fundamental principles and organisation on which reliance should be placed for mitigating the severe mortality they found to exist? As will be perceived from the preceding notes on Sisyphean sanitary progress, these inhibitory factors embrace not only the ca' canny policy of the politician in his effort to act "on the line of least resistance," by advancing education as the pioneer of sanitation, but his product of two generations—the educated representatives of Local Boards and Municipalities to which practically self-government has been afforded for fifty years: nor is it possible otherwise to class the ill-advised efforts of certain administrative officers of the Indian Medical Service, who happen to have possessed proclivities towards Curative Medicine, to absorb and dominate Preventive Medicine which, in the course of these years, had received world-wide recognition as a definite speciality.

As opportunities have offered, neither members of the Indian Medical Service nor of the Indian Civil Service (*minus*, in the case of the latter, as years passed, an ever-decreasing conservative minority) have neglected to urge the progress of sanitation by adaptation of legislation, the issue of Government executive orders, and the carrying out of sanitary works; and be it said to the credit of the Civil Service that each innovation, in the absence of a Public Health Service, thrust upon officials additional toil and responsibility, either in their functions as magistrates or in control of civil staffs of Districts. Under such conditions, sanitary administration in India—notably in the Madras Presidency—has in several respects effected reforms of method in advance of Great Britain.

In the absence of reliable agencies for the collection of vital statistics in the majority of the country, the calculations made for the Census of 1911 give the most trustworthy figures available up to date. By applying the deductions by Mr. Ackland on data given in Mr. Gait's Census Report for 1911 to figures as supplied in the Statistics of India Report (*India*, part vii) of 1911–12, it is found that of the District Boards of India (which rule communal affairs in all extra-municipal areas), the Presidency of Madras, which gave the smallest percentage of its funds, excluding fees received, to Education, and the largest inclusive sum to Medical, Sanitary, and Vaccine Services, had the smallest death-rate per mille of population, namely 33·4, as against 46 for the Punjab, United Provinces 43·3, Bengal 40, and Bombay 35·8. It is an axiom, in gauging vital statistics, that any mortality over 17 per mille per annum, in a free population, denotes the presence of some unrecognised morbific factor. There is no reason why this very moderate estimate of normal mortality of human beings should not be applicable to

India. What, then, must be thought of such heavy rates, when it is remembered that, as averages, they convey no idea of the actual death-rates—especially the infantile—as found in certain areas of the larger cities?

Abiding by statistics for the period 1910–14 as representing facts undisturbed by war conditions, it is found that the death-rate of British[1] troops has been reduced from 69 per mille, as reported by the Royal Commission of 1859–63, to 4·36 per mille, and of Indian troops from 20 to 4·39 per mille, and prisoners from an average of 102 to 21·50 per mille. It has been fully demonstrated that cholera and small-pox, which contributed heavily to the total mortality both of troops and the civil population previous to "the sixties," may confidently be held in abeyance where sanitary measures are put in force whole-heartedly. The introduction of public water-supplies in many of the larger towns of the Bombay and Madras Presidencies has been followed quickly by marked reduction of the total death-rates. No impression has been made on the country as a whole in respect to malaria—nor is it otherwise possible so long as the main reliance of the authorities concerned is upon quinine prophylaxis of the people, as contrasted with the carrying out of permanent anti-malarial works. In 1909 the Government of India admitted that of the 4,500,000 deaths registered annually in India as due to "fevers," 1,000,000 might reasonably be regarded as caused by malaria. It was to that Government, in 1898, that Sir Ronald Ross first reported his discovery of the malaria agency; thereby connoting facts which have revolutionised the economic and international potentialities of hitherto shunned malaria-stricken areas of the tropics. The politician of India, subjected to a nightmare of financing projects for control of surface and subsoil waters, again proved equal to the occasion; it became obvious to him that intensive education of the householder would lead to the killing of the mosquito—at any rate on approach of the millennium; whilst, in the meantime, if it did so happen that the mosquito killed the educated householder, the finance-surfeited Educational Department was capable of proving that beneficent Nature would not waste so highly evolved material.

An excuse often urged for non-existence of a Public Health Service in India is that neither the Governments nor the Local Bodies concerned have the requisite funds. Schemes which have been worked out in detail show that, if funds were more equitably distributed in accordance with public requirements other than education, the cost could be readily borne. And, indeed, even without this desirable reform, the readiness

[1] Under peace conditions, the R.A.M.C. undertakes executive duties with British troops; administrative posts are shared by the A.M.S. and the I.M.S.

with which Local Bodies have consented to impose extra taxation, with resulting accumulation of large funds for railways, proves that the plea is absurd.

Further, if the organisation of such a service be essential for the application of life-saving measures, it may well be added that the neglect is indubitably financially unsound.

On data supplied to him, Mr. Stott, a well-known mathematician, has reckoned that, were attacks and deaths from malaria confined solely to families where the head male member earned as low wages as Rs. 5 per month, India incurs an unproductive expenditure to the extent of over £20,000,000 annually—without regarding the cost of rearing the producer. This may well be classed as a " drain upon India," and equally so is the financial loss resulting from the failure of the indigenous politician—the product of the Educational Department of India—to recognise the interrelation of the health and wealth of communities. There is no reason why the mean expectation of life of an Indian male should not be as good as, if not better than, that of males in England and Wales—provided the former existed under reasonable sanitary conditions; yet, according to Hardy's Life Table, the chances are that at birth his life will not exceed 22·59 years, whilst, *under progressively improved sanitary conditions*, the Briton has been credited in the Life Table of 1841 with an expectation of life of 40·17 years, in that of 1911 of 46·04 years, and in that of 1914 of 48·53 years.

If, as indeed above shown by the present death-rates for both European and Indian troops and prisoners as compared with those reported by the Royal Commission of 1859–63, the life of the Indian is as capable of prolongation under improved sanitary conditions as that of the Briton, then it is evident, in the extravagant outlay of funds for education which should have been devoted in reasonable proportion to both causes, a perpetual annual waste is incurred. From data supplied to him, Mr. Stott has calculated that, 29·5 of the present population of school-going age being regarded as a constant influx, India loses £427,148 each year, by reason of a proportion failing to reach 20 years of age, out of a total of funds public and private expended for education (Rs. 71,868,000), of which £220,140 represents public funds; and that failure to reach the age of 29 represents the loss out of total funds of £851,318, of which public funds amount to £438,744. If " it is assumed that the education gives an increased value to the nation, those who die before 20 will have given about one-tenth of their increased value, and those who die before 29 about one-fourth of their increased value, to the nation, thereby reducing the loss at the age of 20 to £386,434 out of total funds, of which £196,126

represents public funds, and those who die before the age of 29 the loss of £638,438, of which £329,858 represents public funds." It need hardly be said that, at least as a brain-worker, the man who dies before 29 years of age represents a serious loss to his country.

That Education is a most desirable ally of Sanitation is incontrovertible; but in India it has been pursued on fantastic lines which have had inapt relation to the requirements of the country [1]; whilst *education by practical demonstration of sanitary works for the community*, which need not interfere with either caste nor creed (and which the average uneducated Indian is not only capable of grasping the benefit of, but of receiving with much gratitude), have been grossly neglected in the rural areas, containing no less than 93 per cent. of the total inhabitants of the country. It is early yet to form any forecast as to the progress of sanitation in India under the Montagu Reform schemes, but, so far, it would seem that there is infinitely more anxiety to multiply colleges and universities for academic science and curative medicine of archaic origin, than to remember that the "first wealth is health," and that "within human limits, health is purchasable by communities."

[1] In the debate on the Budget for 1913–14 (selected for quotation as typical of a prosperous period undisturbed by war conditions) in the India Imperial Council, it was shown that Mr. Montagu's proposal to increase existing primary schools from the then existing 90,000 to 120,000 implied an additional outlay of 60,000,000 rupees, within an unstated period. In reply to Indian Members urging early action, they were informed that, within the previous four years, expenditure on Education had increased from £1,705,000 to £3,043,000, and on Medical and Sanitary services from £968,000 to £1,683,000, whilst "the growth on Police expenditure has been only 10 per cent., and on Military services has been less than 1 per cent." The Members were further reminded that of grants for Education 71 lakhs, or £500,000, have not yet been disposed of, and, further, that "we cannot really spend at once all the money we can get." The contrast, lucid as it is as to hysterical financing of Education to the detriment of other services—notably the Military, as evident in equipment in Mesopotamia—allows no conception of the small amount allotted to Sanitation by reason of the two branches of Medical service being jumbled together.

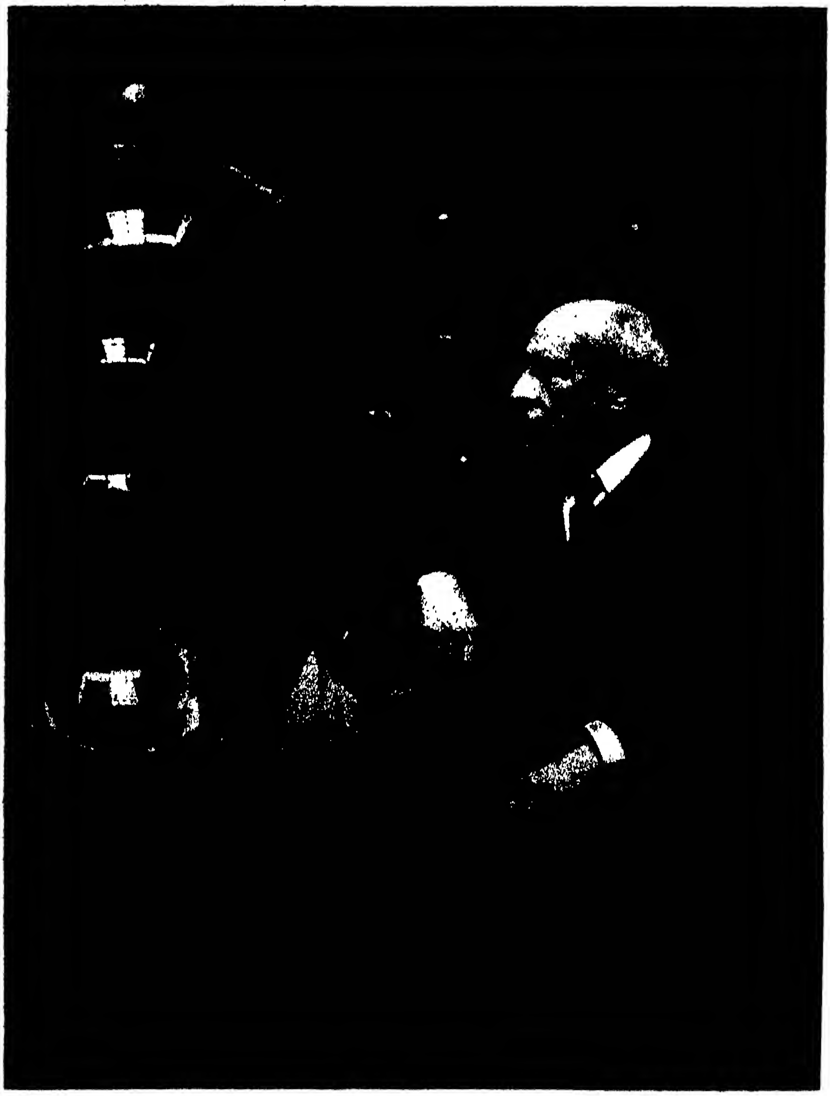
NOTES

In the April Number of Science Progress a review on *A Treatise on Chemistry*, by Sir Henry Roscoe and C. Schorlemmer, fifth edition, revised by Dr. J. C. Cain, appeared on p. 667, following another review on *The Manufacture of Dyes*, by "the late" J. C. Cain. Both these reviews were written and signed by our valued contributor, F. A. Mason, M.A., Ph.D.; and it must have surprised those who are acquainted with Dr. Mason's accuracy to observe that the first-mentioned review referred to Dr. Cain as still living, while the previous review had referred to him as dead. As a matter of fact the review on *A Treatise of Chemistry* had actually appeared two years ago in our April number of Vol. XV, p. 661. By some inexplicable mistake, however, an uncorrected proof of this review, which ought to have been destroyed, had slipped into the make-up for our April 1923 number, and was reprinted without the reviewer's corrections and appeared as stated. We owe our profound apologies not only to our readers, but also to Dr. Mason, for the error.

We have pleasure in announcing the appearance of a new publication, *The Wireless Review and Science Weekly*, edited by Mr. Norman Edwards, and with Mr. G. B. Dowding, A.C.G.I., Grad.I.E.E., as technical editor. Sir Oliver Lodge is the scientific adviser; and the first number contains an article by him on the Ether of Space, another article by Signor Marconi, and several other very interesting contributions. There are a number of illustrations. The editorial office is at Fleetway House, and the price is 17*s.* 4*d.* per annum.

Sir James Dewar, F.R.S., LL.D. (Sir James Crichton-Browne, M.D., F.R.S.)

A great man of science has passed away, resolved into that atmosphere the secrets of which he has done so much to disclose. Sir James Dewar died on March 27th at the Royal Institution in Albemarle Street, which has been for forty-six years the scene



Miss Olive Edis, F.R.P.S

SIR JAMES DEWAR, F.R.S., LL.D.

of his labours, and his remains were, by his express wish, cremated at Golder's Green on the following Saturday.

Born at Kincardine-on-Forth under the shadow of the Ochill Hills, and near Stirling with all its romantic historical associations on September 20, 1842, Sir James Dewar was reared in a Presbyterian home and was early introduced to dialectical theology in the *Shorter Catechism*. In his tenth year there occurred an incident which probably coloured his life. While skating on a winter's day he fell through the ice, and when rescued walked about in his wet clothes till they were dry so that his family might not learn of his misadventure. The result of this was that he had a severe attack of rheumatic fever which crippled him for two years, and left him with a damaged heart. The heart trouble incapacitated him for the active life to which he had been previously disposed and permanently cut him off from strenuous games and exercises, but in no degree impaired his constitutional energy, which remained intact and unsurpassable till his death. It was in these two years when he was laid aside, free from schooling, with only a modicum of private tuition, and cut off from the companionship of other boys of his age, that his native gifts had a favourable opportunity of spontaneous growth. He browsed unconfined on the wholesome pastures of English and Scottish literature, drank deeply of Burns, and above all began to think for himself and to create, and creation is the essence of all genius. Always devoted to music, he had before his illness attained to some degree of proficiency on the flute, but was now debarred from that instrument by breathlessness, and so turned to the violin. With the help of the village joiner, he made for himself several violins, one of which, wonderfully expressive in its tones, was played on at the celebration of his golden wedding in 1921.

When twelve years old Dewar, still a pale and delicate boy, went to the Dollar Academy, a Scottish Secondary School of high repute, of which he always spoke very gratefully, and there he resumed the ordinary routine of the education of the period. It was a little incident at Dollar, the discovery in the garden of Mr. Lindsay, the master with whom he was boarded, of an old and half-buried sundial, in the erection and orientation of which he took some part, that inoculated him with a taste for exact science; but it was not until he went to the University of Edinburgh, at the age of seventeen, that his apprenticeship to science really began. There he soon diverged from the accustomed literary course and plunged, as it were instinctively, into mathematics, physics, and chemistry. In this congenial element his ability was speedily recognised by two of his Professors, Guthrie Tait and Lyon Playfair, the latter of whom made him his class assistant. There was great

intellectual activity in Edinburgh while Dewar's lot was cast there in the sixties of last century, and into that he entered with zest and with an acceptance not usually accorded to so young a man. His teaching power attracted large classes to his practical demonstrations, and the experimental tendencies, which were in the marrow of his bones, unmistakably displayed themselves, leading Lyon Playfair to suggest to him that he should accept an appointment for technical work in connection with the dyeing industry with which his friend Crum Brown, who became Playfair's successor, was, by family ties, associated. Had Dewar adopted this course Perkins might have been anticipated, but he preferred to remain in Edinburgh to carry on his less circumscribed researches there, in the meantime, however, enlarging the scope of his studies, by a sojourn at Ghent, where under Kekuli he gave special attention to organic chemistry.

Returning to Edinburgh as Demonstrator of Chemistry in the University, he engaged, with Guthrie Tait, in experiments with Crookes's newly invented radiometer, and with McKendrick in an inquiry on the physiological action of light. From the University he passed to the Dick Veterinary College as Professor of Chemistry, and it was while diligently working there that an offer of promotion unexpectedly came to him. There was a vacancy in the Jacksonian Professorship at Cambridge, for which there were several candidates, and a selection was imminent, but at this moment the late Sir George Humphrey visited Edinburgh as an examiner in the Medical Faculty and was introduced to Dewar. With keen discernment he took his measure and immediately telegraphed to Dr. Porter, then Tutor, afterwards Master of Peterhouse, "Hold your hand, I have found the man." At the same time Guthrie Tait wrote to Cambridge indicating Dewar, and that settled the matter, and the post was offered to him by telegram. He was busy and happy, a brilliant career in Edinburgh, almost inevitably culminating in a Professorship, was opening out before him; but his young wife, with sure intuition, felt that he deserved a wider field than Scotland could afford, and so the die was cast, and the migration to Cambridge took place.

It would not be correct to say that Dewar found himself in a congenial element in Cambridge at that time. His lectures were an unprecedented success, he made some life-long friends, of whom one, Prof. Liveing, much loved and venerated, still happily survives, but some bristles of the Scottish thistle adhered to him, and chemistry and physics had not then come to their own on the banks of the Cam. He had not even such facilities as he had enjoyed in the North. His laboratory was a small room, without a fireplace and badly lighted; apparatus

was conspicuous by its absence, and his aspirations, very forcibly expressed, were not very sympathetically received. It was therefore with satisfaction that he found himself translated to a more elastic atmosphere when in 1877 he was elected Fullerian Professor of Chemistry at the Royal Institution, in succession Dr. John Hall Gladstone.

It was in the laboratories of the Royal Institution, during his incumbency of the Fullerian Professorship, that all Dewar's triumphs were achieved, more especially those in connection with the liquefaction of gases, and the properties of matter at temperatures approaching the absolute zero. Faraday, the god of his idolatry in all scientific affairs, had led the way in this exploration and had, by means of low temperature and pressure, succeeded in liquefying all the then known gases, except nitrogen, oxygen, and hydrogen, and the compound gases, carbonic oxide, marsh gas, and nitric oxide, and as early as 1874 Dewar was fascinated by the subject, as evidenced by his lecture before the British Association on "Latent Heat of Liquid Gases." In 1878 he showed Cailletet's apparatus in operation in this country. It was, however, the success of Wroblewski and Olsyewski of Cracow in liquefying oxygen in 1884 that withdrew him from his earlier preoccupation, with the heat of the sun, electro-photometry, and the chemistry of the electric arc, and supplied the stimulus to his more memorable discoveries. In 1885 he was able to show a profoundly moved audience at the Royal Institution the air we breathe made visible as a clear liquid, compressed to one-800th of its bulk and produced at a temperature of −192 Centigrade. In 1893 came oxygen in a solid state, an ultramarine ice produced at −216 Centigrade, and in 1897 Fluorine, as a fluid. In the following year appeared liquid hydrogen, and in 1899 a crowning close of the century, that gas in a solid state, at a temperature of − 260, or about 13 degrees above the point of absolute zero, that unplumbed depth, where molecular movement is no more. Helium alone remained unsubjugated by Dewar, and that he would unquestionably have liquefied had not Onnes of Leyden, working on his lines, accomplished the feat while he was preparing for it.

Now that liquid air is an article of commerce, Dewar's liquid-air work has become popular knowledge, but only an expert who has essayed such an enterprise can conceive the patience, the industry, the ingenuity, the constructive genius required in it. Dewar devoted to it years of unremitting toil and pursued it not without risk to life and limb, and sometimes embarrassed by the question of ways and means, to carry on so costly a campaign. To obtain a degree of cold sufficient to liquefy hydrogen by means of internal work done by the molecules

while a gas was being forced through a porous plug, involved the building up of a machine capable of sustaining pressure in many tons to the square inch, even at a temperature of — 260 Centigrade, and fitted together with a nicety and precision, of which even first-class engineering knows little. To protect the liquid gases when produced against the influx of heat special measures were necessary, and the search for these led to the invention of the vacuum-bulb, the parent of the thermo-flask which Dewar's nimble brain devised, which must have brought him a huge fortune had he chosen to patent it, and which, if properly designated, should keep his name alive for ever, even amongst the masses of mankind. But the vacuum-bulb, even when silvered, was not enough. In order to examine the liquefied gases in a static condition, and unevaporated for long periods, specially high vacua were needed and these were procured by Dewar's utilisation of the absorptive power of carbon. "The discovery of the marvellous power of charcoal to absorb gases at low temperature," says Prof. Armstrong, "will render the period 1900 to 1907 ever memorable."

Dewar's liquefied gases, thus detained, became themselves instruments of research, and enabled him to conduct novel and illuminative investigations on electrical conductivity, thermo-electric powers, magnetic properties, and electric constants of metals and other substances at low temperatures and on the effects of extreme cold on chemical and photographic action. Having established that chemical changes are almost quite inhibited at temperatures about 300 F. below zero, Dewar, with the assistance of Prof. Macfadyen, determined to test how far vital processes were affected by the same conditions. A typical series of bacteria was employed for the purpose, possessing varying degrees of resistance to external agents. The bacteria were first simultaneously exposed to the temperature of liquid air for twenty-four hours. In no instance could any impairment of their vitality be detected in either growth or functional activity. This was strikingly illustrated in the case of the phosphorescent organisms. Their cells emit light which is apparently produced by chemical processes of intra-cellular oxidation, and the phenomenon ceases with the cessation of their activity. These organisms, therefore, furnished a crucial test of the influence of low temperature on vital manifestations, and when cooled down in liquid air they immediately became non-luminous, but, on being thawed, the luminosity as speedily returned. In further experiments the organisms were subjected to the temperature of liquid air for seven days. The results were again nil, for on thawing they renewed their life processes with undiminished vigour. The organisms were next exposed to the temperature of liquid

hydrogen—only 28° above the absolute zero and again the results were nil. The fact that life can continue to exist at a temperature at which, according to our present conception, molecular action ceases and the entire range of chemical and physical activities, with which we are acquainted, either ceases or enters on an entirely new phase, affords ground for reflection, as to whether, after all, life is dependent for its continuance on chemical reactions.

Dewar's heroic attempts to reach the absolute zero of temperature, solving problems of supreme importance and intricacy by the way—time-and-strength-consuming though they were—did not exhaust his scientific energies or complete his conquests. As a member of the Explosives Commission in 1888 in conjunction with Sir Frederick Abel, he invented cordite, which became the standard smokeless powder, and during the war he contrived a light and portable apparatus for the conveyance of oxygen so that it might be available as a protection against mountain sickness for men going up in aeroplanes. He conjured up giant soap-bubbles that survived for months, because the air inflating them was like Bonny Kilmenny, "as pure as pure can be," and spread out films of extreme tenuity that in their stream lines and vortex motion yielded to his manipulations, assemblages of dancing rainbows of exquisite beauty. He took part in many inquiries, bearing on the public health and especially on the safeguarding and improvement of our water-supply, and was a much sought and inexorable witness before committees of Lords and Commons. Along with Prof. Liveing he conducted an elaborate series of studies on spectroscopy that have now been collected in a volume, and would by themselves place him in the first rank as a man of science.

Besides doing his own work Dewar was the cause of much work in others. He was eminently suggestive and freely helpful to all who sought his assistance. He did not suffer fools gladly, and was intolerant of pretentious mediocrity; but for the earnest student and honest worker he had unfailing sympathy and encouragement. The fruits of his experience and the seeds of his speculations—and hypotheses of the right sort are valuable commodities in science—were always at the service of those who consulted him. And it is certain that ideas which he thus flashed forth have afterwards, without acknowledgment, materialised in profitable inventions.

Dewar identified himself with the Royal Institution and the Royal Institution became identified with him. He pervaded it so that many of its habitués entering it now feel as if the soul had gone out of it. The scene of his labours became the object of his affections, and he never spared himself in its service.

Proud of its traditions, and conscious of the opportunities it had afforded him, he strove to enhance its reputation and extend its usefulness. He made liberal benefactions to its funds, and was wont to enlarge on the magnitude of its accomplishment with the very meagre means at its disposal, pointing out that the fundamental ideas and experiments on which are based the stupendous chemical and electrical industries of to-day were worked out in its laboratories by Davy, Faraday, Tyndall and himself at an average expenditure on research of £1,000 a year.

During his period of office at the Royal Institution Dewar delivered 238 lectures in all, 49 Friday evening discourses, 48 Christmas lectures, and 151 afternoon lectures. As his lectures were no off-hand demonstrations, but carefully prepared expositions, every experiment being previously rehearsed, they entailed a heavy drain on his time and energy. In the ten years—1884 to 1893—he delivered six of those Christmas Courses of lectures to juveniles, which make peculiarly exacting demands on minute attention and lucid expression, dealing with subjects as varied as "Alchemy," "Meteorites," "The Air," "Clouds and Cloudland," "Frost and Fire," "Light and Photography." It was by the allurements held out by him that the late Dr. Ludwig Mond was induced to make to the Royal Institution the munificent gift of the Davy Faraday Research Laboratory, which affords unique opportunities to those individual and independent investigators on whom Dewar's hopes for the advancement of science were mainly fixed.

Dewar had a singularly impressive and attractive personality. He had a head like Shakespeare, a countenance finely chiselled, expressive of vivid intellect and abounding vim blended with good-humour. He gave the world "assurance of a man," a strong true man, open-hearted and open-minded, quick of temper perhaps, but genial and generous withal, a staunch friend, a delightful companion. With a proper endowment of the *ingenium perfervidum Scotorum*, he was sturdy in spirit, intrepid in manner, fearless, patriotic, and given to hospitality. No one could be more inimical than he to the occult in all its phases, and yet the Press has been not altogether wrong in ascribing to him a certain wizardry—"the wizard of Albemarle Street" they have called him—for he was a wonder-worker and threw a spell over his audience. Bent on the pursuit of reality and on the control of nature through the advancement of knowledge, there was scope in the amplitude of his mind for ideal values. He had imagination, which is the forerunner of science, "the vision and the faculty divine," and was a connoisseur in music and the fine arts. On the book-

shelves in his study, within reach from his easy-chair, were assembled well-worn copies of the Essays of Montaigne, Elia, and Emerson; the Poems of Hardy, Walt Whitman, Rossetti, and Meredith; Landor's *Imaginary Conversations; Carlyle's Heroes; Sesame and Lilies*, and the *Cricket on the Hearth*.

Dewar was knighted in 1904, and that was the only and wholly inadequate recognition offered to him by his country, to which he brought honour and profit. But foreign countries and learned bodies were more appreciative of his merits than the dull-witted ministers at home. The Royal and Philosophical Societies and Academies of Rome, Belgium, New York, Philadelphia, Frankfort, Milan, and Copenhagen were proud to inscribe his name on their rolls, and all the four Scottish Universities, as well as those of Oxford, Dublin, Brussels, and Christiania, conferred on him Honorary Degrees. The Royal Society awarded him its Copley, Rumford, and Davy medal, and he was President of the British Association in 1902.

Sir James Dewar married in 1871 Helen Rose, daughter of Mr. William Banks of Edinburgh, and she survives him. Never had savant a more propitious spouse. Lady Dewar entered keenly into all her husband's interests, sustained him in his heavy tasks, and created the first scientific salon in London. There are few noted people in the world of science who have not attended the receptions in her drawing-room at the Royal Institution after lectures there.

Synthetic Relativity (J. R. Haldane, M.A., LL.B.).

While Einstein's theory provokes much discussion, there is an alternative method of inquiry which has not been adequately investigated. Einstein's method ignores a physical æther, but tacitly assumes a universal reference-frame (his "space") for all the calculations. Relatively to it light is taken as radiating in straight lines with a constant velocity. The "space-time metrics" and "four-dimensional continuum" become necessary for the maintenance of this straightness and constancy in face of the observed phenomena. The alternative method referred to, identified more unreservedly with the Principle of Relativity, would in the first instance ignore all universal reference-frames. The Principle of Relativity requires that any one physical phenomenon may with equal validity (though not necessarily equal convenience) be taken as the reference-system for any or all other physical phenomena, since all physical phenomena are definitely related *inter se* by physical "laws," and cannot be physically related to empty space, the absence of physical existences. This method consequently would set out by confining attention to the observed phenomena of radiation in relation to particular units (or classes of units) of matter and would seek to build up a cosmological system by relating them all *inter se*, as required by the Principle of Relativity.

That this method would repay thorough exploitation is indicated by the tentative deductions which even a superficial survey suggests. A few of the more obvious will suffice here, by way of illustration, and, it is hoped, may lead to more thorough investigations by those competent to make them.

Each unit of matter would be taken as being the nucleus or centre of its own "field" of radiation mechanically attached to it and extending spherically, in decreasing intensity, *ad infinitum*. "Field," in this connection, refers only to the geometrical conformation of radiations relatively to the nucleus, without any further physical implications. Each field would tend to impose its own conformation and velocities on all radiations within its effective influence, and would be considered as passing freely through other fields and their nuclei. The line of any radiation through the universe would thus be the mean of the influences of all fields at each point in its course. Near a nucleus, however (*e.g.* on the Earth's surface), the field of that nucleus would overwhelmingly predominate in determining the conformation and velocities of radiations. *All physical phenomena could thus be stated in terms of pure mechanics and Euclidian geometry*, in conformity with all the known phenomena of radiation, any unit of matter being equally valid as a reference-system. The field of an *electron* would be the ultimate unit. The resistances and attractions caused by the rotation of interpenetrating electron-fields suggest an explanation of the coherence of solid matter. The composite system of fields of a solid body could, for most purposes, be treated as a single field. The diverse rotations at various velocities of such interpenetrating fields would suggest some light on the mechanism of solar systems and binary stars, the orbits of which would have a similar motion to that of the perihelion of Mercury.

The constant, diverse, rotations of the electron-fields within such a composite field would provide an elastic medium for wave-propagation. The tensions thus set up would be in the general form of radii and circles, thus corresponding with a ray and a wave-front of light, for instance. The fields of each different form of radiation (light, sound, mechanical forces, etc.) would appear to coincide, for each separate nucleus, and also to coincide with its *gravitational* field, and they can thus all be treated as a single field for each nucleus, and *inertia* becomes the mean influence of all other fields in the universe. As the component electron-fields of the composite field of a solid body (like the Earth) are not concentric, there would be a rhythmical variation of the resistances when two such fields rotated through each other, suggesting some light on wave-propagation (light, heat, electricity, sound, etc.) and gravitation.

As regards light-phenomena, the rotation of the Earth's field would cause such large displacements of the stars, varying with their distance (and modified by the influence of the field of our whole solar system), that the motion of our solar system would result in a slight apparent motion of all the stars in two interpenetrating streams similar to the actual phenomenon demonstrated by the late Dutch astronomer Kapteyn. Light passing through a more intense part of a field would be deflected very similarly to Einstein's prediction. There would also be some distortion of the image, amounting to a complete halo if the star were directly behind the distorting nucleus. As this displacement would tend to disappear with increasing distance from the distorting field, it would be more apparent in the case of the Moon than of the Sun, and also in the case of light received through the Earth's own field from bodies near the horizon. All these factors would combine, in the case of a star passing near our system at a high velocity, to produce all the phenomena of a comet.

It could not be taken as definitely established, on this view, that the velocity which any one field tends to impose on light is constant. It might be found to vary with the intensity of the field as it does with the density of physical media, the wave-length possibly also varying.

As all these deductions can be stated in definite equations, such investigations would disclose the presence of any physical æther (or "space") pervading our part of the universe, if there is one, by its influence on radia-

tion; and, if it is mathematically more convenient to *assume* an imaginary universal reference-frame, they would disclose its requisite internal mathematical structure. For the rest, the conception of a continuous universal æther (or the mathematician's "space") is a metaphysical one which does not affect the physical problem.

Alcoholism Experiments in the Lower Animals (J. B. G.).

No biologist can or would deny that sociologically extensive alcoholism in communities is an evil. It affects indirectly the entire human family, and is the cause of much misery and backwardness. At the present time complete prohibition of alcoholic stimulants is in force in the U.S.A., and this gigantic physiologic experiment is being watched by other nations with much interest. The writer is quite unbiased, but convinced in a general way, as must be the majority of people, that the "soaking" type of drinker is of no use to the community, has no right to have a family, and cannot be excused by any species of evidence except from the brewery shareholder's point of view. The biologist's objection to this type of drunkard is not that his future children will be affected by the father's habits—for that alcoholism has any deleterious effects on the human sex cells has yet to be proved—but because for economic reasons it is quite evident that such a man's wife and children could not be so well cared for as the family of the sober individual. Thus it must be admitted, to begin with, that the sale of alcoholic stimulants is bad for the weaker vessels of the community. One cannot stop to review the various arguments brought forward to suggest that alcohol has helped literary and artistic folk to produce better work: it can be said at once that no scientific thinker would produce more accurate results by the use of alcohol—the latter definitely interferes with precise operations; one wonders also whether a host of literary gems are sufficient excuse for the misery and degradation of any one family.

The attitude of the average man is undoubtedly that since HE can confine himself to a proper and decent quantity of alcohol, and since HIS efficiency is not noticeably affected thereby, while his happiness and that of his friends are enhanced, there is no reason why such a good thing should be taken away from him by force of law. That prohibition is a good thing economically, the writer feels positively, for he has lived in "newly dried" districts and seen it for himself. This side of the matter simply boils down to the question as to what is most desirable—the most perfect efficiency, or a less perfect efficiency and the pleasure brought by moderate drinking plus the social misery of a section of the least balanced part of the community: the reader must have his own views on this subject. America chose "perfect efficiency."

The well-known work of Miss Elderton and Prof. Karl Pearson, both of London University, has been generally accepted for human beings.

In America, though nothing approaching the work of Elderton and Pearson has been carried out on humans (and, one might add, can now be carried out), still some very interesting work has been done by Americans on the effect of alcohol on animals. Guinea-pigs, fowls, mice, rats, dogs, and rabbits have been used: the results have been contradictory, and some of the evidence showed that alcoholic parents had healthier offspring than non-alcoholic. In nearly all the experiments carried out by Stockard, Nice, Pearl, Arlitt, and Macdowell the object was twofold: to find out whether the offspring of alcoholised animals were mentally or physically inferior to those of non-alcoholised.

Stockard and Papanicolaou, with guinea-pigs, found that alcoholisation was unfavourable to the offspring; Raymond Pearl found that alcoholised fowls produced bigger and healthier offspring; while Nice found, in mice, that mild doses produced greater fertility and growth. In rats, Arlitt re-

ported unfavourably; dogs also were badly affected. In one fact alone does each author agree—alcoholic animals have fewer offspring. They may be larger and healthier, they may be weaker—but they are always fewer. One might breathe the hope that the same applies to man.

In some of these experiments the alcohol was given in the food, but in the best type of experiment the animals were exposed to the vapour or smell of alcohol, till they got " under the table " : in rats this happens pretty soon. Such daily anæsthetisation does not appear to hurt the animals, and certain observers have shown that the patients soon become used to the treatment.

MacDowell has procured some interesting results by using a maze which the rats were taught to learn: this was used as a sort of mental test. Treated rats took longer to learn the maze, produced smaller and fewer litters, and grew more slowly. In the second generation, however, the offspring were heavier, and learnt the maze nearly as soon as non-treated individuals. MacDowell has concluded that alcohol works in two or more different ways: one selectively on certain hereditary characters, the other directly on the germinal material so far as number of litters and growth are concerned.

Wherever mental qualities could be tested, this work on alcoholisation of the lower animals has shown that deleterious results follow. Against this is the work of Miss Elderton and Karl Pearson on human beings, already mentioned. So far as fertility and growth of the offspring of alcoholised animals are concerned, the results are contradictory, and often in favour of alcohol. Here, of course, in humans the economic conditions enter, and then alcohol is definitely injurious when its use is abused.

Finally, if you have shares in a brewery, it is evident that alcohol in large quantities is advantageous; if your shares are in mineral waters, it is quite evident that alcohol is dangerous.

Dr. Kammerer in Cambridge (A. G. Thacker, A.R.C.S.).

All who have followed recent contributions to the perennial controversy concerning the heritability or non-heritability of acquired characters, will be aware that much attention has been attracted in this and other countries to certain experiments which Dr. Kammerer, of Vienna, has been conducting on amphibians. A brief description of some of these experiments was given, for instance, by Prof. MacBride in his article on " The Inheritance of Acquired Characters " in Science Progress for January 1921. Much interest was, therefore, aroused among English biologists when it was announced this spring that the Cambridge Natural History Society had arranged for Dr. Kammerer to visit Cambridge and describe his experiments in full. Dr. Kammerer arrived in England at the end of April, and on April 30 full opportunities were given to those interested to see certain of his specimens and to hear his views. Dr. Kammerer's specimens were exhibited in the afternoon at the Annual Conversazione of the Natural History Society (which had been advanced in date for this purpose); and in the evening a large audience assembled to hear his lecture, giving an account of the much-discussed experiments. The lecture, which was delivered in English, was most lucid. A full official report of it was published in *Nature* for May 12.

The lecturer referred first to the changes induced in the breeding habits of *Salamandra maculosa* and *Salamandra atra* by changing their respective environments (see Science Progress, January 1921, p. 401). *S. maculosa* lives in the lowlands and produces numerous gilled young at a birth. *S. atra* lives in the highlands and produces two more fully developed young at a birth, which have already passed through their gilled stage. By altering the conditions, Kammerer induced *S. atra* to produce through several

generations an increasing number of young (with gills); and conversely, he caused *S. maculosa* to produce through several generations a progressively decreasing number of young, the latter being more fully developed. Dr. Kammerer referred only briefly to these well-known experiments, but described in some detail certain colour changes that he had brought about in *S. maculosa*. Races of salamanders kept on a yellow background for several generations increased their yellow markings; and races kept on a black background increased their black markings progressively. Moreover, when the very yellow breed obtained in this manner were transferred to a black background, it was found that their progeny became black more slowly than did the progeny of normal salamanders. In the very yellow salamanders, the colour was in stripes. Now it happens that similar striped races are found in nature, living on light earths, in the Harz Mountains and elsewhere; and in connection with these races (which are known as the variety *tæniata*) the lecturer brought out a most interesting point. Crossings between normal spotted salamanders and natural *tæniata* gave rise to Mendelian phenomena in the descendants, spottedness being dominant; but crosses between normal salamanders and the artificially produced striped form resulted in blending. And there was another analogous point. Ovaries of spotted females transplanted to natural *tæniata* produced spotted young. "If, on the other hand, ovaries of spotted females are transplanted into artificially striped ones, then, if the father is spotted, the young are line-spotted; if the father is striped, the young are wholly striped."

Next, the lecturer described how he had developed the rudimentary eye of *Proteus* into a functioning organ, by exposing the animals to red light; this was only a development in one generation, but he then explained the facts regarding his experiments on the midwife toad, *Alytes obstetricans*, which he had caused to develop—increasingly through several generations—a nuptial pad (see SCIENCE PROGRESS, January 1921). By inducing the creatures to mate in water, the males developed this pad, which is absent in this species under normal conditions, though present in other toads. Kammerer would not, however, attach first importance to this case, as he thinks it possible to explain it as an atavism.

The lecture was concluded by a description of some experiments on the tunicate, *Ciona intestinalis*. Amputations of the siphons of these animals caused the siphons to be regenerated to beyond the normal length; and it was found that the next generation had abnormally long siphons. And this was the case even when the gonads were removed from the first generation, and had to be regenerated.

As the reader will have gathered, the lecture was of the greatest interest; and Dr. Kammerer had a most cordial reception from his audience. The facts regarding the different behaviour of the natural and artificial striped salamanders when crossed with the normal spotted type, and the facts regarding the tunicates, are highly suggestive. But the range of the phenomena described is narrow, too narrow to form the basis of any far-reaching generalisation on such an immensely important subject as the inheritance of acquired characters. Moreover, the information given was not sufficiently exact. The case of the toad seemed pretty clear; but Dr. Kammerer gave no exact statistics regarding either the salamanders or the tunicates. We did not hear how many salamanders were used, how many died, how many survived; nor were there any data showing the normal range of variation in the colour of their skins. Similarly, there was no exact statement regarding the number of tunicates used in the experiments, what is the normal range of variation in the length of their siphons, and by how much the siphons were elongated in parents and offspring respectively. On all these subjects, exact scientific statements are essential. Perhaps such data will be forthcoming later. A further point to be remembered in refer-

ence to the salamanders is that they belong to a group of animals which are apt to behave very differently in different circumstances, as witness the notorious case of *Amblystoma*. It is possible that the common ancestors of *S. maculosa* and *S. atra* were able to vary their breeding habits according to changes in the environment. Lastly, it may be said that Dr. Kammerer did not forward his case by misstating the opposing view. He gave vent to the general observation that: "If what changes cannot be hereditary, and if what is hereditary cannot change, we can only predict the immutability of species, and therewith dogmatically leave on one side, not only the inheritance of acquired characters, but the whole theory of evolution."

This dilemma is a truism, but it has no meaning in modern biological controversy. Followers of Weismann do not make the nonsensical statement that what is hereditary cannot change. All that they say is that a change, to be hereditary, must be a change in the germ-plasm.

Notes and News.

Among the names of many notable men of science whose death has been announced during the past quarter are the following: Prof. M. Abraham, mathematical physicist; Count Fernand de Montessus de Ballore, seismologist; E. E. Barnard, Professor of Astronomy in the University of Chicago; Sir James Dewar; Dr. C. P. Goerz, the optician; Mr. F. W. Harmer, geologist; Prof. Paul Jacobsen, General Secretary to the German Chemical Society; Prof. G. Lunge, the well-known authority on sulphuric acid; Prof. E. W. Morley—of Michelson-Morley fame—Professor of Chemistry at the Western Reserve University; Prof. W. N. Parker, Emeritus Professor of Zoology at the University College of South Wales and Monmouthshire; Hon. R. C. Parsons, hydraulic engineer; Prof. J. Ritchie, Professor of Bacteriology in the University of Edinburgh; Dr. J. Venn, President of Gonville and Caius College, Cambridge; Prof. A. G. Webster, physicist, of the Clark University, Worcester, U.S.A.; Rev. W. Wilks, for twenty-five years Secretary of the Royal Horticultural Society and producer of Shirley poppies.

The Royal Medals of the Royal Geographical Society have, this year, with the approval of H.M. the King, been awarded as follows: Founder's Medal to Mr. Knud Rasmussen for his work in the Arctic; Patron's Medal to the Hon. Miles Staniforth Cater Smith for his explorations in Papua.

Sir J. J. Thomson received one of the John Scott Medal awards from the City of Philadelphia at a special meeting of the American Philosophical Society held during his visit to the city last April, and at the same time a similar award was made to Dr. Aston. These medals are given on the recommendation of an advisory committee representing the National Academy of Science, the American Philosophical Society, and the University of Pennsylvania.

The Hansen Prize for distinguished microbiological work has this year been awarded by the Danish trustees to Dr. E. J. Allen, Director of the Marine Biological Association Laboratory at Plymouth.

The Anders Retzius gold medal of the Geographical Society of Stockholm has been given to Sir Aurel Stein for his archæological research in Central Asia.

Dr. C. K. Ingold has, for the second time, been awarded the Meldola Medal of the Institute of Chemistry.

Prof. W. Duane, of Harvard, has received the Comstock Prize of the American National Academy of Sciences for his work on X-rays, and Dr. A. W. Hull, of the G.E.C. Research Laboratory, the Howard W. Potts gold medal of the Franklin Institute, for his work on crystal structure.

Sir David Bruce has been nominated President of the British Association for the meeting in Toronto next year.

Prof. A. C. Seward has been elected President of the Geological Society; Prof. A. Barr of the Optical Society, and Dr. J. L. E. Dreyer of the Royal Astronomical Society for the coming session.

Prof. A. V. Hill has been appointed to the Jodrell Chair of Physiology at University College, London, in succession to Prof. Starling; and Dr. R. W. Whytlaw-Gray, Professor of Chemistry in Leeds University, in succession to Prof. A. Smithells.

The Royal Society has received the magnificent gift of £100,000 from Sir Alfred Yarrow to be used as capital or income as the Council may think fit, without any restriction, but with the excellent advice that the money should be employed for the payment of workers and apparatus rather than to erect costly buildings. The Royal Society also benefits to the extent of £50,000 from the death of the widow of the late Dr. Mond. Unusual as it is for any scientific institution to receive gifts on this scale, in the United States it is a matter of almost everyday occurrence; but even there the munificence of Mr. Arthur H. Fleming is worthy of record. Mr. Fleming has for many years been President of the California Institute of Technology at Pasadena, and has now handed over the whole of his fortune, amounting to about one million pounds, to that fortunate Institution.

As a result of an appeal from the editor of the *Field*, a sum of money has been promised for carrying out a research on the cause of distemper in dogs. The Medical Research Council has taken charge of the work and has appointed a committee, with Sir William Leishman as chairman, to investigate the matter and to endeavour to find a prophylactic. Practically nothing is known about the disease. It is possibly due to a filter-passer, and it is not improbable that the discovery of the cause of distemper might throw light on other diseases of a more vital human interest.

Letters from the Physical Institute, Copenhagen, relating to the elements hafnium (discovered by Coster and Hevesey) and celtium (Urbain and Dauvilliers), to both of which the atomic number 72 had been assigned by their discoverers, would seem to indicate that celtium does not exist at all. The spectral lines attributed to it appear to belong to lutetium—one of the two elements into which Urbain succeeded in dividing Marignac's original ytterbium.

It is stated that the results of the measurements of the photographs of stars taken by the Lick party, at Wallal, Australia, during the eclipse of the sun last September, show displacements which are in agreement with those calculated from Einstein's theory. Prof. Campbell, of the Lick Observatory, who was formerly not an adherent of this theory, is stated to regard the question as settled.

At a lecture delivered at University College, London, last May, Prof. H. A. Lorentz described the experiment which Prof. Michelson has now in hand for detecting any effect of the rotation of the earth on the velocity of light at its surface. By splitting up a beam of light into two parts which travel in opposite directions round the sides of a triangle, a first order effect on the interference fringes formed by the overlapping beams is obtained. It will be remembered that the effect sought by Michelson and Morley was only of the second order.

The five-year period during which the Government has given financial aid to the Industrial Research Associations is now drawing to a close, and scientists will watch with interest and no little anxiety their subsequent fate. It is to be feared that many trades will be unwilling to bear the cost. The Scientific Instrument Research Association will, it is believed, receive sufficient support to ensure its continuance, but the future of the Photographic Association is very uncertain at the moment.

The British Non-Ferrous Metals Research Association has hit upon a rather ingenious way of communicating the results of its research investiga-

tions to its members. Lectures are arranged at one or more centres to which as a rule only the members of the Association itself are admitted. Two objects are served in this manner: firstly, early confidential communication of the results of the research is assured to those who have given it financial support, and, secondly, the investigator gets into close and immediate contact with that section of the industry chiefly interested in his work. In this manner the future direction of work and the relative practical importance of its varied issues are subjected to the mutual consideration of researcher and the industry. So far three subjects have been dealt with by this private lecture system, and very large attendances have been obtained, but it would seem that the method can only be satisfactory for industries located in one or two centres in the country.

W. Heffer & Sons, Ltd., of Cambridge, have in the press a volume entitled *The Expert Witness*, by C. Ainsworth Mitchell, M.A., editor of *The Analyst*. This book is written on similar lines to, and is in many respects a sequel to, the author's *Science and the Criminal*, now in its second large edition. It gives an outline of the latest application of scientific research to the investigation of criminal problems, and also an account in non-technical language of the use of expert evidence of all kinds, illustrated by reference to old and modern trials. The nine chapters deal with the latest methods of identification by means of patterns on the feet; by the pores of the skin; by the detection of latent prints on paper, etc. The latest scientific methods of handwriting are also described, and an outline is given of the author's methods of estimating the age of ink in writing. In the description of Secret Writing there is an account of the scientific evidence given at the trials of German spies. The last chapter deals with expert evidence in art, and with the application of such scientific methods as the use of X-rays to identify old masters.

In these Notes last quarter an account was given of the investigations of the causes of Building Stone Decay carried out by Mr. J. E. Marsh, F.R.S., of Oxford. This matter is now to be investigated by a special committee of the Building Research Board of the Department of Scientific and Industrial Research, which will report on the best methods by which decay in building stones, especially in ancient structures, may be prevented or arrested. Sir Aston Webb will be chairman of the committee, which includes among other members Prof. C. H. Desch and Dr. Alexander Scott.

In his presidential address to the Institute of Chemistry on March 1, Mr. A. Chaston Chapman gave the membership figures of the Institute for the ten years 1913–1923. The number of Fellows has increased from 1,172 to 1,601; the Associates from 248 to 2,461! This increase has all occurred since 1918, and reflects the post-war enthusiasm for the study of chemistry.

Dr. B. Petronievics has sent us his pamphlet from the Smithsonian Reports, *On the Law of Irreversible Evolution*, in which he treats of Dollo's law of irreversible based on the Belgian palæontologist's own work.

At the first Dollo expressed his law as follows: An organism cannot return even in part to a previous condition already passed through in the series of its ancestors. Later Dollo expressed his law with greater exactitude: An organism never exactly renews a previous condition, even if it finds itself placed in an environment identical with the one through which it has passed. But by virtue of the indestructibility of the past, it always retains some trace of the intermediate stages which it has traversed.

Dollo, however, did admit the reversibility of conditions of existence; he wrote: "Evolution is irreversible as regards the structure of organisms, but reversible as regards environment (Ethology)."

Dollo will always be regarded, like Cuvier, as the founder of a great law of the organic world.

Some years ago, by means of agglutinin tests, four blood groups were

found in man. Von Dungern and Hirschfeld, and Learmonth and Ottenberg suggested that these blood groups in man depend for their inheritance upon simple Mendelian factors.

Ottenberg and Friedman investigated rabbits, and claimed to have found certain blood groups, but the work of later observers using a variety of animals, cats, dogs, sheep, swine, cattle, horses, rabbits, guinea-pigs, rats, and frogs, has failed to establish the presence of blood groups. In some cases evidence of agglutinins was found, but not of grouping.

The latest work on this subject is by E. C. MacDowell and J. E. Hubbard, who have shown that there is an absence of iso-agglutinins in mice.

If the blood groups submit to Mendelian analysis, they must be investigated in man: the writer knows of no work at present in blood groups in apes and monkeys.

We have received from the Wireless Press four books dealing with wireless telephony from the amateur's point of view which it will be convenient to refer to here. Mr. P. R. Coursey provides two of notable excellence (*How to Build Amateur Valve Stations*, pp. 70, with 70 figures in the text, paper covers, price 1*s*. 6*d*. net; and *The Radio Experimenter's Handbook*, Part 2, pp. vi + 72, with 63 figures, boards, price 3*s*. 6*d*. net). The first contains a simple explanation of the action of a thermionic valve, followed by explicit details concerning everything that an amateur requires to enable him, if he so desires, to construct his own set, from the erection of his aerial to the completion of a four-unit receiving panel, with a full statement of all necessary dimensions and quantities involved. The second volume is intended for those who desire to undertake serious experimental work. It contains, for example, formulæ, tables, and curves required for the design of inductances and capacities, and describes a number of simple but fundamental measurements to be made on valves and with valve circuits. The third volume (*Your Broadcast Receiver and How to Work it*, Percy W. Harris, pp. 68, with photographic illustrations, paper covers, price 6*d*.) contains a popular account of the subject suited to the needs of the purchaser of a receiving set who, having no scientific knowledge, still desires to know something of its mode of action and to obtain the best results from it. The last book (*The Amateur's Book of Wireless Circuits*, by F. H. Haynes, pp. 107 + 111 wiring diagrams, price 2*s*. 6*d*. net) is simply a collection of wiring diagrams with a bare minimum of description. Capacities of condensers and, occasionally, magnitudes of resistances, are given; but it would seem to the present writer that the book could easily have been made more useful and interesting than it now is. However, those who are interested in the various circuits used for the reception and transmission of wireless signals will assuredly find here all that they need.

A second report dealing with Dr. Alexander Scott's work on *The Cleaning and Restoration of Museum Exhibits* (H.M. Stationery Office, price 2*s*. net) has now been issued, the demand for the first report and numerous inquiries at the British Museum having shown how much the work is appreciated. The new report deals further with the cleaning and after-treatment of prints and pictures (including pictures on mural tablets and on silk), and also with the treatment of objects made of stone, earthenware, lead, silver, brass, and wood. Many new results have been obtained which will be found of the greatest possible value, and the pamphlet will be very welcome to all who have occasion to deal with objects of this kind.

ESSAYS

OILINESS AND LOW-SPEED LUBRICATION (F. J. Thorpe, B.Sc., Principal, Technical Institute, Wellingborough).

THE explanation of high-speed lubrication is based upon the fact that by virtue of its viscosity and density an oil insinuates itself between the relatively moving surfaces and forces them apart. The resistance to motion is then due only to the internal friction of the liquid. Its value depends upon the viscosity of the lubricant, the relative speed of the surfaces, their area and inclination to each other, and also upon the mean thickness of the lubricating film. When these conditions are known, the resistance offered to the relative motion of the surfaces can very approximately be calculated.

These facts were established by the researches of Beauchamp Tower and Osborne Reynolds over thirty years ago, and the theory has more recently been extended by Michell to thrust bearings, and developed in greater detail by Martin, Harrison, Sommerfeld, and others.

But our knowledge of the way in which lubricants act at low speeds and under high pressure is not nearly so complete even now, and the explanation, instead of being the solution of a problem in hydrodynamics as in the high-speed case, seems likely to come from the chemical or even from the ultra-molecular constitution of the ingredients of the oil and to involve some of the most recent work on the arrangement of atoms in the molecule, and even on the arrangement of electrons in the atom. In many practical cases it is impossible to form the complete film of oil between the moving parts. For example, we may take the cases of various worm gears, or the slow-moving pulleys of cranes, or slide valves of steam engines. Many very different kinds of viscous liquids, and even air, can be used as lubricants in high-speed work, but in these low-speed cases oils are the only liquids which can be used, and their value seems to depend on a particular property of oils which is distinct from their viscosity. It has become the custom to call this property "oiliness." In this sense we speak of the animal and vegetable oils as being in general more "oily" than the mineral lubricating oils.

The researches instituted by the Lubricants Inquiry Committee of the Department of Scientific and Industrial Research in 1919 (1) emphasised the importance of this property, and at the discussion on the subject at the meeting of the Physical Society (2) on November 28, 1919, most of the suggested explanations were reviewed.

It had been supposed that the superior lubricating powers of the fixed oils might be due to the change of their viscosity under the high pressure. If there is a great increase of viscosity with pressure, those oils which showed the greatest increase would obviously also be those which formed the most persistent film. In other words, we should expect the fixed oils to show the greatest increase of viscosity with pressure. The researches of Stanton and Hyde (1), however, showed that it is the mineral oils which display the greatest increase, so it is to be concluded that the greater oiliness of the fixed oils does not arise from this cause.

Another point of importance which differentiates low-speed from high-

speed lubrication is that it depends upon the metals of which journal and bearing are made, as well as upon the oils. Archbutt (2) speaks of a case in which he found that under the same conditions of speed and pressure, and with the same oil, bearings lined with white metal would carry double the load carried by bronze bearings without any increase of the friction.

Again, it has been the custom to speak as if the frictional resistance experienced when we try to slide one solid face over another is due entirely to the roughness of the surfaces, and that the higher the degree of polish we attain, the less will be the friction. Lubricants act in the high-speed case by lifting the surfaces so far apart that the little projections and roughnesses are clear of each other, and in the low-speed case by filling up the depressions in the rough surface and so preventing the projections from interlocking. No doubt these actions are going on in the majority of ordinary cases, but Hardy (3) has shown that it is far from being the case that increasing the degree of polish indefinitely reduces the static friction. On the other hand, he found that if two extremely smooth glass surfaces were placed together it was impossible to slide one over the other without tearing away the lower surface. He takes the view that in the ultimate case static friction is entirely due to cohesion between the faces. This cohesion depends upon the surface energy of the solid. The function of a lubricant is to reduce the energy of the surface, and thereby to reduce the capacity for cohesion and the resistance to slip when two composite surfaces (made up in some way of oil and metal) are applied the one to the other. Work is done by the forces of cohesion when the film of lubricant is applied to the solid face, and the surface energy is decreased by this quantity. A further quantity of work is done when the two oiled surfaces are applied to one another, with a further change in the surface energy. The resulting static friction is an unknown function of the total change in surface energy. Very thin films of certain liquids can prevent the molecules constituting the metallic surfaces from coming within their normal distance of cohesive attraction. What is the nature, then, of the association between oil and metal which gives minimum friction? Oiliness must be the expression of changes in the surface energy of both liquid and solid when in contact, and we can only speculate as to what these changes may be when we know something of the chemical constitution of the oils.

Vegetable and animal oils principally consist of the esters of fatty acids, each oil generally containing a mixture of several such esters. In olive and rape oil the chief ester is olein. The principal constituent of palm oil is palmitin, while in tallow and in castor oil we have the esters derived from stearic and ricinoleic acids respectively. Sperm oil consists mainly of esters of monohydric alcohols and is therefore regarded as a fluid wax. We may note, however, that all these esters are derived from fatty acids containing the carboxyl group COOH, and that in the ester the H of this group is replaced by some alkyl radicle. Mineral oils are very different indeed from these fixed oils in composition. They consist almost entirely of hydrocarbons. We owe much of our knowledge of them to Dunstan and Thole, who conclude that all good lubricating mineral oils contain unsaturated molecules (4). "In no case," say they, "has the chemical constitution of a component of a lubricating oil been established, but the chemical behaviour of these oils indicates that among the components are unsaturated hydrocarbons (possibly open chain, but more probably naphthenic and polynuclear or perhaps of both types), saturated hydrocarbons (naphthenic and probably to some extent polynuclear, but not to any appreciable extent paraffinoid), and aromatic hydrocarbons (to an unknown and possibly a limited extent). The unsaturated compounds constitute between 20 and 40 per cent. of most lubricating oils. It appears, then, that the true lubricant is an unsaturated compound, possessing all the attributes of such a compound, namely—

(1) Capacity to absorb iodine, bromine, oxygen, and so on.
(2) Solubility in strong sulphuric acid.
(3) Higher C/H ratio than the saturated derivative.

The oiliness of a mineral lubricating oil would then appear to be due to the unsaturated molecules of the lubricant entering into some sort of firm physico-chemical union with the metallic surface, thus forming a "friction surface" which is a composite of oil and metal. With the fixed oils it is the ester molecules which behave in this way. Now it may be that this composite surface is much more than one molecule thick, the oil penetrating some little distance into the metal and altering its physical properties, and some support is given to this view by the fact that the oil cannot be completely wiped off and that even a file does not cut a surface like this which has been oiled and then wiped as well as it does a clean metal surface. In order to get rid of the oil it is necessary to grind the surface under water or to slice off a thin layer. But the failure of lubricants is really due to the fact that in these thin layers the rate of shear may become so high that there is actually slip between the oil and the metal and that the lubrication breaks down in consequence. Fixed oils are the better lubricants under low-speed and high-pressure conditions, because in their case the layer in immediate contact with the metal can stand a higher rate of shear than the corresponding layer in the mineral oils. This view of lubrication and the cause of its failure seems to fit in better with the alternative theory that the oils, or certain constituents of them, are adsorbed rather than absorbed at the solid face.

The theory is briefly this. When an oil is in contact with a metal it ceases to be perfectly uniform throughout, but certain of its molecules concentrate at the surface of contact, and not only so, but at that surface they take up a particular orientation. In one case it is known that the surface is built up of rod-like molecules all packed tightly together standing on their ends. We may thus picture the surfaces of the bearing and journal as covered with a layer one molecule thick, resembling a piece of velvet firmly glued to the metal with the pile outwards (5). Two such velvet-like surfaces can glide over one another with very little friction. It is obvious that the formation of such layers must depend on the forces called into play between those molecules of the oil which are capable of adsorption and the molecules of the metal of which the bearing and journal are made. In other words, oiliness must depend not only on the oil, but also on the nature of the metal with which the oil is in contact. We have seen this to be the case.

What, then, is the evidence for adsorption, and what is the evidence for a special orientation of the molecules at an interface or composite surface? In the case of the fixed oils the esters and any free fatty acid molecules show tendencies to adsorption at a solid or even at a liquid surface with which the oil may be in contact. If we find the percentage of free acid in a sample of commercial olive oil, for instance, and then shake some of the oil with powdered glass and afterwards titrate a portion of it, we shall find that the percentage of free acid is now less owing to adsorption of the free acid having taken place at the surface of the powdered glass. When we examine the mineral oils we find their behaviour rather different. Dunstan and Thole consider that these oils must be included in the category of iso-colloids, that is, polyphase systems in which the disperse component is of the same chemical nature as the dispersion medium. Just as water must be regarded as a system in which molecules such as $(H_2O)_3$ coexist with simple H_2O molecules, so in a lubricating oil the disperse phase is a molecular aggregate suspended in a dispersion medium of simpler and similar structure. As evidence for this they cite the high viscosity, large temperature coefficient of viscosity, and especially the temperature-viscosity hysteresis effects characteristic of emulsoids observed by Glazebrook, Higgins, and Pannell (6). As long ago as 1905, Schneider and Just (7) were led to the same conclusion by ultra-

microscopic observations. Hardy states emphatically that a true lubricant is always a substance which is adsorbed at the solid face.

If we shake a pure mineral oil with powdered glass, what will be adsorbed?

It is difficult to examine directly the behaviour of an oil in contact with a metal in order to obtain therefrom any light on the question of a specific orientation of the molecules, but researches have been made on the behaviour of the oils and of the fatty acids on water which are very suggestive as to what may also happen when these oils are in contact with a metal bearing; and Harkins and Ewing (8) have shown that the adhesional work between an organic liquid and mercury is always greater than between the organic liquid and water. This may also be true for the solid metals.

In a recent paper P. Woog (9) has given evidence that the greasiness of fatty substances depends to some extent on the tendency of the molecules to coalesce, a tendency which should become more pronounced as the molecules increase in dimensions. It was found that the molecular volumes for a number of fatty oils are distinctly higher than those of mineral oils of corresponding viscosity, and that for mineral oils the greasiness runs closely parallel to the molecular volume. Now we have seen that adsorption takes place if there are molecular aggregates distributed through the body of the liquid, and thus it seems as though there will be the greatest adsorption with those oils in which the volume of the molecular aggregates is the greatest. Again, Langmuir (10) showed that films of the fatty acids spread upon the surface of perfectly clean water in layers only one molecule in thickness, and moreover that these molecules are all arranged in similar orientation. The molecules of the saturated fatty acids are attached to the water by their carboxyl groups. Oleic acid is attached by the carboxyl group and also at a point in the middle of the chain, and is bent down toward the water at the latter point. Palmitic acid lies with its molecules all perpendicular, with the carboxyl end, of course, on the water. Woog found measurement of films of the majority of fatty oils and of their principal constituents to give similar results. An exception was castor oil, which seemed to show asymmetry of its molecules. N. K. Adam (11) has examined thin films of palmitic acid on water, and had confirmed Langmuir's results. Thin films of fatty oils or fatty acids spread upon water into layers one molecule thick, but pure mineral oils gather up into drops. The difference in behaviour between fatty and mineral oils is also well shown if we attempt to find their interfacial tensions with water. A suitable form of apparatus is that described by Reynolds (12), which is a modification of the ordinary capillary rise method for surface tension. By fixing the capillary tube centrally inside a burette, filling the burette with water, and then turning on the tap so that the water drips out, at the same time pouring the oil in at the top to take its place, we can get the water-oil surface about half-way down the burette and clearly see the capillary elevation in the inner tube. Reynolds found that pure organic solvents with water gave a definite capillary rise, but that the presence of impurities often caused the rise to decrease with time. As this falling away was also characteristic of colloidal sols in contact with benzene, it was taken as being due to progressive adsorption at the interface. All the distillation products of petroleum tested by Reynolds showed similar results, which thus confirms the view of Dunstan and Thole that these are colloidal in structure. The writer has used this apparatus for fixed oils and for mineral oils, and has found that olive, rape, and neatsfoot oils, if neutral or containing not more than about 1·5 per cent. free acid, have a definite interfacial tension with water (as shown by the capillary rise, which takes a number of hours to establish itself, but then remains constant for days), but pure mineral oils show a decrease in the capillary rise with time. This decrease goes on generally until the level of the liquid is the same in the capillary as outside. The conclusion is that with the fixed (oily) oils there is a definite molecular concentra-

tion at the interface, while with the mineral oils (not so oily) there is a progressive adsorption from the interior, causing continual change in the state of molecular aggregation at the interface. It has been long known that the addition of a small quantity of a fixed oil to a mineral oil increases its oiliness, but Wells and Southcombe (13) have shown that a very small percentage of fatty acid added to a mineral oil produces the same effect as a much larger amount of the fatty oil. Some interesting figures relating to this effect have been given by Archbutt (14). The table shows the results of tests carried out by Deeley.

Pressure in lb. per square inch.	Straight Mineral Oil.	Same Mineral Oil and 5 % Free Fatty Acid.
	Static Friction.	
10	8·8	6·4
20	18·8	12·4
30	27·2	21·2
40	34·8	27·1
50	45·4	31·2
60	58·0	39·1
70	61·7	42·1
80	66·5	50·0
90	89·5	57·6
100	107·1	64·7

Again, some tests with the Lanchester worm gear showed that the addition of this small quantity of acid enabled the mineral oil to retain its lubricating properties at a higher temperature than the pure oil. Pure mineral oil gave a constant efficiency of 95·95 per cent. up to 100° F., after which the efficiency fell off suddenly and continued to do so. Mixed oils (5 per cent. free fatty acid) gave 96·2 per cent. up to 120° F., and then the efficiency only fell off gradually. Wells and Southcombe have made up a special mixture called Tonicol, which, when added in small quantities to a pure mineral oil, increases its lubricating power in the same way that a fatty acid does. Since the addition of this small percentage of acid to a pure mineral oil has such a marked effect on its lubricating power, we should naturally expect it to have some effect upon the interfacial tension. This is in fact the case. In general, the addition of the small percentage of acid has the effect of making the rate of falling away of the capillary rise less, and there appears to be one particular percentage which when added to an oil completely stabilises the interfacial tension and makes the mineral oil behave like a fixed oil. Experiments of this nature have only been made on a few oils, but the evidence seems to point to the fact either that the acid itself is adsorbed at the interface and takes up a definite molecular orientation, or that it causes molecular aggregates definite in form and orientation to be adsorbed at the interface. Of course, it does not follow that the molecular behaviour at an oil-metal interface will be the same as at an oil-water interface, but the behaviour in the one case may certainly throw some light on the behaviour in the other. This view is strengthened by the experiments of Harkins and Ewing (8 *loc. cit.*).

Hardy (15) has pointed out that a knowledge of the chemical constitution alone of a liquid will not enable us to determine its lubricating power. We must also know the metal with which it is to be in contact. For instance, water is an antilubricant for glass, a feeble lubricant for bismuth, and an admirable lubricant for ebonite. Acetic acid is a good lubricant for glass

and a feeble lubricant for bismuth. Does this mean that with any given oil only certain solids are capable of producing that particular adsorption and orientation of the molecules which will give the most complete lubrication ? Willows has suggested an explanation along these lines. But by experimenting with one metal only, namely bismuth, Hardy has been able to form a few conclusions as to the effect of the chemical constitution of the oil. He does not agree with Dunstan and Thole that unsaturation is essential, but he states that in most homologous series lubrication increases as molecular weight increases, and moreover that this rule holds true even when the members of the series change from liquids to solids at the temperature of observation. The influence of chemical constitution is, however, extraordinarily complex, and the only absolutely general rule he has found is that the introduction of a single hydroxyl group into a molecule increases its action as a lubricant. More hydroxyl groups may have the opposite effect. In general the single hydroxyl group is very much more effective attached to a ring or closed chain nucleus than to an open chain nucleus. The worst lubricants are simple symmetrical compounds, such as benzene and carbon tetrachloride, but putting H in place of Cl or CH_3 in place of H increases lubrication.

How does the adsorptive effect of the metal on certain molecules of the oil actually bring about a lubricating effect ? For an explanation we have to get right back to our fundamental conceptions of the structure of matter. Hardy describes how, if we start with two liquids whose surface tensions are practically equal, *e.g.* benzene and propionic acid, and place a drop of each on a clean glass plate a few millimetres apart, the drop of acid which is the good lubricant will chase the non-lubricant benzene to the edge of the plate. This is direct evidence that the forces of attraction operate more strongly between a solid face and a good lubricant than between it and a bad lubricant. The better lubricant is obviously more strongly adsorbed by the solid face. Willows (16) looks to the theories of Thomson and Langmuir to give us the explanation of this adsorption. In certain molecules some of the atoms are supposed to have lost and some to have gained electrons ; hence these molecules will exhibit the properties of electrical doublets, and will be surrounded by a strong electrical field. Thomson shows, from measurements of dielectric constants, that certain chemical groups, like OH and COOH, are especially marked in this respect. Thus cohesion of a liquid—and by implication the surface tension and viscosity also—must arise, in part at all events, from the attraction between such doublets.

Extending this to the case in which the liquid is in contact with a metal, it is well known that the electrons in metals are in a state of considerable freedom, hence when a liquid whose molecules possess fields of force comes near a metal, it may have the power to bind certain of the electrons from the metal, so creating an electrical double layer, whose negative portion is in or near the oil. This is adsorption, and if the molecular aggregates in the metal should also possess electrical fields the effect will be increased. There will thus be considerable cohesive forces between the solid and the liquid, and the viscosity of the oil will be large. Thus two of the main characters of a good lubricant—adhesion to the solid and high viscosity—can be accounted for, and such friction as there is will be between the adsorbed layer and the liquid layers near it. The previously mentioned results of Harkins and Ewing seem to confirm this view of the action of a metal.

The most important factors are thus seen to be (1) the amount of lubricant which is necessary to form the contact film, (2) the chemical constitution of the lubricant, (3) the composition of the solid faces. Concentrating his attention on these three points, Hardy (17) has been working with the normal paraffins and their related acids and alcohols against bismuth, glass, and steel. He concludes with regard to (1) that the friction is independent of

the quantity of lubricant present, provided there is enough to cover the surfaces with a certain essential primary film. The critical value of the friction, *i.e.* the point at which it ceases to depend on the quantity of oil, is when the lubricated faces are just in equilibrium with the saturated vapours of the lubricant present. This critical value is, of course, also the lowest value of the friction for this particular case. The lowering of the friction is proportional to the concentration of the molecules of the lubricant in the gas phase.

Secondly, he finds that if he takes each chemical series in turn, *e.g.* the paraffins, or their corresponding alcohols, when the coefficient of static friction for each of these was plotted against the molecular weight the curve was found to be a straight line.

Thirdly, the effect of the nature of the solid face was unexpectedly simple, for in changing from glass to steel the curve for a series is merely moved parallel to itself, and in moving from steel to bismuth there is a further shifting.

There is no doubt now that it is actually the molecules which form the contact layers which are the most important.

As an alternative to the colloidal structure of lubricating oils as suggested by Dunstan and Thole, Brillouin (18) has given another possible explanation of their behaviour. Dealing first with a thick film between two planes, one of which is moving while the other is at rest, he considers that there is evidence that the lateral movements of lubricants transverse to the direction of motion (of the one plane over the other) are due to an elastic resistance which is called into play, and he considers also that the characteristic properties of lubricating oils are due to the fact that they are not liquids in the true sense of the word, that is, they are not isotropic, but that they are anisotropic, only possessing true fluidity in one or at most two directions.

Practical lubricants, he considers, are probably very concentrated solutions of a crystalline solid in a very small quantity of a very viscous isotropic fluid, *i.e.* solutions which, according to the degree of concentration, will be solids, anisotropic fluids, or liquids. It is possible that the limits of these three states may be displaced by the heating, or by the addition of liquid. It is the intermediate (anisotropic fluid) state which lubricates. Possibly the neighbourhood of solid surfaces may also influence the properties of the lubricant, and in the superficial layer the lubricant is perhaps entirely solid while anisotropically plastic throughout its mass.

In this direction we may find another explanation of the increased lubricating powers conferred by the addition of the small percentage of fatty acid as discovered by Wells and Southcombe, for Labrouste (19) has shown that certain films of fats and fatty acids spread upon water may be in the solid state when under pressure at temperatures above the melting-point.

Experimenting on films of substances melting between 0° and 100°, he found that the film remained solid up to a certain temperature, after which it became increasingly brittle as the temperature rose, until the solid aspect completely disappeared and liquid droplets were formed. If, however, at this last temperature the film is compressed until its surface is reduced to that occupied in the cold, it seems to revert to the solid state. The clearest results were obtained with trimyristin, but oleic and palmitic acids gave similar effects, though the transformation points were not so clearly defined.

It would appear, then, that there are many interesting investigations now proceeding, which, though not all primarily undertaken for that purpose, may throw a great deal of light on the behaviour of oils in contact with metals, and especially on the ability of good lubricants to resist breaking down under the abnormally high rates of shear developed under low-speed conditions. The problems, like those of that other topic of such interest to the engineer, froth flotation, seem as if they will receive their solution

at the hands of the pure physicist or colloid chemist, working on the subjects of surface tension, adsorption, and molecular structure.

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REVIEWS

MATHEMATICS

A Treatise on the Integral Calculus, with Applications, Examples, and Problems. By JOSEPH EDWARDS, M.A. Vol. II. [Pp. xv + 980.] (London: Macmillan & Co., 1922. Price 50*s.* net.)

THE second part of this voluminous work continues in the spirit of the first. The author's object is clearly to present as large an assemblage of facts about the integral calculus and its applications as can well be collected into one book, and he carries out his object without any very obvious regard for the more modern spirit of mathematical analysis. Perhaps one may be disposed to wonder why Mr. Edwards chose those particular topics which he has included in his treatment of the subject: thus why does he include Legendre Coefficients and the Tesseral and Sectorial Harmonics, while the equally important Bessel functions are hardly mentioned? But on the whole the second volume is very comprehensive in its outlook, dealing as it does with Eulerian Integrals, and Beta, Gamma, and Gauss's Π Functions, many types of Definite Integrals, Contour Integration, Elliptic Integrals and Functions, Calculus of Variations, Fourier Series, Probabilities, Combination of Observations, and Harmonic Analysis. What one misses in Mr. Edwards' discussion of these topics is some kind of unifying principle, a common thread of argument, that could help to make the reader grasp the fundamental conceptions of modern investigations in these matters.

It seems that Mr. Edwards' book suffers from the effects of the very long delay experienced in its production: it has become too big and pretentious for the sort of mathematical atmosphere it betrays. Honours students at many of our universities would have been very grateful to Mr. Edwards if he had issued a one-volume work on the Integral Calculus, comparable in size, scope, and cost with his well-known *Treatise on the Differential Calculus*. The modest but important function of teaching students the elements of the subject, and affording them the opportunity for acquiring the necessary manipulative skill, is one that Mr. Edwards can perform with credit and in a manner pleasing to the learner. But a work in two volumes, comprising 2,000 pages, and costing five pounds, must offer more of permanent value to the scholar than, apparently, Mr. Edwards has the desire to produce.

Cours de Physique Mathématique de la Faculté des Sciences. Par J. BOUSSINESQ. **Compléments au Tome III, Conciliation du véritable déterminisme mécanique avec l'existence de la vie et de la liberté morale.** [Pp. xlviii + 217.] (Paris: Gauthier-Villars et Cie, 1922. Price 30 frs.)

THE main topic of this book is made clear by the sub-title, but it also contains a multiplicity of minor points. In the first part with Roman pagination the writer discusses the possibility of the existence of the sort of ether required for optical phenomena, and a problem on the rotation of fluid with surface tension. He then paves the way for the second part of the book, describing how he had written on the question of mechanical determinism and moral freedom over forty years ago, quoting at length the referee's

statement on a paper on the subject submitted in 1878 to the Académie des Sciences morales et politiques, and discussing the agreement between his own view of the matter and the most accredited opinions on the nature of vital phenomena.

The second part with Arabic pagination is essentially a reissue of the work published by the author in 1878. At bottom the idea is that moral freedom can exist concurrently with mechanical determinism, if we assume the existence of a directing agency whose influence does not affect the mechanical quantities involved in any course of action. To conceive the possibility of such an agency, Prof. Boussinesq suggests that in a mechanical problem a solution which satisfies all the conditions may be "tangential" to another satisfying the same conditions, just as the solution of a differential equation is tangential to the singular solution when it exists. The directive agency would then decide which solution to follow, without having to add or subtract any momentum or energy.

Many remarks of an interesting character on various mechanical and philosophic questions are made by this veteran exponent of mechanical principles and their physical applications.

ASTRONOMY

The Mathematical Theory of Relativity. By A. S. Eddington, M.A., M.Sc., F.R.S., Plumian Professor of Astronomy in the University of Cambridge. [Pp. x + 247.] (Cambridge: at the University Press, 1923. Price 20*s.* net.)

This volume is a development of a mathematical supplement which was written for the French translation of the author's *Space, Time and Gravitation*. The latter work was an exposition of the fundamental ideas which underly the generalised relativity theory, which did not call for any extensive knowledge of mathematics on the part of the reader. The necessity for a revision of the older physical ideas (as we may now call them) was traced out, and it was shown how the new theory provided a rational explanation of the difficulties which the older ideas had encountered. In the present volume, Prof. Eddington assumes a general acquaintance with the discussion given in *Space, Time and Gravitation*, and proceeds to develop the mathematical formulation of the new conceptions and to follow out their consequences.

A summary of the principles of the absolute differential calculus is first given. The necessity for the use of this calculus, without which the theory could hardly have been developed, is admirably summarised in the following passage:

"I do not think it is too extravagant to claim that the method of the tensor calculus, which presents all physical equations in a form independent of the choice of measure-code, is the only possible means of studying the conditions of the world which are at the basis of physical phenomena. The physicist is accustomed to insist (sometimes quite unnecessarily) that all equations should be stated in a form independent of the units employed. Whether this is desirable depends on the purpose of the formulæ. But whatever additional insight into underlying causes is gained by stating equations in a form independent of units, must be gained to a far greater degree by stating them in a form altogether independent of the measure-code. An equation of this general form is called a *tensor equation*."

And again: "A tensor expresses simultaneously the whole group of measure-numbers associated with any world-condition; and machinery is provided for keeping the various codes distinct. For this reason the somewhat difficult tensor calculus is not to be regarded as an evil necessity in this subject, which ought, if possible, to be replaced by simpler analytical

devices; our knowledge of conditions in the external world, as it comes to us through observation and experiment, is precisely of the kind which can be expressed by a tensor and not otherwise." We have emphasised these passages as it is thought by some that there should be a simpler way to the theory which would avoid the introduction of the differential calculus.

Following upon the development of the tensor calculus, successive chapters deal with the Law of Gravitation; Relativity Mechanics; Curvature of Space and Time; Electricity; World Geometry. Thus we are led from the more elementary conceptions into the parts of the theory which are more speculative and more uncertain. It is not all easy reading, even for the expert mathematician, but the whole possesses that admirable lucidity which is characteristic of Prof. Eddington. In the mathematical portions, the successive stages of the arguments are given in sufficient detail for the reader to have no difficulty in following.

Prof. Eddington's report to the Physical Society greatly helped to spread a knowledge of the mathematical bases of the theory in this country in the early days of its development. The report was, as might be expected under the circumstances, far from perfect in some respects, and we are grateful to him for having now prepared this valuable account of the mathematical theory. It should be read with appreciation by many.

H. S. J.

Introduction à la théorie de la Relativité. Calcul différentiel absolu et géométrie. Par H. Galbrun, D.Sc. [Pp. x + 460, with figures.] (Paris: Gauthier-Villars et Cie, 1923. Price 60 frs.)

For a thorough understanding of the generalised theory of relativity, a knowledge of the absolute differential calculus as developed by Ricci and Levi-Civita is essential. To probably the majority of students of the subject, the original papers in which this calculus was developed are not easily accessible. It is true that many of the books which deal with the mathematical side of the theory contain a more or less complete account of the fundamental ideas which underly it and of their development in so far as they are required for the exposition of the relativity theory. By this means the student may acquire a sufficient knowledge to follow the development of the physical theory, but not to become really at home with it to such an extent that he can proceed to further developments himself. Moreover, different authors develop the subject from different points of view, and their correlation is not always easy for the beginner.

The appearance of the book under review is therefore very welcome. It provides a connected account of the absolute differential calculus together with an account of some of its applications. The treatment is somewhat detailed, and in order that the student may never lose sight of the fact that summations are always being dealt with, the summation sign is never omitted. This is probably an advantage at first, as it keeps the meaning of the formulæ present to the mind of the student and makes the process of changing dummy indices and suffixes intelligible. If this is not done, there is a tendency for the beginner to lose sight of the fact that summations are intended. But thereby much of the compactness of the formal expression of the theory is lost, and it is to be regretted that after the first few chapters the author did not abandon the summation sign.

On the other hand, the use of a special symbol to denote covariant differentiation is to be recommended. The author uses throughout the symbol A_{ν}, and so avoids the confusion which is liable to occur when $A_{\mu\nu}$ is used both for any covariant tensor of the second order and for the covariant derivative of the covariant tensor of the first order. The author's notation may not be the best, but some distinction seems desirable.

In dealing with the geometry of a non-Euclidean space of n dimensions,

Levi-Civita's method of considering the space as a section of a Euclidean space of order m greater than n is fully discussed as well as the geometry of Weyl applicable to the same space. Weyl's notion of "affine connection" is very clearly explained. The chapter dealing with Weyl's geometry forms a fitting introduction to Weyl's own treatment in his *Raum-Zeit-Materie*, which is very condensed and difficult for a student approaching the subject for the first time.

The volume closes with a full account of the application of the calculus to the restricted theory of relativity. The generalised theory is not touched upon, but a student who has first mastered this volume will find that that contains no terrors for him. As may perhaps be expected from a French author, the treatment throughout is admirably clear and logical.

H. S. J.

The Quantum Theory. By Fritz Reiche, Professor of Physics in the University of Breslau. Translated by H. S. Hatfield and Henry L. Brose. [Pp. v + 183.] (London: Methuen & Co., 1922. Price 6*s*. net.)

The Quantum Theory has been developed very rapidly within the last few years and has achieved some notable successes, particularly in the explanation of the fine structure of spectral lines. As many of the original papers bearing on the theory have appeared in foreign publications which are in many instances not easily accessible to the general scientific reader or which he probably does not see regularly, it is difficult to keep touch with the latest developments. The volume under review will be found of great value in providing a general account of the present state of the theory. It is somewhat condensed, but as very full references are given throughout to the original papers, means are provided by which the reader will be able to find further information on any particular details in which he may be especially interested.

The author does not assume the reader to possess any great knowledge of mathematics. The results of mathematical investigations are quoted and, in the case of the more elementary results, brief proofs are given amongst the notes at the end which may be read with profit by the reader who possesses some mathematical training. These notes are mixed up with the references to the original papers quoted. It would have been more comfortable for the reader if they had been separated. It is distracting to break the thread of the argument to have to refer continually to the end of the book for a note, only to find that it is nothing but a reference to original literature. These one may pass over in reading, whereas it is desirable to read the mathematical and other notes along with the general argument.

As is frequently the case with German authors, there is a tendency to pass over the work of English and other non-German scientists. The work of Jeans, for instance, is barely mentioned. Yet Jeans's proof that Planck's law of radiation necessarily involves something of a quantum nature was an important step in the development of the theory. The book is not without a number of misprints. Thus in Note 15, $\sigma = \pi\gamma$ should read $\sigma = 2\pi\gamma$. On p. 105 an Angstrom unit is tacitly but incorrectly assumed to be 10^{-8} cms. But no serious error was detected, and the reader is not likely to be misled by such errors as were noticed.

H. S. J.

PHYSICS

Colour Vision. By W. Peddie, D.Sc., Harris Professor of Physics, University College, Dundee. [Pp. xii + 208.] (London: Arnold & Co., 1922. Price 12*s*. 6*d*.)

Such a book as this of Prof. Peddie's has long been needed. It gives for the first time in English what may be termed a systematic account of the

generalised Helmholtz theory of colour vision. As compared with the chapters on colour vision in the *Physiologische Optik* the statement here is better arranged and is modified somewhat to meet the work that has been done since Helmholtz's time. The author has also extended Helmholtz's work successfully in several directions, but with unusual modesty makes no reference to his own work.

The elementary textbooks on physics usually assert, that according to Helmholtz there are three primary colour sensations, and that the colour-blind fall into definite classes, red blind, green blind, etc. Prof. Peddie points out that the second of these statements is not essential to the theory. The latter (p. 29) consists (1) in the recognition "that all colours can be compounded of three definite and independent colours, and (2) in the recognition that this fact implies further the co-existence in the perceiving organ of three independent and mutually non-interfering activities." The step (1) is universally accepted; (2) has been a source of controversy for years, and the reviewer is of the opinion that Helmholtz's mathematical treatment does not require so narrow a basis as it affords.

The application of Fechner's law, with all that it entails, is probably the most interesting and useful portion of the book, and will come as a novelty to nearly all the readers. But it ought to have been stated that the method of determining the fundamental sensations referred to on p. 133 led to results very different from König and Dieterici's and Abney's generally accepted values. R. A. H.

The Structure of Atoms. By Prof. Alfred Stock, University of Berlin. Translated from the 2nd German Edition by S. Sugden, M.Sc., A.R.C.Sc., A.I.C. [Pp. viii + 88, with 18 diagrams.] (London: Methuen & Co., Ltd., 1923. Price 6s. net.)

This small but well-written book is primarily intended for the benefit of students of chemistry, and to all such whom the demands of specialised study or research have hindered from keeping in touch with recent far-reaching advances in physics we can heartily recommend it. It presents in book-form the substances of a series of lectures on "Ultrastrukturchemie," and practically is a brief account, without mathematical details, of the contributions of pure physics to the essential problem of chemistry, atomic and molecular structure.

The contributions of various branches of physics are considered in turn, optics, electricity, radio-activity and crystal-structure, and though the scope of the book forbids more than a rapid review, yet sufficient is said to put the harassed chemist, so frequently forgetful of his debt to physics, in the way of what has lately been done in a subject which has almost adopted atomic and molecular theory wholesale. The translation is good, and the treatment is fair in that it indicates the most promising lines of development of the theory without unduly emphasising any aspect by extending it into realms where the evidence or argument is as yet conflicting. We have only one fault to find, and that is that the author has perhaps a tendency to be a little forgetful of the part played by the English-speaking races in the development of atomic theory. But probably this attitude is only subconscious, though it is particularly noticeable in the section on Crystal-structure.

Many of the remarks on crystal-structure, such as, in referring to the Bragg method, "This method is not, however, suitable for complicated crystals," need modification in the light of results which have been published in this country since the book was written. But such defects are inseparable from any scientific work in this age of rapid progress.

The book is not calculated to afford much profit to advanced students of physics. But it is very suitable as a non-technical supplement to more specialised works on kindred branches of science. W. T. A.

Advanced Laboratory Practice in Electricity and Magnetism. By E. M. Terry, Ph.D. [Pp. ix + 261, with 130 figures.] (New York and London: McGraw-Hill Book Co.. Price 15*s.* net.)

Dr. Terry's book is designed to meet the practical requirements of students of electricity in the more advanced stages of their course. It provides for a systematic series of experiments of a very up-to-date character and contains, in addition to useful practical hints, a great deal of explanatory theoretical matter. The material dealt with falls naturally into three main divisions, (*a*) steady current (non-inductive) work, (*b*) variable current (inductive) work, and (*c*) what must, for the lack of a better term, be called "the modern physics."

The first division deals with the measurement of simple electrical quantities, Resistance, Current, E.M.F., etc. It is true that one is inclined to regret the omission of some well-established experiments, such as the calibration of a bridge wire, the absolute determination of a capacity with a ballistic galvanometer and electro-chemical measurements—the more, indeed, on account of their instructional value than for their intrinsic merits. The introductory chapters on units and on instruments will be found specially useful. That part of the book which deals with alternating currents and alternating-current bridge methods is probably its most valuable section. Chapter x might perhaps have been omitted, since it contains such matter as is exhaustively discussed in the standard theoretical works, but chapters xi and xii will pay for careful study, and the experiments suggested will be welcomed by those who have spent much time and more patience on the old-fashioned direct-current bridge methods for the measurement of inductive constants.

Discharge through gases, Radioactivity, Valve work and Pyrometry form the third section of the work. Discharge through gases is a subject of some difficulty in laboratory application for the general student, involving, as it does, fairly good glass-blowing technique and a considerable amount of time. The author has done well to limit himself to a few well-chosen experiments, but even then probably experiment 32 will be within the capabilities of but few. It is a pity that more precise experiments on radioactivity, such, for example, as the range of α's and the absorption of β's have not found a place in this otherwise comprehensive work. The letterpress is very good, and the diagrams and numerous illustrations excellent. Many teachers will doubtless envy Dr. Terry his laboratory equipment, but will surely find consolation in the knowledge that the improvisation of apparatus is not the least valuable part of a student's training.

R. C. Richards.

La Molécule. Equilibres et Réactions Chimiques. Les Edifices Physico-Chimiques. Tome II. Par Dr. Achalme. [Pp. 232, with 149 figures.] (Paris: Payot et Cie, 1922. Price 15 frs.)

The author applies his views of the structure of the atom (Science Progress, 1922, 65, 157) to the formulation of chemical compounds, to electrolysis in aqueous solution, and to the kinetics of chemical change.

The dynamic theories of the atom, developed largely to explain phenomena met with in radio-activity and spectroscopy, lack that simplicity which is so essential in dealing with chemical problems. Although at present many difficulties lie in the way of their application to the formulation of chemical molecules, it must be recognised that they have been extraordinarily successful in their limited field. One is thus unable to agree with the violent and polemical attack made by the author of this book on the planetary atom of the mathematical physicist. Neither is any useful purpose served in putting forward a non-nuclear theory of atomic structure which ignores the work of

Rutherford and his collaborators, unless it can be shown that this theory possesses marked advantages in correlating hitherto unclassified phenomena.

The acceptance of the structure of the carbon atom put forward by the author, according to which the four valencies lie in one plane, would necessitate a return to a view of carbon compounds which has long been discarded by organic chemists. The van't Hoff tetrahedral arrangement of the carbon valencies in space was a fundamental advance on which the whole of modern organic chemistry is based. None of the applications of this fantastic theory of the atom to chemical processes appears to warrant its insinuation into a series of volumes purporting to give a rational interpretation of all chemical and biological phenomena.

W. E. G.

CHEMISTRY

A Dictionary of Applied Chemistry. Edited by SIR EDWARD THORPE, C.B., LL.D., F.R.S. Vol. IV, revised and enlarged edition. (L to Oxydisilin.) [Pp. viii + 740, with illustrations.] (London: Longmans, Green & Co., 1922. Price 60*s.* net.)

WITH the issue of the present fourth volume the new edition of the *Dictionary of Applied Chemistry* is approaching completion, and the increase in the number of volumes required to cover our present-day knowledge of the subject is alike a tribute to the thoroughness with which it is treated and to the ever-growing importance of chemical science and practice.

Generally speaking, the book follows on the lines of the previous edition and calls for no special comment. The now classical article on Naphthalene by Prof. Wynne has been revised and expanded so that it occupies 106 pages, thus constituting a veritable treasure chest of information on this important substance; Prof. Partington's excellent summary of the present Nitrogen Fixation situation, occupying thirty-seven pages, gives a good picture of the state of development of the industry, and Dr. Rosenhain's careful account of the subject of Metallography succeeds in compressing much valuable information within the limited scope of twenty-three pages. On less important matters Dr. Whitely's short résumé of the literature and properties of malonic acid and its derivatives should be of great value to many research workers.

The only adverse comments that may be made are, firstly, the lack of cross-references between articles; for instance, in Dr. Hart's article on the manufacture of nitric acid, the synthetic production from ammonia is merely touched on (pp. 567–8) and the reader is referred to other sources of information for details as to cost, efficiency etc., which can, as a matter of fact, be found some thirty pages later under the heading of "Nitrogen, Atmospheric, Utilisation of."

Secondly, the value of the work is greatly reduced by the complete absence of an index such as is found in foreign encyclopædias. As an example the first heading in the book may be taken, namely "L-Acid. 1-Naphthol-5-Sulphonic Acid." If one is duly familiar with the *Dictionary*, this may be taken to indicate that further details may possibly be found under "Naphthalene." As already noted, Prof. Wynne's article on this subject is 106 pages long and unindexed in any way, so that, although the writer is fairly familiar with these matters, it required several minutes' careful searching before the right paragraph could be found.

It is much to be hoped that the editor and publishers will be able to see their way to issuing a complete index to the *Dictionary* when the last volume is published, as by so doing they will double the value of the work.

F. A. M.

The Elements of Fractional Distillation. By CLARK SHOVE ROBINSON. [Pp. ix and 205, with 41 diagrams.] (New York: McGraw-Hill Book Company; and London: 6 Bouverie Street, 1922. Price 12s. 6d. net.)

ALTHOUGH books with a similar scope have been published previously in France and Germany, this is the first volume to appear in English which deals with the application of the principles of the phase rule, and of fractional distillation to operations on the plant scale. It is intended mainly for engineers and plant operators, and the theoretical principles involved in fractionation have been explained briefly with this end in view. This summary, though necessarily somewhat disconnected, is a useful survey of the theoretical basis of fractionation. The continuous and discontinuous methods of fractionation are considered in the light of the methods of analysis due to W. K. Lewis, and a special case, the fractionation of water, and acetic acid are discussed in detail. The best ideas of chemical engineering are applied to the design of continuous and discontinuous stills, fractionating columns, condensers, and other accessories, and four industries are chosen as illustrative of the principles involved. Modern apparatus is discussed dealing with ammonia recovery, benzolised wash oil, methyl alcohol, and ethyl alcohol. At the end of the volume is given an appendix containing a large number of reference tables of data useful in the design of fractionating columns. This treatise should find a place in the libraries of all chemical engineers, and others interested in this branch of chemical technology.

W. E. G.

Colloid Chemistry of the Proteins. By PROF. WOLFGANG PAULI. Translated by P. C. L. THORNE. [Pp. xi and 140, with 27 diagrams and numerous tables.] (London: J. and A. Churchill, 1922. Price 8s. 6d. net.)

THE sudden expansion of scientific knowledge of the proteins, which owed its origin to Emil Fischer, gave a broad idea of the constitution of the protein molecule, and the manner in which its component parts were coupled together. With this gain established, however, further progress became very slow, and it appeared that a detailed knowledge of the constitution of even the simplest protein was unattainable. Fortunately proteins are colloidal substances, and colloid chemists, among whom Prof. Pauli is one of the foremost, have seized upon their colloidal properties to advance yet further our knowledge of their constitution and properties.

In this small volume the author gives an account of the recent work on the physical chemistry of proteins, which is widely illustrated by numerous tables and diagrams. Owing to the amphoteric character of a protein, containing as it does free carboxyl, amino groups, etc., it may function as an anion in alkaline solution, or as a cation in acid solution. The author shows how the variations in H˙ and OH′ concentrations affect the degree of ionisation of the salts formed, its behaviour in the electric field, the viscosity of its solutions, and its ease of precipitation from aqueous solutions by other solvents. At the isoelectric point, the protein as a whole is neither positively nor negatively charged, and in general is less ionised, and hence is less viscous and possesses a lower osmotic pressure than under any other set of conditions. Numerous physical methods are given of determining the isoelectric point of proteins. Light is thrown by means of physical measurements on the rôles played by the terminal amino groups, and of the imino group of the peptide linkage in acid solutions, and the slow alterations in the state of the alkali proteins with lapse of time. An account is given of some of the physical properties of the degradation products of proteins, and of changes in the mobility of the protein ions with alteration in the hydrogen-ion concentration. Portions of the book are not very easy to read owing to a certain haziness of description: thus on p. 41 " the dissociation of OH′ and H· ions." On p. 122,

first and second paragraphs, a confusion has arisen in regard to the meanings of the terms basic, neutral, and acidic salts.

The book, which is provided with a good index, forms not only a valuable contribution to original knowledge, but is also a very useful summary of the facts of the colloid chemistry of the proteins. W. E. G.

Handbook of Chemical Engineering. DONALD M. LIDDELL, Editor-in-Chief. In two volumes. [Pp. 1008.] (New York: McGraw Hill Book Company; and London: 6 Bouverie Street. Price £2.)

IN this handbook eighteen chapters are devoted to various phases of the problems confronting the chemist and chemical engineer in industry.

The sciences of chemistry and engineering are now so specialised that an expert in one field cannot hope to be more than the merest tyro in another, and a book compiled by various contributors to serve both ends must necessarily prove somewhat unequal in thoroughness of treatment. In this respect the present volume is no exception.

By far the best chapters in the book are those devoted to the problems of leaching, evaporation, thermometry, refractories, distillation, oxidation, and reduction, whilst a good deal of useful and accurate information has been presented in a very readable form in the chapters devoted to transportation, refrigeration, catalysis, colloid chemistry, the radio-active elements, and the rare gases.

It is a pity that such a small amount of space is devoted to the increasingly important subject of ore flotation in the chapter on concentration, whilst the article on high temperature production is rather too much compressed both in theoretical treatment and in engineering data to be of much practical utility. Other chapters include information on mixing and kneading, power generation, crushing, grading, crystallisation, sampling, fermentation, electrochemistry, smelting, lutes and cements, the rare metals, whilst the last three sections are devoted to considerations such as construction, material, plant, design, and methods of financing.

The typographical mistakes such as "mediums" for "media" (p. 656), "Ramsey" for "Ramsay" (p. 916), are comparatively few in number, whilst the same is true for the more serious errors in statement, *e.g.* (p. 933) "rust is ferric hydroxide," $FeCrO_3$ for $FeCr_2O_4$ (p. 946). Certain sections, such as those dealing with the production of high temperature and with the various smelting operations, would be greatly enhanced in value if the various chemical equilibria involved and the influence of temperature on these equilibria, as well as examples of the now general plant thermal balance sheets, had been included.

The elucidation of certain operating details developed by the rule of thumb process in the light of scientific knowledge could in some cases have been attempted by reference to some other section of the volume. Thus, for example, the significance of certain temperatures in the distillation of oil, with and without the addition of steam (p. 644), could be grasped much more readily by reference to the chapter on distillation where the theoretical treatment of such a problem is dealt with *in extenso*. The practice of ore flotation (p. 337) should likewise contain a cross-reference to the theory outlined on p. 785.

For a book published in 1922 it is somewhat surprising to find that "no satisfactory explanation can be advanced at the present time for the anomalous position of argon in the periodic table" (p. 913), although the existence of isotopic lead is discussed on p. 860.

Apart from these minor criticisms, the book on the whole serves a useful purpose; the printing is good and the semi-limp binding is extremely attractive.

ERIC K. RIDEAL

Theories of Organic Chemistry. By Dr. Ferdinand Henrich. Translated and enlarged from the revised fourth German edition of 1921 by Treat B. Johnson and Dorothy A. Hahn. [Pp. xvi + 603.] (New York: John Wiley & Sons; London: Chapman & Hall, 1922. Price 30s. net.)

Much has been said about national propaganda in the guise of scientific textbooks, but it is seldom one comes across so interesting an example as Prof. Henrich's book, which should bear the title of "German Theories of Organic Chemistry." The original German book, of which the above is a translation, is at times almost amusing in its efforts to ignore the work of chemists outside Germany, and although it would be invidious perhaps to mention names, a glance through the author index will indicate to anyone acquainted with organic chemical research how unbalanced the book is. An interesting minor example of the author's attitude of mind may be given. Talking of rules for substitution in the benzene ring Henrich says: "Später haben Brown und Gibson, sowie Armstrong und Vorländer Gesetzmässigkeiten formuliert. Nach letzteren soll m-Substitution eintreten, wenn das Element des vorhandenen Substituenten ungesättigt, o, p-Substitution, wenn es gesättigt ist." The rule of Crum Brown and Gibson is at least as serviceable as that of Vorländer and at least has the claim of priority. This is perhaps trivial, but it is typical, and is quoted in order to draw attention also to a serious mis-translation in the American edition, "vorhandenen Substituenten" being rendered as "the entering group" instead of "the group already present," with fatal results to the meaning of the passage.

The translators have been sensible to the limitations of the book and have improved it to some extent by the addition of several new chapters incorporating the work of American chemists. Perhaps after it has been translated into several other languages a more balanced outlook will be achieved. Prof. Henrich, in the Preface to this edition, excuses himself by deploring the scarcity of foreign periodicals in Germany at the present time, but this hardly applies to work published before 1914. Beyond these limitations the book is a very useful one and makes stimulating reading for those interested in organic chemical research. Among the topics dealt with are, theories of valency, the constitution of benzene, tautomerism, application of physico-chemical principles to organic chemistry, the speculations of Nef, free organic radicals, molecular rearrangements, and the basic properties of oxygen. The subjects are discussed clearly, and the evidence for and against the various theories is well presented. The student will perhaps be a little discouraged thereby, as with each theory the pros and cons are so delicately balanced that it seems impossible to draw any definite conclusion as to the relative correctness of the views put forward. For example, in the case of the Beckmann Change the impression is conveyed that Beckmann's original suggestion that an exchange of radicals takes place is at least as satisfactory as any explanation of the mechanism which has as yet been put forward. This is cold comfort, as an interchange of radicals is rather a re-statement of the experimental facts than an explanation of them. However, a departure from the didactic attitude towards chemical theories is more calculated to advance the science than otherwise.

The book is well printed, the formulæ clearly set out, and the literary style pleasant and easy to read. With due allowance for its one defect, it is certainly a book to be read by every serious student of organic chemistry.

O. L. B.

GEOLOGY

Evolution of the Essex Rivers and of the Lower Thames. By Prof. J. W. Gregory, D.Sc., F.R.S. [Pp. 68, with 10 illustrations.] (Colchester: Benham & Co. Price 2s. 6d. net.)

In this work Prof. Gregory continues the theme of one of his earliest geological investigations and presents his mature reflections on a subject which, as he

tells us in the Introduction, first led him to the study of geology. The interpretation of Essex rivers is peculiarly difficult owing to the simple geological structure and low levels of the county; the only stones available to show the directions of the initial streams have been brought in from outside. The chief novelty in Prof. Gregory's treatment is the early dates assigned to the older gravels. Thus the Brentwood gravels are regarded as probably Upper Eocene, and the Danbury gravels as Miocene. The Cainozoic history of the Essex rivers is rather complicated. They began as north-western and south-eastern tributaries of a primal Thames, which flowed north-east of its present course along the axis of the Thames syncline. The north-western tributaries brought in the quartzite drift from the Midlands; and at a later date the south-eastern and southern tributaries brought in the Lower Greensand cherts from Kent. Further changes were brought about by the Pliocene subsidence of the Wealden area, which diverted the Thames to the south into its present course, and caused the Chelmer to extend south-east into the Crouch. By a still later change the Chelmer was forced to discharge into the Blackwater, and the Crouch was left as a beheaded estuary or föhrde. This exposition of a difficult subject is remarkably easeful, and is much helped by ten clear maps and diagrams.

G. W. T.

Essentials for the Microscopic Determination of the Rock-forming Minerals and Rocks. By A. Johannsen. [Pp. 53, with tables, diagrams, etc.] (University of Chicago Press, 1922. Price $2.)

This is a simplified laboratory manual containing practically all the data given in the writer's earlier work, *Determination of Rock-forming Minerals*. The minerals are tabulated first as opaque or transparent; the transparent minerals are then divided into two groups, isotropic or anisotropic; finally the anisotropic minerals are tabulated as colourless or coloured, pleochroic or non-pleochroic, uniaxial or biaxial. This affords a simple and easy method of running down the rock-forming minerals as seen in thin section. The discrimination is aided by a concise statement of the characters of each mineral in its appropriate place, and by the provision of excellent tabular presentations showing the position of each mineral in regard to refractive index and double refraction. There are supplementary sections on the determination of the felspars, and of the pyroxenes and amphiboles; the modes of occurrence of various mineral groups; a summary of petrographic methods; and an outline of Johannsen's quantitative mineralogical classification of igneous rocks. We have found from actual use in the laboratory that the manual provides an excellent method for the quick determination of the commoner rock-forming minerals.

G. W. T.

MISCELLANEOUS

The Soybean. By Charles V. Piper, M.S., D.S., and William J. Morse, B.S.A. [Pp. xv + 329, with 83 figures.] (London: McGraw-Hill Publishing Company, 1923. Price 20s. net.)

In this volume the authors give an account of the soybean from both agricultural and commercial standpoints. The information given includes that obtained from experiments made at Agricultural Experimental Stations throughout the United States. Practical advice is given regarding suitable climate, soil, manure, and treatment of the bean. The authors consider that the importance of the soybean lies largely in the fact that the seeds can be produced more cheaply than those of any other leguminous crop. A pleasing feature of the book is the large number of photographs of agricultural operations showing special adaptations of farm implements desirable for the successful culture and harvesting of the soybean.

As regards composition of the seed, the soybean contains a large percentage of digestible protein and little or no carbohydrate. The high oil content makes the bean one of the most important sources of vegetable oil. The residual cake or meal is a valuable food for stock. One section of the book deals with the preparation of soybean dishes for human consumption. For many centuries the soybean has been a staple article of diet in China and Japan; its recent introduction into the United States and Europe has, however, not yet resulted in its extensive use for food in these countries.

The book contains much useful and interesting information upon an important subject, and should stimulate efforts to utilise far more fully than hitherto the unique properties of the soybean.

DOROTHY M. ADKINS.

Colour and Methods of Colour Reproduction. By L. C. MARTIN, D.Sc., Lecturer in the Optical Engineering Department, Imperial College of Science and Technology, South Kensington. With Chapters on Colour Printing and Colour Photography by WM. GAMBLE, F.R.P.S., Editor of the *Process Year Book.* [Pp. xiv + 187, with 73 figures.] (London: Blackie & Son, 1923. Price 12*s.* 6*d.*)

THIS book, which is attractively produced with three colour plates, is of somewhat the same scope and appeals to the same readers as *Colour and its Applications*, by M. Luckiesh, published eight years ago. It opens with a description of the spectrum, hue and wave-length, passes on to colour mixing, primary colours, mixing of pigments, colour notation, where there is an account of Munsell's work, then deals with the energy distribution in the spectra of light sources and colouring materials. The second part treats of the eye and its reactions to light, photometry, instruments for colour measurement such as the colorimeter, tintometer, etc., colour vision and colour blindness. The last two chapters are on colour printing and colour photography. There is an appendix with a few tables, and a very good index.

The book, consequently, is for the "partial physicist" rather than the amateur. He will find it up-to-date, fair, interesting, and as complete as its size will allow. It is also accurate. It is thus somewhat surprising that the author considers the König-Martens spectrophotometer economical of light (p. 26) and accurate (p. 38). The general opinion was that Kayser's condemnation of it (*Spectroscopie*, vol. iii) in his exhaustive account of spectrophotometry was thoroughly justified. The reviewer, who speaks from a considerable experience of both instruments, found it much inferior to the Hüfner type under both these heads, though it is undoubtedly a neat little instrument. The adjustments of the double image prism and biprism have to be watched very carefully or the wave-length scale is thrown out, and these adjustments require a trained physicist. This was also the experience of a leading German chemical laboratory, where considerable work was done on absorption spectra.

The above is, of course, a small point, were it not for the fact that, appealing to the practical man as it does, the book may influence some in the choice of an instrument.

Mr. Gamble states, in his last sentence, that it is difficult to conceive any radically new lines on which research in colour photography can proceed. On this point there is wide agreement. But the fascination of the subject is such that it will never lack workers.

R. A. H.

of Science. By GEORGE SAMPSON. [Pp. vi + 147.] (London, Methuen & Co. Price 2*s.* net.)

THIS book is a selection of what may be called literary scientific extracts. It is well compiled, and covers a wide range of matter. There are thirteen

extracts in all, commencing with an appropriate discourse to medical students by the Scottish scientific litterateur, Dr. John Brown. It is entitled "With brains, sir," and though delivered in Mid-Victorian times it contains much advice that is sorely needed at the present time.

The various extracts cover such widely different subjects as Earthworms, by Charles Darwin; the influence of the imagination on the passions, by David Hume; the moon, by Robert Ball; and others by Faraday, Huxley, Tyndall, and Wordsworth. Taken together, they form a very choice reader that should prove exceedingly valuable to teachers of science who wish as far as possible to widen the basis of their training and to lift their teaching out of the material plane.

W. C. B.

The Evolution of Knowledge. By George Shann. [Pp. viii + 100.] (London: Longmans, Green & Co., 1922. Price 4*s.* 6*d.* net.)

This racy little book may serve some purpose in giving the general reader an idea of certain views about the nature of knowledge and some of the methods by which it is built up. Mr. Shann's theory of knowledge is anything but new. Anyone acquainted with either the pragmatist or the "instrumental" view of truth or knowledge (which goes back to Lange, Nietzsche, and others, about the middle of the nineteenth century, if not earlier) or with "behaviourist" psychology, did not require to have his attention called to this aspect of knowledge now. Anyway, Mr. Shann's main point is this. The common view is that so-called knowledge, including scientific knowledge, must either conceive things as they actually are, or it is worthless. In reality, however, human knowledge is only part of our adaptation to our environment, and even the highest scientific knowledge has grown from the need of the organism to forecast the consequences of its actions. So long as our knowledge serves this purpose it does not matter how it is in fact related to the actual world. The view is illustrated by the following parable. Suppose a sheet of paper to be marked with dots belonging to a great number of superposed patterns whose original connecting lines have disappeared. A succession of these dots is found to lie approximately along a curve whose form is calculable, and when the curve is produced the succession of dots along it recurs in regular series. The calculated curve may not correspond with the original connecting lines, yet, if the patterns were recurrent, dots belonging to them might continue indefinitely to lie along that curve. Now if the dots be taken to represent experiences, the curve may represent an hypothesis of recurrence, the series of dots first noticed representing the original observations which suggested the hypothesis, while the series of dots lying along the produced curve represent the further observations verifying the hypothesis. The curve helps one to forecast the positions of the dots, even if it throws no light on the original patterns.

A. Wolf.

Modern Microscopy. A Handbook for Beginners and Students. By M. I. Cross and Martin J. Cole. Fifth Edition revised and rearranged by Herbert F. Angus. [Pp. x + 315, with 12 plates and 144 text-figures.] (London: Baillière, Tindall & Cox, 1922. Price 10*s.* 6*d.* net.)

This well-known book on the microscope and its uses has now reached its fifth edition, although it has been so thoroughly revised as to constitute, in fact, a new, and in many respects an improved, handbook. Its purpose is clearly to provide the amateur rather than the professional with a general guide both as to the use of the microscope and to some of its more important applications. The first portion on the use and construction of the instrument itself is of general interest, and is so clearly and practically dealt with

that it may be regarded as a useful addition to the literature of the subject. The portion dealing with Elementary Optical Theory and its bearing in practice is lucid and interesting, the diagrams are sufficiently well drawn, and only a few microscopes of particular design are figured. This is a feature to be commended, as some similar treatises depend largely on descriptions and illustrations from makers' catalogues. The apparatus described and figured is confined to English manufacturers, in fact the four recognised English firms are the only ones mentioned in the preface. Some of the applications of the microscope are described by well-known workers in particular branches. Whether this method of dealing with such diverse subjects is desirable, or in the best interests of the reader, is open to question. The result is that this part of the book really consists of a series of essays: most of them are interesting, but from the limitations imposed are not of necessity sufficiently thorough to constitute a guide for the serious worker. The book is well produced, and has a number of good photomicrographs illustrating particular branches of work.

J. E. B.

Collected Papers on Acoustics. By WALLACE CLEMENT SABINE, late Hollis Professor of Mathematics and Natural Philosophy in Harvard University. [Pp. ix + 279, with plates and illustrations.] (Cambridge: Harvard University Press. London: Humphrey Milford, 1922. Price 17*s*. net.)

THE Harvard University Press has done a signal public service in collecting and publishing the original papers on acoustics of the late Professor Wallace C. Sabine. These papers, scattered as they are through various American journals, have in the past been difficult of access, but they are now available in a single volume which it is a pleasure to handle. The type, the paper, the format and the illustrations are all excellent, and the price of seventeen shillings compares more than favourably with that of other volumes of similar size published to-day.

It is fitting that these papers should be so collected, for they form the most important contribution from any one pen to our knowledge of the subject of architectural acoustics, and subsequent work by other experimenters has confirmed the conclusions reached by Sabine.

The value of the collection is much enhanced by the severity of the criticism that Professor Sabine always applied to his own productions. This has resulted in the loss to the public of much of his work which he considered incomplete, but it gives the more confidence in accepting as sound whatever he considered worthy of publication.

The researches were undertaken in the first instance not from choice but at the request of the Corporation of the University of Harvard for information that would enable them to remedy acoustical defects in the lecture-room of the Fogg Art Museum, a building that had just been completed. Throughout his investigations Sabine was never content to rely merely on theoretical relations and his work was always carried out in consultation with architects, so that it is work not only of academic interest, but essentially practical.

The first set of experiments occupied five years, and dealt mainly with the problem of reverberation for a single note. A second series extended this investigation to notes of the whole musical scale and also dealt with the accuracy of musical taste. Later experiments devised a method of calculation in advance of construction in which actual sound waves were photographed as they traversed a small model of an auditorium. In this way objectionable effects could be predicted and avoided.

References occur in the text to papers to be published but which never seem to have been written. This is the more regrettable in that one of these papers, mentioned on p. 31, would have dealt with the effect of the location

of absorbent materials in respect to the initial distribution of the sound and to discrete echoes, an aspect of the question which is most important but on which Sabine's papers contain little or no information. That he had the information himself is not doubted.

It is most important at present to expound the results of past investigations in a way that will appeal to members of the architectural profession. This volume, as is inevitable to some extent with a collection of separate papers, does not contribute largely to that end. There is a good deal of repetition in it, and there are occasional contradictions which require but have not received editorial annotation; in one case numbers, erroneous in the original paper, have been altered without comment. It is to be hoped that these defects will be eliminated from any subsequent edition.

G. A. SUTHERLAND.

Life Contingencies. By E. F. SPURGEON, F.I.A. [Pp. xxvi + 477, with tables.] (London: C. & E. Layton, 1922. By the authority and on behalf of the Institute of Actuaries.)

PART II of the Textbook of the Institute of Actuaries, dealing with the Theory of Life Contingencies, was first issued in 1887. The inevitable changes in the course prescribed by the Institute since that time have brought the Council to the necessity for the rearrangement of the Textbook. The present volume has, therefore, been prepared to meet, with other textbooks, the needs of the present-day student of Actuarial Science. This book deals only with that part of the theory relating to Life Contingencies, and contains throughout illustrative examples.

E. C. RHODES.

The Rhythm of Speech. By WILLIAM THOMSON, B.A., D.Litt. [Pp. 552.] (Glasgow: Maclehose, Jackson & Co. Price £5 5s.)

WITH characteristic point and lucidity Dr. Thomson's preface states the scope and purpose of his work, and its larger aims. His subject—the Nature and Laws of speech-rhythm in verse and prose—is, if not altogether an undiscovered country, at least one which is full of almost unexplored territory whence the adventurous have returned with spoils that puzzle curiosity, and furnish the experts with subjects for acute controversy. In this case the expert is also the pioneer and explorer. A considerable part of the book is therefore devoted to the destructive criticism of theories and methods which Dr. Thomson finds erroneous in principle or in detail. The ordinary reader is likely to be surprised when he discovers from this work how extensive is the literature of the subject, how much time, patience, and ingenuity have been devoted to it, and how meagre and unsatisfactory the results have often been. But Dr. Thomson is not content with merely correcting the errors of other pioneers, ancient and modern, or with erasing from the map their false orientations and unverifiable discoveries; he furnishes a new chart and a new guide; also he brings back some genuine and highly important products from his twenty-five years of voyaging and residence in the Land of Rhythm.

His method throughout is thoroughly scientific. He takes the concrete facts of living speech, as they present themselves to the attentive listener; he then applies the ordinary processes of close observation, careful induction from examples, deliberate experiment, and verification by repetition of the experiment by others. His results, therefore, may rightly claim to be scientific, more especially as he has been able to formulate them as laws of universal validity within their range, and as he has provided a consistent, simple, and practically useful notation in which the subject-matter may be duly and permanently recorded. His work is, therefore, a valuable

contribution to modern Æsthetic, for his material is language, and in Croce's words, "Nothing exists outside Æsthetic, which gives knowledge of the nature of language."

It is no light and easy task which the author has set himself. Even the definitions demanded by the most elementary discussion of the subject have in the past been obscure, or vague, or dominated by preconceptions and irrelevant considerations. Whether he agree with him or not, the reader of Dr. Thomson's book cannot complain as regards this fundamental requirement of scientific writing. The terms used are strictly, adequately, and clearly defined, and, in so far as one can discover, they are used with absolute consistency. This is a great, and perhaps unusual, virtue in a work dealing with so subtle and variable a thing as the actual speech of living men. Without certainty as to what is to be taken as the meaning of such terms as *rhythm*, *accent*, *quantity*, *syllable*, *measure*, *time*, *stress*, and many more, it is plain and obvious that no analysis can give any real knowledge of rhythmical structure, and that we must always fall back upon these in every case of difficulty or doubt.

Dr. Thomson's definition of Rhythm is therefore of prime importance for his whole book. If he can secure general agreement with it, if it is accepted as adequate and exact, it will be difficult to resist the conclusions based upon it; for his reasoning is closely logical, and his examples carry conviction to every unprejudiced mind. He accordingly devotes his first chapter to a discussion of the "Nature of Rhythm in Sounds." He restricts his discussion naturally to sound-rhythm, although he is evidently aware that rhythm is one of the essential facts of nature and perhaps the most universal fact. The interesting distinction between organic rhythms and inorganic rhythms takes him into the province of Psychology, and supplies him with another limitation: his rhythms are human rhythms, organic rhythms, they are inevitably supplied by ourselves if we listen to a series of like sounds produced with a certain ascertainable rapidity. In themselves such a series may be called rhythmical, for each is a wave with a definite wave-length, but the important point is that we cannot hear such a series without superimposing upon it from our own mental activity another kind of rhythm, which emphasises the crest of the wave at certain regular intervals: and thus we have accent and quantity and measure, the essentials of organic rhythm. Now this has been matter of observation from the days of the makers of folk-lore and of proverbs: witness "As the bell clinks the fool thinks," and "Turn again Whittington, Lord Mayor of London"; but why it should be so is a question to which even Dr. Thomson's thoroughness gives no definite answer. It is possible that the answer might be found in the subconscious influence of the two great vital rhythms, that of the heart-beat and that of the respiration. We do not consciously, in health, hear either of these unless we attend to them, but their influence in rhythmising every other series of sounds consciously heard may nevertheless be quite fundamental. The second chapter goes into detail in regard to the definition of terms. It is both constructive and destructive, and if the definitions and theories attacked survive the assault, it will not be because their assailant lacks either weapons or courage. The list of authors dealt with in the appendix to this chapter, and the very pointed criticisms contained in it, justify this remark. It may suffice in this connection to say that the list includes not only Saintsbury and Quintilian, but several names almost as famous, and that in the criticism such phrases occur as these: "The thought is loose, obscure, and charged with error"; "The student will be able to join the great band of disciples rounded in by such tissues of jargon"; "As thought it is sheer muddle." But Dr. Thomson is not always tilting at the windmills of the prosodists. He is more frequently engaged in the peaceful work of giving students of literature, and

especially of verse, a series of well-founded principles by the use of which they can very greatly enhance their knowledge of the structure of poetry, and therefore their enjoyment of it. For he teaches the indispensable facts regarding poetical form, and form is of the very essence of poetry. He would probably agree with those who find that the distinction between poetry and prose "cannot be justified save in that of art and science. Poetry is the language of sentiment, prose of the intellect." "Prose," says Dr. Thomson, "remains the fit vehicle for the fact and the commonplace, and the intonations of the voice accord with this mental attitude, whereas poetry lends itself to the expression of the subjective and the emotions, including tones that of themselves suggest this withdrawal in spirit to a higher atmosphere." He might even, like Vico, identify language and poetry in their origins and early history.

Technically the core of the book, apart from Chapter III, which consists of instruction in the Training of the Rhythmical Faculty, is to be found in Chapters IV to VII, and of these the most important, original, and fundamental portion is Chapter VI, which formulates the twenty-five laws of speech-rhythm with elucidations. All that can be said here of this part of Dr. Thomson's work is that it may be judged (it probably will be) by two very different sets of people. For those who know nothing about speech-rhythms technically, but who have the time, patience, and necessary delicacy of ear to make the experiments and verify the laws by examples, the study will prove as fascinating as chess; for the experts these chapters may be expected to form for long a field for the never-ending intellectual tournaments in which all investigations are liable to be involved, except those whose results can be weighed and measured, and photographed.

The remaining five chapters are among the most interesting portions of the work. These chapters are frequently vivid and illuminating in a high degree, and no one can read them without feeling that Dr. Thomson has brought to his task the trained ability, the vast knowledge of literature and of languages, the keenly analytical qualities of intellect necessary for such a work, and that he has succeeded in making the dry bones of the skeleton of speech throb with the very pulses of life. They deal with Theories of Modern Verse; with the Theory of Greek and Latin Verse, and with verse-rhythms in these languages; and they give some elaborate rhythmical analyses on the larger scale, and a discussion of unsuspected Metres in English Poetry.

John H. Murray.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Descriptive Geometry. By Laurence E. Cutter, Associate Professor of Mechanical Engineering, Stanford University. New York: McGraw-Hill Book Company; London: 6 & 8 Bouverie Street, E.C.4, 1923. (Pp. vii + 244, with 130 figures.) Price 12*s.* 6*d.* net.

Géométrie Descriptive. Par Gaspard Monge. Augmentée d'une Théorie des Ombres et de la Perspective extraite des Papiers de l'Auteur par Barnabée Brisson. Paris: Gauthier-Villars, 55 Quai des Grands-Augustins, 1922. (Pp.: Vol. I, 144; Vol. II, 140.) Price 6 frs.

Principes et Premiers Développements de Géométrie générale synthétique moderne. Par Émile Bally. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1922. (Pp. viii + 218.) Price 20 frs.

History of the Theory of Numbers. Vol. III, Quadratic and Higher Forms. By Leonard Eugene Dickson, Professor of Mathematics in the University of Chicago. With a chapter on the Class Number. By G. H. Cresse. Washington: Carnegie Institution, 1923. (Pp. v + 313.)

The Mathematical Theory of Relativity. By A. Kopff, Professor of Astronomy at the University of Heidelberg. Translated by H. Levy, M.A., D.Sc., F.R.S.E. London: Methuen & Co., 36 Essex Street, W.C.2. (Pp. viii + 214, with 3 diagrams.) Price 8*s.* 6*d.* net.

La Théorie de la Relativité d'Einstein et ses Bases physiques. Exposé Élémentaire par Max Born. Traduit de l'allemand d'après la seconde édition par F. A. Finkelstein et J. G. Verdier. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1923. (Pp. xi + 339.) Price 25 frs.

Études Élémentaires de Météorologie pratique. Par Albert Baldit. Deuxième Édition. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1922. (Pp. 428, with 132 figures.) Price 24 frs.

The Air and its Ways. The Rede Lecture (1921) in the University of Cambridge, with other contributions, and meteorology for Schools and Colleges. By Sir Napier Shaw, Sc.D., F.R.S. Cambridge: at the University Press, 1923. (Pp. xix + 237, with 100 figures.) Price 30*s.* net.

The Nebular Hypothesis and Modern Cosmogony. Being the Halley Lecture delivered on May 23, 1922. By J. H. Jeans. Oxford: at the Clarendon Press, 1923. (Pp. 30.) Price 2*s.* 6*d.* net.

Les Théories de la Relativité dépassent les Données de l'Expérience par le Lieutenant-Colonel Corps. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1923. (Pp. 39.) Price 3 frs.

Cours de Mécanique céleste. Par M. H. Andoyer, Membre de l'Institut et du Bureau des Longitudes, Professeur à la Faculté des Sciences de Paris. Tome I. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1923. (Pp. vi + 439.) Price 50 frs.

Four Lectures on Relativity and Space. By Charles Proteus Steinmetz, A.M., Ph.D. London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4, 1923. (Pp. x + 126.) Price 10*s.* net.

The Marine Chronometer: Its History and Development. By Lieut.-Commander Rupert T. Gould, R.N. (retired), F.R.G.S., Member of the British Horological Institute. With a Foreword by Sir Frank W. Dyson, LL.D., F.R.S., Astronomer Royal. London: J. D. Potter, Admiralty Agent for Charts, 145 Minories, E.1, 1923. (Pp. xvi + 287, with 39 plates.) Price 25*s.* net.

Les Divers Aspects de la Théorie de la Relativité. Par J. Villey. Avec une Préface de M. Brillouin. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1923. (Pp. xi + 95.) Price 5.50 frs.

Modern Electrical Theory. Supplementary Chapters. Chapter XVI, Relativity. By Norman Robert Campbell, Sc.D., a Member of the Staff of the Research Laboratories of the General Electric Company, London. Cambridge: at the University Press, 1923. (Pp. viii + 116.) Price 7*s.* 6*d.* net.

Atoms. By Jean Perrin, Professeur de Chimie physique à la Sorbonne. Authorised translation by D. L. Hammick, M.A., Fellow and Tutor, Oriel College, Oxford. Second English Edition, revised. London: Constable & Co., 1923. (Pp. xv + 230.) Price 8*s.* 6*d.* net.

Surface Tension and Surface Energy, and their Influence on Chemical Phenomena. By R. S. Willows, M.A., D.Sc., and E. Hatschek. Third Edition. London: J. & A. Churchill, 7 Great Marlborough Street, 1923. (Pp. viii + 136, with 25 figures.) Price 6*s.* 6*d.*

Advanced Practical Physics for Students. By B. L. Worsnop, B.Sc., and H. T. Flint, M.Sc., Ph.D., Lecturer in Physics, King's College, London. London: Methuen & Co., 36 Essex Street, W.C.2. (Pp. vii + 640, with 394 figures.) Price 21*s.* net.

A Dictionary of Applied Physics. Edited by Sir Richard Glazebrook, K.C.B., D.Sc., F.R.S. In Five Volumes. Vol. IV, Light, Sound, Radiology. London: Macmillan & Co., St. Martin's Street, 1923. (Pp. viii + 914.) Price 63*s.* net.

Éléments de la Théorie Électromagnétique de la Lumière. Par Ludwik Silberstein, Traduit de l'Anglais par Georges Matisse. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1923. (Pp. iv + 94.) Price 6 frs.

A Method for the Identification of Pure Organic Compounds. By a Systematic Analytical Procedure based on Physical Properties and Chemical Reactions. Vol. IV, Containing Classified Descriptions of about 3,700 of the More Important Compounds belonging to Fourteen of the Higher Orders. By Samuel Parsons Mulliken, Ph.D. New York: John Wiley & Sons; London: Chapman & Hall. (Pp. vii + 238.) Price 30*s.* net.

Organic Syntheses. An Annual Publication of Satisfactory Methods for the Preparation of Organic Compounds. By G. H. Coleman and others. New York: John Wiley & Sons; London: Chapman & Hall. (Pp. vii + 100.) Price 7*s.* 6*d.* net.

Vat Colours: Synthetic Colouring Matters. By Joscelyn Field Thorpe, C.B.E., D.Sc., Ph.D., F.R.S., F.I.C., Professor of Organic Chemistry in the Imperial College of Science and Technology, and Christopher Kelk Ingold, D.Sc., Lecturer in Organic Chemistry in the Imperial College of Science and Technology. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1923. (Pp. xv + 490.) Price 16*s.* net.

Chemical Technology and Analysis of Oils, Fats, and Waxes. By Dr. J. Lewkowitsch, M.A., F.I.C. Sixth Edition, entirely revised by George H. Warburton. In Three Volumes; Vol. III. London: Macmillan & Co., St. Martin's Street, 1923. (Pp. viii + 508, with numerous illustrations.) Price 36*s.* net.

A Comprehensive Treatise on Inorganic and Theoretical Chemistry. By J. W. Mellor, D.Sc. Vol. III. London: Longmans, Green & Co., 39 Paternoster Row, 1923. (Pp. x + 927, with 158 diagrams.) Price 63*s.* net.

The Chemistry-Tangle Unravelled: Being Chemistry systematised on a New Plan based on the Works of Abegg, Kossel, and Langmuir. By Francis W. Gray, M.A., D.Sc., Senior Lecturer in Chemistry, Aberdeen University. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4. (Pp. x + 148.) Price 6*s.* net.

The Spectroscope, and its Uses in General Analytical Chemistry. By T. Thorne Baker, A.M.I.E.E., F.R.P.S. Second Edition. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, 1923. (Pp. x + 208, with 98 figures.) Price 8*s.* 6*d.* net.

The Phase Rule and its Applications. By Alexander Findlay, M.A., Ph.D., D.Sc., Professor of Chemistry, University of Aberdeen. Fifth Edition. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1923. (Pp. xvi + 298, with 158 figures.) Price 10*s.* 6*d.* net.

Modern Chemical Lecture Diagrams, with Uses and Applications Fully Described. By Dr. Geoffrey Martin, D.Sc., Ph.D., F.I.C., F.C.S. Assisted by J. M. Dickson, B.Sc., and Major J. W. Christelow, B.Sc., A.I.C. London: Sampson Low, Marston & Co. (Pp. iii + 88, with 36 figures.) Price 3*s.* 6*d.* net.

Thermodynamics and the Free Energy of Chemical Substances. By Gilbert Newton Lewis, Professor of Chemistry in the University of California, and Merle Randall, Associate Professor of Chemistry in the University of California. London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4, 1923. (Pp. xxiii + 653.) Price 25*s.* net.

The Chemistry of Urea: The Theory of its Constitution, and of the Origin and Mode of its Formation in Living Organisms. By Emil A. Werner, M.A., Sc.D., F.I.C., Professor of Applied Chemistry in the University of Dublin. London: Longmans, Green & Co., 39 Paternoster Row, 1923. (Pp. xii + 212.) Price 14*s.* net.

Practical Chemistry. By E. J. Holmyard, B.A., Head of the Science Department, Clifton College. London: G. Bell & Sons, 1923. (Pp. xvi + 267.) Price 4*s.* net.

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Plant Physiology. By Vladimir I. Palladin, Professor in the University of Petrograd. Authorised English Version based on the German Translation of the Sixth Russian Edition and on the Seventh Russian Edition (1914), edited by Burton Edward Livingston, Ph.D., Professor of Plant Physiology and Director of the Laboratory of Plant Physiology of the Johns Hopkins University. Second American Edition, with a Biographical Note and Chapter Summaries by the Editor. Philadelphia: P. Blakiston's Son & Co., 1012 Walnut Street. (Pp. xxxiii + 360, with 173 illustrations.) Price $4 net.

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Heredity and Child Culture. By Henry Dwight Chapin, M.D., President of the Children's Welfare Federation of New York, with a Foreword by Prof. Henry Fairfield Osborn. London: George Routledge & Sons, 68 Carter Lane, 1923. (Pp. viii + 192.) Price 6*s.* net.

The Vaso-motor System. By Sir William M. Bayliss, M.A., D.Sc., LL.D., F.R.S., Professor of General Physiology in University College, London. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1923. (Pp. iv + 163, with 67 figures.) Price 7*s.* 6*d.* net.

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The Debt of Medicine to the Fine Arts. The Presidential Address, delivered on October 11, 1922, at the opening of the Fiftieth Session of the Bristol Medico-chirurgical Society. By J. A. Nixon, C.M.G., M.D., F.R.C.P., Physician to the Bristol Royal Infirmary, Consulting Physician to Southmead Infirmary. From *The Bristol Medico-chirurgical Journal*, January 1923. Bristol: J. W. Arrowsmith, 1 Quay Street. (Pp. 29, with 11 plates.) Price 2*s.* 6*d.* net.

Considérations sur l'Être vivant. Troisième Partie, La Characée considérée au Point de Vue orthobiontique. Beauvais: Dumontier et Hagué, 23 rue Jules-Michelet, 1922. (Pp. 54.)

Interfacial Forces and Phenomena in Physiology: Being the Herter Lectures in New York in March 1922. By Sir William M. Bayliss, M.A., D.Sc., F.R.S., LL.D., Professor of General Physiology in University College, London. London: Methuen & Co., 36 Essex Street, W.C. (Pp. ix + 196, with 7 figures.) Price 7*s.* 6*d.* net.

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Practical Mechanics and Strength of Materials. By Charles Wilbur Leigh, B.S. London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4, 1923. (Pp. viii + 293.) Price 11*s.* 3*d.* net.

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American Machinist Gear Book. By Charles H. Logue. Thoroughly revised by Reginald Trautschold, M.E. Third Edition. London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4, 1922. (Pp. ix + 353, with 273 figures.) Price 15*s.* net.

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The Year Book of Wireless Telegraphy and Telephony, 1923: Amateur Edition. London: The Wireless Press, Ltd., 12 Henrietta Street, W.C.2.

Your Broadcast Receiver and How to Work it. By Percy W. Harris. London: The Wireless Press, Ltd., 12 Henrietta Street, W.C.2. Price 6*d.* net.

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SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

PURE MATHEMATICS. By F. PURYER WHITE, M.A., St. John's College, Cambridge.

History.—G. A. Miller (*School Science and Mathematics*, **23**, 1923, 138–49) prints a third list of marginal notes on Cajori's *History of Mathematics*.

In his Presidential Address to the Mathematical Association Sir Thomas Heath (*Math. Gazette*, **11**, 1923, 248–59) gave an account of the evolution of the infinitesimal calculus in Greek geometry, pointing out that in the works of Archimedes there are six cases where the investigation gives the equivalent to an *integration*.

Algebra, Groups, and Theory of Numbers.—Sir Thomas Muir (*Proc. Roy. Soc. Edin.*, **43**, 1923, 127–48) gives an annotated list of nearly fifty papers dealing with the theory of Alternants in the period 1896–1917.

P. A. MacMahon (*Proc. Camb. Phil. Soc.*, **21**, 1923, 376–90) develops the algebra of symmetric functions; particular applications have been made by the same author (*Quarterly Journal*, **49**, 1920, 1–35) to American Tournaments and to Chess Tournaments (*ibid.*, 353–84).

In a Presidential Address delivered before the American Mathematical Association G. A. Miller (*Amer. Math. Monthly*, **29**, 1922, 319–28) discusses "Contradictions in the Literature of Group Theory," calling attention to the fact that in group theory we employ two extreme definitions of what is meant by the same group. According to one of these a characteristic operator must correspond to itself in every possible automorphism of the group, but according to the other this is not the case. In the case of non-regular substitution groups of finite order there are three definitions of what is meant by the same group. The third, which is the oldest and goes back to Cauchy (1845), states that it is necessary and sufficient that one can be transformed into the other by means of some substitution. The first definition adds to this the condition that

the two substitution groups in question shall be regular, and therefore implies that a simple isomorphism can be established between their substitutions or operators. The second definition implies that two groups are the same if, and only if, they involve the same substitutions or operators irrespective of their arrangement within the group.

Details of the proof sketched by L. J. Mordell (SCIENCE PROGRESS, **18**, 1923, 54) of the theorem that the equation $ey^2 = ax^3 + bx^2 + cx + d$ has only a finite number of integer solutions, if the right-hand side has no squared factors in x, have now been published (*Proc. L.M.S.*, **21**, 1923, 415–19).

H. W. Richmond (*Proc. L.M.S.*, **21**, 1923, 401–9) gives an account of the analogue of Waring's problem for rational numbers, the possibility of the expression of any rational number as the sum of squares or cubes of rational numbers.

In a fifth memoir under the general heading "Some Problems of Diophantine Approximation," G. H. Hardy and J. E. Littlewood (*Camb. Phil. Soc. Trans.*, **22**, 1923, 519–34) investigate the analytic properties of certain Dirichlet's series which are intimately bound up with the problem of the distribution of the numbers $n\theta$ to modulus 1.

L. J. Mordell (*Proc. L.M.S.*, **21**, 1923, 493–6) has a note on trigonometric series involving algebraic numbers, related to a paper by C. L. Siegel (*Gött. Nach.*, 1922, 17–24), in which the latter proves Minkowski's theorem that the discriminant of an algebraic field is greater than unity for the particular case of a totally real field.

Hypercomplex numbers of the form $a + be$, where a, b are integers and $e^2 = 0$, possess as a rule several factorisations into undecomposable numbers. L. E. Dickson (*Bull. Amer. Math. Soc.*, **28**, 1922, 438–42) shows that unique factorisation cannot be restored by defining hypercomplex ideals analogous to algebraic ideals, nor by the introduction of any sort of ideals obeying the laws of arithmetic. Similar results had been stated previously by Du Pasquier (*Comptes Rendus Strasbourg*, 1921), but without proof.

Analysis.—If D denote the differential operator d/dx, two operators $f(D)$, $g(D)$ with constant coefficients are commutative, but two operators $\phi(D)$, $\psi(D)$ with coefficients which are functions of x are not, as a rule, commutative. More generally, if P be any operator, the operators $f(P)$, $g(P)$ are commutative. But it is not difficult to see that these are not the most general types of commutative operators. J. L. Burchnall and T. W. Chaundy (*Proc. L.M.S.*, **21**, 1923, 420–40) propose the problem of determining all commutative operators which are not reducible to a form $f(R)$, $g(R)$, where R is some linear operator $\lambda D + \mu$. They do not solve the general problem,

but they establish certain characteristic properties of all commutative operators, and obtain a complete solution, in terms of hyperelliptic functions, of the case in which one operator is quadratic.

In 1906 E. Landau showed that the points of convergence or divergence of the factorial series

$$F = \frac{1}{\Gamma(x)} \sum_{1}^{\infty} \frac{a_\nu (\nu - 1)!}{x(x + 1) \ . \ . \ . \ (x + \nu - 1)}$$

are the same as for the corresponding Dirichlet series $D = \sum_{1}^{\infty} \frac{a_\nu}{\nu^x}$, with the same coefficients $a\nu$, and, further, that the analytic functions thereby represented are closely related. F. Nevanlinna (*Ann. Ac. Sci. Fennicæ*, A18, 1922, No. 3) extends these results to the more general series

$$D = \sum_{1}^{\infty} \frac{a_\nu}{\gamma_\nu^{\,x}}, \quad F = \sum_{1}^{\infty} a_\nu \frac{\Gamma(\gamma\nu)}{\Gamma(x + \gamma\nu)},$$

which reduce to the above when $\gamma_\nu = \nu$.

R. Nevanlinna (*Math. Zs.*, 13, 1922, 1–9) examines the properties of bounded functions with prescribed values at n given points of the boundary; he shows that such a function exists, and only exists, if the given values satisfy n different inequalities. If the points are made to coincide he obtains necessary and sufficient conditions for the first $2n$ coefficients of the Taylor development of a bounded function at a boundary point. In a related paper (*Ann. Ac. Sci. Fennicæ*, A18, 1922, No. 5) he examines what are the necessary and sufficient conditions for a sequence of real numbers $c_1, c_2 \ . \ . \ . \ c_n \ . \ . \ .$ so that a function $f(x)$ of the complex variable $x = s + it$ may exist which (1) is regular for $t > 0$ and has an imaginary part which is not positive, and (2) is asymptotically represented in the neighbourhood of the point at infinity within the angle defined by $\epsilon <$ arg $x < \pi - \epsilon$ for any positive ϵ, however small, by $\frac{c_1}{x} + \frac{c_2}{x^2} + \ . \ . \ . \ \frac{c_n}{x^n} + \ . \ . \ . \ .$ Moreover, when do they define a single function of this kind, and, if more than one function, what is the general expression? He also develops the connection between this problem and the Stieltjes moment problem, which, as stated by Hamburger, is as follows: What are the necessary and sufficient conditions for the real numbers c_n, for a real increasing function $\psi(u)$ to exist such that c_n is equal to the moment $\int_{-\infty}^{\infty} u^{n-1} d\psi(u)$, $(n = 1, 2 \ . \ . \ .)$, the integrals being taken in the Stieltjes sense? Nevanlinna

shows, in fact, that each function of the class considered may be represented by a Stieltjes integral $\int_{-\infty}^{\infty} d\psi(u)/(x-u)$, where $\psi(u)$ is a solution of the moment problem.

W. J. Mellin (*Ann. Ac. Sci. Fennicæ*, A18, 1922, No. 4) argues that this theory of asymptotic series is contained in the more general theory of reciprocal functions and integrals.

Just as from Fourier's integral formula we obtain the reciprocal relations

$$f(x) = (2/\pi)^{\frac{1}{2}} \int_0^\infty \cos xuF(u)du, \quad F(x) = (2/\pi)^{\frac{1}{2}} \int_0^\infty \cos xuf(u)du,$$

connecting two functions $f(x)$ and $F(x)$, so from Hankel's integral formula $f(x) = \int_0^\infty J_\nu(ux)udu \int_0^\infty J_\nu(ut)tf(t)dt$, we have, writing $x^{-\frac{1}{2}}f(x)$ for $f(x)$, $f(x) = \int_0^\infty (ux)^{\frac{1}{2}} J_\nu(ux)F(u)du$ and $F(x) = \int_0^\infty (ux)^{\frac{1}{2}} J_\nu(ux)f(u)du$. Two functions connected by relations of this kind are said to be Hankel transforms each of the other; the theory of such is developed by E. C. Titchmarsh (*Proc. Camb. Phil. Soc.*, 21, 1923, 463–73).

G. H. Hardy (*Proc. Camb. Phil. Soc.*, 21, 1923, 492–503) gives a summary of a chapter of a manuscript notebook of Ramanujan, dealing with the summation and transformation of the hypergeometric series.

An analytic function of two complex variables z_1, z_2 is defined by Osgood as a function satisfying the following conditions:

(A) For every value of z_1 it is an analytic function of z_2, and for every value of z_2 it is an analytic function of z_1.

(B) It is bounded.

The problem then arises whether condition (B) is not implicitly contained in (A); this does not seem to have been solved. W. H. Young (*Proc. L.M.S.*, 21, 1923, 441–55) shows that it is sufficient to replace condition (B) by a wider one (B'), prescribing the existence of the double integral of the modulus of the function taken round the boundary of a domain D' inside the domain D for which (A) holds.

S. Pollard (*Proc. L.M.S.*, 21, 1923, 456–82) gives a detailed discussion of the conditions for Cauchy's theorem, classifying them under the heads (*a*) Conditions affecting the region itself; (*b*) Conditions ensuring a proper connection between the region and its boundary; and (*c*) A special condition to be satisfied by the boundary.

T. Carleman (*Proc. L.M.S.*, 21, 1923, 483–92) proves a theorem concerning the average value of the kth power of $|s_\nu - f(x)|$, where $f(x)$ is a summable function and s_ν is the sum of the first $\nu + 1$ terms of its Fourier expansion.

One of the important problems in the theory of linear differential equations of the second order with coefficients rational functions of the independent variable x is the determination of those equations which have an algebraic solution, with possibly a factor $(x - a)^\lambda$, where λ is irrational. Such solutions are the Lamé polynomials—so called by Klein, who investigated them in his lectures on Differential Equations (see also a summary reprinted in his *Collected Works*, t. 2, p. 587). The traditional mode of stating the problem is not to investigate a prescribed differential equation with regard to its integrability, but to determine the arbitrary parameter of a differential equation with given singular points and exponents (roots of the indicial equation), so that the definite kind of integrability may arise. The differential equation is taken in a "normal" form, in which for each singular point one of the exponents is zero. Also one singular point may be transferred to infinity and the $n - 1$ finite singular points may be taken as real. They then define $n - 2$ intervals of the real axis, and among these we can distribute any k real points in $(k + n - 3)!/(n - 3)!k!$ ways. Then this is, in general, precisely the number of Lamé polynomials of degree k of the given equation. All the Lamé polynomials of degree k are, in fact, real, and equated to zero have only real roots lying in the $n - 2$ intervals, and further are individually characterised by the distribution of the roots among the $n - 2$ intervals. This was first proved by Klein in 1881, for the case, arising in mathematical physics, in which the second exponent at all the finite singular points is $\pm \frac{1}{2}$ (*l.c.*, p. 512). In 1884 Stieltjes showed (*Acta Math.*, t. 6, p. 321) that the theorem is true *as long as the second exponent of the finite singular points is less than unity.* Stieltjes' limit is a real one, since if one of the exponents is greater than 1 some of the Lamé polynomials may become imaginary or have imaginary roots or agree with one another in the distribution of their real roots among the $n - 2$ intervals. In the recently published (1922) second volume of his *Collected Works* (p. 597) Klein has added a discussion of the simple case of the hypergeometric equation when the exponents exceed Stieltjes' limit. O. Haupt and E. Hilb (*Math. Ann.*, 89, 1923, 130–46) investigate in a similar way the case of a more general differential equation.

Geometry.—D. M. Y. Sommerville (*Proc. Roy. Soc. Edin.*, 43, 1923, 85–116) investigates the various ways in which it is possible to divide the plane into congruent triangles, and

space of three dimensions into congruent tetrahedra. In obtaining his networks he makes no distinction between elliptic, Euclidean, and hyperbolic space, and it is difficult to compare his results with those of P. A. MacMahon.

J. P. Gabbatt (*Proc. Camb. Phil. Soc.*, **21**, 1923, 297–362) shows that some of the properties of circles associated with the triangle in a Euclidean plane, notably those relating to the pedal line and circle, Feuerbach's theorem, and the Wallace-Steiner properties of the quadrilateral, are special cases of polar properties of cubic loci and envelopes in a projective plane.

An algebraic curve of order n is met by any straight line in n points, which may be real or imaginary; this, of course, is a commonplace, but the question how many or how few may be real is more complicated. In 1902 Miss C. A. Scott introduced the notion of the *index* of a plane algebraic curve or of a circuit of such a curve (German *Zug*; see, for instance, Hilton, *Plane Algebraic Curves*, p. 366, for Harnack's theorem that a curve of genus p can have at most $p + 1$ circuits). The *index* is the smallest number of real points in which the curve or the circuit can be met by any straight line. Miss Scott showed that there exist curves of every order n and of genus 0 or 1 with a circuit of index $n - 2$, and for genus 1 there may be also a simple oval (of index 0). She also generalised this theorem as follows: For any order n curves of genus p exist which have a circuit of index $n - 2q$, where q is the greatest integer in $\frac{1}{4}(p + 6)$. The method of proof depends on Cremona transformations of the plane; the curves so found are the trigonal curves, having a linear series of grade 3 and freedom 1. This work was followed up by P. Field, who, using the method of small variations of the constants of the curve, showed that for any order n algebraic curves without singular points exist which consist of one circuit of index $n - 4$. He also asserted, but only proved in special cases, that for every order n curves of genus $p (1 \leq p \leq n - 2)$ exist which consist of circuits the sum of whose indices is $n - 2$, with possibly also a simple oval. J. v. S. Nagy (*Math. Ann.*, **77**, 1916, 416) extended the theory to space curves and proved Field's theorem in a more precise form: For any order n there exist plane curves of genus $p (0 \leq p \leq n - 2)$ consisting of $p + 1$ circuits of indices $i_1, i_2 \ldots i_{p+1}$ where the i's are any positive integers whose sum is $n - 2$, *one* of which may be zero. He also introduced the term *order of reality* (or, shortly, *order*) for the greatest number of real points of a circuit lying upon a straight line, and showed that the order of a circuit of a plane or space curve of index-sum $n - 2$ exceeds the index by 2. Subsequent papers by H. Mohrmann (*Gött. Nach.*, 1916, 197;

Sitzungsber. Münch., 1916, 291; Math. Ann., 78, 1917, 171) deal also with *non-algebraic* curves of maximum index. In a recent paper (*Math. Ann.*, 89, 1923, 32–75) Nagy returns to the subject, introducing the dual idea of *class-index*. He finds that, by means of a simple construction from a finite number of elementary arcs lying wholly in a finite region, curves of maximum class-index can be obtained with the most different general properties. He is thus able to show that there are curves of maximum index of which the index is greater than the sum of the indices of its circuits, which contradicts one of Mohrmann's theorems.

A. B. Coble (*Bull. Amer. Math. Soc.*, 28, 1922, 329–64) gives an excellent account, with full references, of the theory of Cremona transformations and their applications to algebra, geometry, and modular functions. He points out that while the projective group, with projective geometry and the theory of algebraic forms, and the birational group, with algebraic geometry and algebraic functions, have been highly developed, yet for the group of Cremona transformations, lying between these two domains, no distinctive geometry and no distinctive invariant theory have as yet been formulated. His aim is therefore to give a brief résumé of achievement in this field along the somewhat scattered lines in which research has been pursued and to indicate problems that await solution.

A paper of a somewhat similar kind has been published still more recently by G. A. Bliss (*ibid.*, 29, 1923, 161–83) on the transformation of the singularities of a plane algebraic curve. There are two principal theorems. The first, due to Noether (1871), is that any plane algebraic curve can be transformed into another with only ordinary singularities (*i.e.* multiple points with distinct tangents) by means of a sequence of quadratic transformations, *i.e.* by a Cremona transformation. The second, of which the origin seems uncertain, is that any plane algebraic curve can be transformed birationally into a plane curve whose only singularities are ordinary double points. Bliss discusses the various proofs, geometrical, arithmetical, and from function theory, of these two theorems, and advances several criticisms, especially of the geometrical proofs of the second theorem, which employ projection from higher space without, in his opinion, sufficient examination of the process.

Plücker's equations connecting the point and line singularities of a plane algebraic curve do not contain in themselves any existence theorems, and there is no guarantee that a curve exists with characteristics corresponding to any solution in positive integers of the equations. In 1913 Lefschetz showed that there exist plane curves of any order and any number of ordinary double points up to the maximum, and that the

requirement of each additional double point imposed exactly one new condition. He also obtained upper limits for the number of cusps, but his results depend upon what he calls the *postulate of singularities*, *i.e.* he assumes that if we require a curve to have an additional cusp we do not automatically impose upon it another cusp. J. L. Coolidge (*Bull. Amer. Math. Soc.*, **28**, 1922, 451–5) re-examines this matter of cusps, following Lefschetz's methods, and arrives at a more definite conclusion, incidentally proving his postulate. His result is that the maximum number of cusps for a curve of odd order n and genus p, where $n \geq 2p + \sqrt{8p+9}$, is $\frac{3}{2}[(n-2)+2p] - \frac{1}{2}$; while for a curve of even order n, where $n \geq 2p + 1 + \sqrt{8p+1}$, the maximum number is $\frac{3}{2}[(n-2)+2p]$. There exist curves of order n and genus p having cusps in any number up to this maximum.

C. G. F. James (*Proc. Camb. Phil. Soc.*, **21**, 1923, 435–62) obtains formulæ for the genus of the curve of intersection of two surfaces residual to a system of multiple curves with mutual intersections, and for the number of points of intersection of three surfaces residual to such a system. Going on to space of four dimensions, he finds a formula for the genus of a curve which is the intersection of three varieties residual to a group of curves and surfaces in arbitrary position.

There is, in general, no collineation which transforms a double-six of lines into itself; but there are special types of double-six which are invariant for some collineations. One such type was investigated by Burnside in 1912, who showed that associated with any five points in space there is a double-six which is invariant for every collineation which transforms the five-point into itself. E. Stenfors (*Annales Ac. Sci. Fennicæ*, **A18**, 1922, No. 1) gives a complete investigation of all such special double-sixes. To this end he develops a new construction of the double-six, and gives new proofs of Schäfli's theorem that if five lines have a common transversal the five other lines which meet four of them have also a common transversal, and of Schur's theorem that conjugate lines of a double-six are reciprocal polars with regard to a quadric. In a subsequent paper (*ibid.*, No. 7) he investigates the similar problem for the complete configuration of 27 lines, without making use of the cubic surface on which they lie. The results are in part the same as those obtained by K. Bobek in 1899 on cubic surfaces invariant under collineations, but not entirely, as the cubic surface may be real without all the 27 lines being real. It is this latter case that Stenfors considers.

An irreducible quartic surface with a twisted nodal curve must necessarily be ruled and rational. For the twisted curve must be a cubic and the chord through any point of the surface not lying on the curve will meet the surface in five points and will therefore lie wholly upon it. The surface is, therefore, ruled, and it is rational because the section by any plane is a quartic curve with three double points. Since a rational ruled quartic surface is the line-geometry image of a rational quartic curve, and since such a curve must certainly lie in a four-dimensional space, it follows that any rational ruled quartic must belong to a linear complex, which may be general or special. Surfaces of the type considered have been the subject of several investigations, and brief summaries in English are to be found in Salmon (*Solid*, 1915 edition, t. 2, p. 207) and Jessop (*Line Complex*, p. 78). Cayley (1863–9) and Cremona (1868) distinguished two species, R_1, whose generators belong to a general linear complex, and R_2, whose generators belong to a special linear complex. Clebsch (1870) defined such surfaces as the locus of the joins of corresponding points of two projectively related conics, and proved them to be identical with the locus of those chords of a twisted cubic which belong to a linear complex, but he was of opinion that R_2 is only an unessentially different special case of R_1. As a matter of fact, R_2 contains no irreducible conics, and thus cannot be obtained by Clebsch's method; it is the locus of the joins of corresponding points of a straight line and a projectively related twisted cubic. A new investigation, bringing out the essential points of difference between R_1 and R_2, has recently been published by H. Mohrmann (*Math. Ann.*, 89, 1923, 1–31). He deals with the surfaces as projections of normal quartic surfaces in space of five dimensions, but of normal surfaces of distinct types. Another method of investigation, employed by Rohn (1887), is by means of the symmetrical [2, 2] correspondence of points on the nodal curve arising from the generators of the surface. Through any point P of the twisted cubic pass two generators, meeting the curve again in Q and R. Rohn did not, however, consider the special case in which the three points P, Q, R form an involution; it is this case which gives rise to the surfaces R_2. This was pointed out by Sturm (*Liniengeometrie*, t. 1, p. 56), who also examined the case in which the [2, 2] correspondence split up into two non-involutory [1, 1] correspondences. His methods being purely synthetic, he did not remark what Mohrmann shows analytically, that these two cases are not mutually exclusive, and that there is a variety of quartic surface generated by the joins of corresponding points of a twisted cubic whose generators belong to a special linear complex. It can be obtained most

simply as the locus of those chords of the cubic which meet the line of intersection of any two different osculating planes. If these planes are real, all the non-singular real generators are ideal chords of the cubic; if they are conjugate imaginary, the chords meet the curve in two real points. The projectivity on the cubic is characterised by the fact that the cross-ratio of a pair of corresponding points and the two fixed points is equal to an imaginary cube root of unity.

APPLIED MATHEMATICS. By S. BRODETSKY, M.A., Ph.D., F.Inst.P., etc., University, Leeds.

THE results of the eclipse expeditions of last September are now available, and they confirm the bending of rays of light that pass near the sun to an extent almost exactly as predicted by Einstein's theory. This is important as establishing decisively the existence of the Einstein light-bending, and Prof. Campbell considers that further expeditions for this purpose are now unnecessary. In a sense, therefore, we have reached a stage in the development of relativity theory where we can stop and take stock of the exact state of affairs. There can be no doubt now that in two important respects, namely as regards the perihelion of Mercury, and as regards the effect of gravitating fields on light rays, Einstein's theory has received abundant confirmation. The third test, in connection with the shift in the lines of the solar spectrum, has not yet led to a decisive result, the evidence for and against being at present something like fifty-fifty.

A cautious student of mechanics must, it seems, adopt a view somewhat like the following. It has been established that the gravitational field of the sun does not follow Newton's law of gravitation with absolute exactness, and there is every reason to believe that Einstein's law of gravitation, as given in his famous equations $G_{\mu\nu} = 0$, is a nearer approach to the truth. Further, it has been proved that light rays are affected by the gravitational field of the sun in such a way as to suggest that the path of a ray of light is that of a particle with certain "initial conditions" at infinity. It is therefore necessary so to modify our views of mechanics and optics as to make them include these new discoveries. The applied mathematician is not primarily interested in the exact properties of space—he wants to explain the phenomena of dynamics, and to work them up into a system based on as few fundamental principles as possible. This should therefore be the next task before us, namely, to introduce into our conceptions of dynamics such changes as will take account logically of the new discoveries.

This should not be very difficult, and no doubt before very long all advanced students of mechanics will be able to appreciate the fundamental changes involved. That some thinkers do find it difficult to assimilate Einstein's interpretation is obvious from the somewhat heated discussion that is still going on about the subject. J. Le Roux (*Comptes Rendus*, **175**, 1922, 809–11) returns to the attack. His point is that Einstein's theory makes the field at any point depend upon four variables, three of space and one of time. The field cannot, therefore, depend on all the bodies present in the universe, and thus there cannot be such things as planetary perturbations. Le Roux puts it very spectacularly when he concludes that according to Einstein's theory the perihelion of Mercury moves through 42″·9 per century, instead of 531″ as actually found. M. Brillouin (*ibid.*, 923) gives the very obvious reply that Le Roux's statement betrays a complete misunderstanding of the meaning of the Einstein theory of gravitation. But Le Roux is quite unaffected by this (*ibid.*, 1135–6). He writes again (*ibid.*, 1395–7) claiming that Newton's laws of mechanics are not really an approximation to Einstein's; he urges that so far only the case of a single gravitating body has been found to give results very nearly the same in both theories, and prophesies that no such agreement will be found to exist for the field due to several gravitating bodies.

Serious doubts as to whether gravitation can be absorbed into a frame of space and time are raised by J. Larmor (*Proc. Camb. Phil. Soc.*, xxi, 1923, 414–20), who seems to be influenced by some of the arguments of Le Roux. A rather novel view is expressed by L. Lécombe (*Comptes Rendus*, **175**, 1922, 1194–6). He explains the motion of the perihelion of Mercury on the assumption that gravitation is electrical in origin, and finds that the result given by observation is obtained if it is assumed that the dielectric constant of Mercury is 2·4. Another novel view in connection with relativity is given by E. M. Lémeray (*ibid.*, 91–4, 261–3), who suggests that the apparent form of the Milky Way is due to light bending in gravitational fields. He thinks that the universe of stars is really spherical, but that, owing to light-bending, any observer sees the universe in the form of a flattened system. It need hardly be mentioned that Lémeray is an enthusiastic adherent of the relativity view—he shows this particularly in a recent book, *L'Éther actuel et ses précurseurs* (Paris, 1922).

One of the basic ideas of relativity is that the ordinary classical notion of simultaneity is untenable, that in fact there cannot be such a thing as simultaneity except for observers tied down to the same frame of reference. E. H. Synge (*Phil. Mag.*, (6), **43**, 1922, 528–31) claims that the notion of simul-

taneity is involved in the generalised co-ordinate frames used in relativity.

Some interesting points on relativity are raised by M. Brillouin (*Comptes Rendus*, **175**, 1922, 1009–12). The chief one is this: In a static field symmetrical round a point let the part of ds^2 due to the space be $S_1(R)dR^2 + S_2(R)\ (d\theta^2+\sin^2\theta.d\phi^2)$, when S_1, S_2 are functions of the radius vector only. Define the ratio of the circumference to diameter at any point R as $\Pi = 2\pi\sqrt{S_2}/2\int_0^R \sqrt{S_1}dR$. This varies with R, being of course π in empty space far removed from all gravitating matter. A material point is where Π becomes infinite, and the mass of this point-mass is $1/4\pi$ times the circumference of the point-sphere round it. In this way Brillouin defines a point-mass in Einstein's theory, and he goes on to say that the field round such a point-mass is not the same as that round a sphere of finite density.

Applied mathematicians should also find the following papers interesting:

OGURA, K., "Sur le champ de gravitation dans l'espace vide," *Tôhoku Math. Jour.*, **22**, 1922, 14–37.

MURNAGHAN, F. D., "On the Derivation of Symmetrical Gravitational Fields," *Phil. Mag.*, (6), **43**, 1922, 19–31.

MURNAGHAN, F. D., "The Deflexion of a Ray of Light in the Solar Gravitational Field," *ibid.*, 580–8, where the author derives the value of the deflexion without using optical theorems that are proved only for Euclidean space.

JEFFERY, G. B., "The Identical Relations in Einstein's Theory," *ibid.*, 600–3, giving a short proof of these identities.

EDDINGTON, A. S., "The Propagation of Gravitational Waves," *Proc. Roy. Soc.*, A, **102**, 1922, 268–82.

EDDINGTON, A. S., "On the Significance of Einstein's Gravitational Equations in Terms of the Curvature of the World," *Phil. Mag.*, (6), **43**, 1922, 174–7.

DIENES, P., "Sur le déplacement de tenseurs," *Comptes Rendus*, **175**, 1922, 209-11.

CARTAN, E., "Sur un théorème fondamental de M. H. Weyl dans la théorie de l'espace métrique," *ibid.*, 82–5.

CAMPBELL, J. E., "On a Class of Surfaces in Euclidean Space which generate an Expression for the Space-time Interval in Einstein's Geometry of a Particular Form," *Proc. Lond. Math. Soc.*, (2), **21**, 1922, 317–24.

On the nature of gravitation several opinions of interest are to be recorded. Thus A. S. Eddington (*Astr. Jour.*, **46**, 1922, 71–2) thinks that the confirmation of Majorana's results on the absorption of gravitation would act as a support to the Principle of Equivalence—but he has other misgivings, such as that Majorana's results would make a perpetual motion machine possible. Of particular interest is the brief note by P. E. Shaw and N. Davy (*Proc. Roy. Soc.*, A, **102**, 1922, 46–7),

in which is recorded the result of further experiments on the effect of temperature on gravitation. New experiments ranging from 0° to 250° C. show that the positive results announced by Shaw in 1916 have to be rejected. There is no real evidence of such a temperature effect and the matter can now be dropped. A further negative result is mentioned by O. W. Richardson and E. Simons (*Phil. Mag.*, (6), **43**, 1922, 138–45). Experiments were carried out based on the idea that gravitation is due to slight modifications of the law of force between positive and negative electrons, and no results were obtained. Richardson considers this failure as tending to confirm Einstein's theory of gravitation.

The reader should also refer to the following papers dealing with problems involving the theory of potential:

HUMBERT, P., "Sur les surfaces de Poincaré," *Jour. l'Éc. Poly.*, (ii), **20**, 1919, 1–82, dealing with the theory of rotating fluids.

RUSSELL, A., "The Problem of Two Electrified Spheres," *Proc. Phys. Soc.*, **34**, 1922, 10–29.

KENWARD, E. H., "On a Simplified Proof for the Retarded Potentials and Huyghens's Principle," *Phil. Mag.*, (6), **43**, 1922, 1014–17.

BATEMAN, H., "On Lines of Electrical Induction and the Conformal Transformations of a Space of Four Dimensions," *Proc. Lond. Math. Soc.*, (2), **21**, 1922, 256–70.

Considerable interest was aroused by the remarkable gliding feats of last summer. It is hardly necessary to write about these flights in any detail, because everybody will have read about them in the daily and weekly press. But a considerable number of papers have appeared recently dealing with the problem of gliding and soaring flight. An excellent account of the art of gliding as based on mechanical principles is given in a paper by C. Runge entitled "Über den Segelflug" (*Die Naturwissenschaften*, x, 1922, 879–82). Runge emphasises several important points that are often forgotten by "popular" writers. Rather similar in content is the article by S. Brodetsky (*Nature*, Sept. 6, 1922), who urges that the flight without a motor should be termed *wind-flight*, in order to make clear that it is a kind of flight which depends upon the existence of wind, either with upward component, or horizontal but irregular: this has been approved by some other writers.

G. T. Walker (*Proc. Camb. Phil. Soc.*, xxi, 1923, 363–75) discusses the meteorological conditions associated with the non-flapping flight of tropical birds. He controverts the view expressed by Hankin that there is some mystery about the supply of energy in such flight, and asserts his belief in the existence of vertical currents explaining the flight. Observations on soaring flight are recorded by H. Fabre (*Comptes Rendus*, **175**, 1922, 1042–4). They refer to flights across the Mediterranean, and

the writer mentions two types of such flight. In one the direction of flight was perpendicular to the lines formed by the wave-crests, giving effects similar to the gliding flights over hilly ground where upward currents of air are produced. In the other type the flight was parallel to the waves, the bird keeping in the upward current produced by one row of waves. Further observations are given in three short papers by E. H. Hankin, entitled "Flying Fishes and Soaring Flight," "On the Air-brake used by Vultures in High-speed Flight," and "Soaring Flight of Gulls following a Steamer" (*Proc. Camb. Phil. Soc.*, xxi, 1923, 421–3, 424–5, 426–9, respectively).

But the most important scientific issue involved in recent work in aeronautics is in connection with the theory of aerofoil pressure. How is it that the type of aerofoil section used in practice produces such a large lift for so small a drag? Many years ago Lanchester suggested in his *Aerodynamics* that this was to be explained by the existence of a "circulation" round the wing, giving greater relative velocity of the air above the wing than below, and thus producing greater pressure below than above. Little was done with this as regards the calculation of actual results till L. Prandtl, of Göttingen, took the matter in hand and gave the suggestion a mathematical form—he also incorporated some ideas of Joukowski and Kutta in order to get more satisfactory results.

There can be no doubt that the Prandtl theory is a distinct advance in our treatment of the problem of the aerofoil. Results have been obtained that agree very well with observation, and even its opponents, like L. Bairstow, admit this. A full account of the theory was published recently by the American National Advisory Committee for Aeronautics (Report 116), the author being Prandtl himself. This report is rather difficult to obtain in this country, but several papers by H. Glauert and others supply the deficiency somewhat, and add some new developments.

In his paper "Aerofoil Theory" (*Aeronautical Research Committee, Reports and Memoranda*, 723, 1921, 20 pp.) Glauert gives a brief account of the Prandtl theory. He deals first with the fundamental two-dimensional problem, or "aerofoils of infinite span." He then considers the finite monoplane wing and the pressure distribution over it. Biplanes are treated next, and then the influence of walls and free boundaries of streams, in order to get results applicable to wind-channel and other experiments. It is interesting to note how the effect of aspect-ratio can be deduced mathematically. In a later paper (*ibid.*, 752, 16 pp.) the theory is compared with experimental results. Glauert contributes a third paper, "An Aerodynamical Theory of the Airscrew" (*ibid.*, 786, 20 pp.);

in which the Prandtl theory is used to examine the problem of the airscrew hydrodynamically. The interesting result emerges that Glauert's theory justifies the use of two-dimensional aerofoil values on the various elements of the screw, with a correction factor which is theoretically established.

The Prandtl theory is also dealt with in a paper by R. McKinnon Wood (*Aer. Jour.*, xxvi, 1922, 480–501). An extended discussion of the theory is to be found, however, in the report of a paper read by A. R. Low called "Review of Airscrew Theories" (*ibid.*, xxvii, 1923, 37–72). The weaknesses of the Prandtl theory are here laid bare, the chief critic being L. Bairstow. He admits that the theory is very good empirically, but he claims that it is not well established theoretically. He asks: "When should a circulation be applied and when not; how much should be applied?" Bairstow emphasises the necessity of proceeding, no matter how laboriously, with the integration of the equations of viscous motion. He and his collaborators are doing so: some work has already been published, and we must wait for further results.

An interesting and at first sight paradoxical application of air pressure is that described by Constantin, Joessel, and Daloz (*Comptes Rendus*, 175, 1922, 683–5). The nature of the application is adequately described by the title of their paper: "Sur un bateau qui remonte le vent en se servant du vent lui-même comme puissance motrice." Successful sailings with such boats are recorded. The idea is to have an airscrew which is driven round by the wind blowing from the front, and which by its rotation produces a forward force on the boat.

Two fundamental papers on the dynamics of aeroplane motion are published by G. H. Bryan. In the first paper, "The Canonical Forms of the Equations of Motion of an Aeroplane in Still and Gusty Air" (*Aer. Res. Comm., R. & M.*, 1921, 689, 43 pp.), Bryan gives the results of work carried out partly in collaboration with S. Brodetsky and D. Williams. The idea is to divide up the motion into portions in each of which the resultant velocity of the centre of gravity of the machine makes an angle with the propeller axis lying between certain definite limits. In each such zone the motion is considered by first neglecting all forces like gravity, propeller thrust, etc., and even some terms of the air-forces themselves. Using a new variable $s = \int U_1 dt$, where U_1 is the velocity component in the forward direction as seen by the pilot, in fact the "log-run," and introducing an idea comparable with that of normal co-ordinates in the Lagrange theory of vibrations, Bryan deduces a discriminating cubic whose roots can be used to obtain the complete solution of the problem. The terms and

forces omitted are introduced in the form of integrals. The numerical work is due to Williams. In order to deal with the integrals, use is made of series suggested by Brodetsky. They are essentially modified forms of the sine and cosine series, being in fact rather simple hypergeometric series.

The second paper, "The Theory of Initial Motions and its Applications to the Aeroplane" (*ibid.*, 744, 25 pp.), shows how to use Maclaurin's series for dealing with initial motions of aeroplanes after being disturbed by a gust or any other agency. In both papers the notation of Brodetsky's book *Mechanical Principles of the Aeroplane* is adopted.

N. Basu (*Bull. Calc. Math. Soc.*, xiii, 1922, 141–50) discusses the stability of an aeroplane in circling flight, using the type of laterally stable aeroplane as calculated by Bryan. He obtains a biquadratic, but surely the argument is not applicable to an aeroplane up in the air. Basu, in fact, assumes that in the disturbed motion the centre of gravity moves in a horizontal plane. Further dynamical theory is dealt with in two papers on Helicopters by J. Case (*Aer. Jour.*, xxvi, 1922, 390–407, 435–47). The author deals with the hovering helicopter, with descent, forward motion by inclination of the screw axis, gliding, performance, and stability. The stability condition for vertical flight leads to a determinantal equation of the tenth degree—but the discussion is only preliminary, since we still know insufficient about the behaviour of airscrews moving in directions inclined to their axes.

Papers of historical interest in aeronautics are contributed by A. Ogilvie, "Some Aspects of Aeronautical Research" (*ibid.*, 381–9), and L. Bairstow, "The Work of S. P. Langley" (*ibid.*, 420–34). The latter mentions the interesting and significant fact that the glider on which Maneyrol performed his great glide last summer, beating the German record of over three hours' stay in the air, was of very much the same form as the man-carrying aeroplane constructed by Langley. This is also mentioned in a note issued by the Smithsonian Institution on November 28, 1922.

Mention should be made of the papers set at the examination for the Associate Fellowship of the Royal Aeronautical Society, held in September 1922 (*Aer. Jour.*, xxvi, 1922, 448–55). There are six papers. Three of them will interest applied mathematicians. Paper I deals with strength of materials, elasticity, and structures, in the engineering way. Paper II deals with aerodynamics, the questions testing practical knowledge rather than theory. Paper IV is on mathematics, the standard being not unlike that of a final pass standard at the Universities.

The following papers should be noted:

FRAZER, R. A., " Juvenile Lecture—Modern Aircraft," *ibid.*, xxvii, 1923, 105–13, which contains some good flow patterns of fluids round obstacles.
CASE, J., " Stresses in Airscrews due to Varying Engine Torque," *ibid.*, xxvi, 1922, 321–4.
MORRIS, J., " The Vibration of Airscrew Blades," *ibid.*, 472–5.
MORRIS, J., " On the Stability of Aero Engines," *ibid.*, xxvii, 1923, 182–217.

A considerable amount of work on hydrodynamical problems is to be recorded. The study of hydrodynamics has received a great impetus from the extensive researches on the aerodynamical properties of various types of bodies, aerofoils, struts, airships, etc. Very important contributions to the subject have been made by G. I. Taylor. In a paper on the " Motion of a Sphere in a Rotating Liquid " (*Proc. Roy. Soc.*, A, **102**, 1922, 180–9) Taylor describes some beautiful experiments in which he shows that when a sphere moves through a rotating fluid there is a sheath of fluid round the sphere which is not in rotation. This follows a lengthy paper on the mathematics of the problem by S. F. Grace : " Free Motion of a Sphere in a Rotating Liquid Parallel to the Axis of Rotation " (*ibid.*, A, **102**, 1922, 89–111). In another paper, " Stability of a Liquid contained between Two Rotating Cylinders " (*ibid.*, 1923, 541–2, and *Phil. Trans.*, A, **223**, 1923, 289–343), Taylor examines a special type of instability that arises, changing the motion from two-dimensional to three-dimensional. The problem is dealt with both when the cylinders are rotating in the same sense and in opposite senses. Lord Rayleigh's results are verified experimentally. In particular the spiral type of motion that arises is also considered. Taylor's theoretical and experimental researches are doing very much to aid us in obtaining a more thorough understanding of fluid motions in general.

The problem of discontinuous fluid motion is one of considerable mathematical interest, even if for applications to aerodynamical problems the work of Prandtl and Bairstow and their followers is more immediately useful. Basing himself on the general solution of the discontinuous fluid motion past any barrier given by Levi-Civita and later developed by Cisotti, Brillouin, Villat, and others, Brodetsky (*Proc. Roy. Soc.*, A, **102**, 1923, 542–53) calculates, by a process of successive approximations, the pressures on circular and on elliptic cylinders, the motion of the stream being parallel to one of the principal axes of the latter. In another paper (*ibid.*, A, **102**, 1922, 361–72) the same writer calculates the general formula for the position of the line of action of the resultant pressure for any barrier.

Vortex motion has to be considered from many points of view. While Prandtl deals with the aerofoil problem by

the circulation method, others are attempting to deal with the turbulence introduced by the motion of a body through a fluid. Thus Villat contributes a paper: "Sur les mouvements plans tourbillonnaires dans un fluide simplement et doublement connexe, contenant des parois solides" (*Comptes Rendus*, **175**, 1922, 445–6). D. Riabouchinski writes: "Sur les équations du mouvement à deux dimensions de solides dans un liquide avec tourbillons" (*ibid.*, 442–5). W. M. Hicks considers the mutual threading of thin circular vortex rings (*Proc. Roy. Soc.*, A, **102**, 1922, 111–30), while N. Sen discusses in considerable detail the exact mathematical solution of circular vortex rings of finite section in incompressible fluids (*Bull. Calc. Math. Soc.*, xiii, 1922, 117–40).

G. B. Jeffery, in his paper "The Motion of Ellipsoidal Particles immersed in a Viscous Fluid" (*Proc. Roy. Soc.*, A, **102**, 1922, 161–79), introduces the hypothesis that these particles assume definite orientations in relation to the motion of the fluid. This is examined experimentally by G. I. Taylor (*ibid.*, A, **103**, 1923, 58–61), who uses particles made of aluminium moving in waterglass. He finds that the particles do assume such orientations, but only after a considerable time, and that till then there are oscillations as indicated by Jeffery's mathematical analysis.

Other papers that should be consulted are:

Woodward, R. S., "Some Extensions in the Mathematics of Hydromechanics," *Proc. Nat. Acad. Sci. U.S.A.*, **9**, 1923, 13–18, who considers in particular the equations of viscous motion and emphasises the value of what he calls preharmonic functions, namely, solutions of the equation $\nabla^4\psi = 0$, called biharmonics by European writers.

Camichel, C., "Sur le régime turbulent," *Comptes Rendus*, **175**, 1922, 743–5.

Lamb, H., "On Water Waves due to Disturbance beneath the Surface," *Proc. Lond. Math. Soc.*, (2), **21**, 1922, 359–72.

Mitra, S., "Surface Waves due to a Submerged Elliptic Cylinder," *Bull. Calc. Math. Soc.*, xiii, 1922, 167–74.

Miyagi, O., "Notes on the Draught Tube of a Water Turbine," *Tech. Rep. Tôhoku Imp. Univ.*, iii, 1922, 57–69.

Green, G., "Some Problems relating to Rotating Fluid in the Atmosphere," *Phil. Mag.*, (6), **41**, 1921, 665–75; **42**, 1921, 200.

Brunt, D., "The Dynamics of Revolving Fluid on a Rotating Earth, *Proc. Roy. Soc.*, A, **98**, 1921, 397–402.

On the general principles of mechanics and their applications the following papers are of interest:

Hall, F. G., Notes on "A Treatise on the Rectilinear Motion and Rotation of Bodies" by George Atwood, M.A., F.R.S. (Cambridge, 1784), *Math. Gaz.*, xi, 1922, 108–10.

Réveille, T., Examen critique du Mémoire ed Poinsot sur la "Théorie Nouvelle de la Rotation des Corps," *Jour. l'Éc. Poly.*, ii, **20**, 1919, 107–14.

TAYLOR, G. I., "A Relation between Bertrand's and Kelvin's Theorems on Impulses," *Proc. Lond. Math. Soc.*, (2), **21**, 1923, 413–14, who proves that the reduction in energy due to the imposition of constraints in the Bertrand case is less than the increase in energy due to the imposition of the same constraints in the Kelvin case.

HAYASHI, T., "Some Problems in the Theory of the Cyclone," *Sci. Rep. Tôhoku Imp. Univ.*, xi, 1922, 87–93, dealing with kinematical problems.

BROMWICH, T. J. I'A., "Kinetic Stability," *Phil. Mag.*, (6), **43**, 1922, 70–2, who points out an error in Jeans' "Problems of Cosmogony and Stellar Dynamics," and discusses the method of small oscillations for the study of stability. He shows that in general the method is sound, except for critical cases.

BAKER, B. B., "The Vibrations of a Particle about a Position of Equilibrium," Part II, "Relation between Series and Elliptic Function Solutions," *Proc. Ed. Math. Soc.*, xl, 1922, 34–49.

POPOFF, K., "Sur l'intégration des équations de la Balistique dans des conditions générales de la résistance," *Comptes Rendus*, **175**, 1922, 337–40.

ANDRADE, J., "Les déterminismes mécaniques et la notion de milieu, orbites pseudo-elliptiques et orbites circulaires," *ibid.*, 138–40, dealing with resisted motion.

WRINCH, D., "On the Orbits in the Field of a Doublet," *Phil. Mag.*, (6), **43**, 1922, 993–1014, dealing with a very interesting problem in particle dynamics.

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ASTRONOMY. By H. SPENCER JONES, M.A., B.Sc., Royal Observatory, Greenwich.

The Further Verification of Einstein's Theory.—Although only a very summary report is to hand, at the time of writing, of the results obtained by the Lick Observatory and the Canadian

expeditions which observed the eclipse of September last in Australia, and although the results were widely referred to in the daily press, some reference to them in these notes is appropriate as bearing upon the progress of astronomy.

Dr. Campbell's announcement of the results obtained by the Lick Observatory expedition states that three of the eclipse plates were compared with three comparison plates previously secured at Tahiti. These plates were obtained when the eclipse region was at the same altitude as during the eclipse. The plates showed some seventy or so stars, and although to obtain so large a number necessitated a rather long exposure with the consequent risk of the nearer stars being drowned in the corona, it is probable, judging from the shape of the corona as revealed in such reproductions as are available, that some at least of the nearer stars were shown. As the photographs were obtained under excellent conditions, a reliable determination of the deflection was to be expected from these plates. The announcement states that the deduced values for the deflection of a ray which passes the sun at grazing incidence ranged from 1″·59 to 1″·86 with a mean value of 1″·74, Einstein's predicted value of the deflection being 1″·75. Although it is not possible to judge of the internal agreement of the deflections as given by the different stars until the detailed results are published, it is evident that the Lick observers are themselves perfectly satisfied with them, as Dr. Campbell states that observations to determine the gravitational deflection will not form part of the programme of the Lick Observatory expedition which will observe the eclipse of September 10 next. We may assume, therefore, that very satisfactory results have been obtained.

A brief announcement has also appeared of the results secured by Prof. Chant, who was in charge of the Canadian party which also went to Australia. It would seem, from the very scanty details which are to hand, that the results are not of the same order of accuracy as those obtained by Dr. Campbell, but at the same time they also support Einstein's value of the deflection. The report states that the results are not of sufficient accuracy to give decisive information as to the law of falling off of the amount of the deflection with increasing distance from the sun. It is rather probable that the images were somewhat diffuse, giving rise to somewhat increased errors of measurement. The announcement from the Lick Observatory does not make any mention of the information obtained on this point, which is one of some considerable interest. It may be recalled that the results of the 1919 expedition to Brazil gave a somewhat steeper gradient than Einstein's law. It would be of interest to know whether this was a purely

accidental result or whether there is a residual displacement due to refraction of the light by the solar corona. Should such a result be confirmed, it might assist in throwing some light on the nature of the corona. Perhaps the Lick results will enable this point to be settled.

Meanwhile, there can now be little doubt that there is a deflection of the rays of light passing through the gravitational field of the sun which is of the amount predicted by Einstein. It is an important thing to have this settled once and for all. It is already evident that the opponents of the theory are not thereby convinced. The present position is that the theory has accounted for the motion of the perihelion of Mercury, for which no satisfactory explanation was previously available, and that it has *predicted* the amount of the deflection of light rays by a gravitational field which observation has adequately confirmed. The opponents of the theory are not able to advance a satisfactory explanation of either of these phenomena, and therefore their attitude can hardly be regarded as a logical one.

The Constitution of the Outer Planets.—In a paper which appears in *M.N.*, *R.A.S.*, **83**, No. 6, 1923, Dr. Jeffreys advances the view that the four outer planets are cold and solid bodies. As the hitherto generally accepted view has been that they are hot and gaseous, the arguments upon which this change of view is based require careful examination. It was shown by Poynting in 1903 that the temperature of an ideal planet (by which is meant a planet satisfying certain conditions specified by Poynting in order that exact mathematical treatment might be given to the problem, but which need not be here specified) at the distance from the sun equal to that of the earth would be 312° absolute, at the distance of Mars would be 253°, and at the distance of Neptune would be 56°. These figures may be expected to be of the right order of magnitude provided that the planets do not radiate heat of their own. It has been generally assumed that the four outer planets do radiate heat, in which case Poynting's results do not apply.

Dr. Jeffreys advances reasons to refute this view. He shows first that if the interior of the planet were in complete thermal communication with its surface and no heat were being received from outside, the planet must by now have solidified on any reasonable hypothesis concerning its initial temperature. As soon as solidification set in there would no longer be free thermal communication, but instead a slow conduction of heat through the surface. The surface temperatures must therefore be controlled by the rate of supply of heat to the surface by solar radiation and by the slow conduction outwards from the interior. In the case of the earth it is shown that the predominant factor is the amount of heat received from the sun. If the external

portions of the other planets may be regarded as similarly constituted to the earth, it easily follows that, even at the distance of Neptune, the heat received from the sun, which decreases per unit area inversely as the square of the distance, will still predominate. Unless, therefore, there is considerable selective absorption, the surface temperature of the outer planets must be lower than the earth, still assuming that the surfaces are similarly constituted. Thus far, doubtless, Dr. Jeffreys' argument will carry assent. But the question is raised whether the surfaces can possibly be regarded as similarly constituted.

In the first place, Jupiter and Saturn, and probably the two outermost planets also, are densely covered with clouds, and the outer cloud layers may be supposed to be the effective radiating and absorbing layer. By arguing from the theory of the general circulation of the earth's atmosphere, it is concluded that the depth of the homogeneous layer equivalent to the atmosphere of Jupiter is of the order of 1/150 of the radius, and in the case of Saturn of the order of 1/20 of the radius. It is usually believed that the depths of the atmospheres are greater than this. It is probable that most experienced observers of the planets would fail to be convinced by this part of the argument.

The mean densities of the outer planets are much lower than the mean density of the earth, that of Saturn being less than water. Their constitution must therefore be very different from that of the earth. Dr. Jeffreys considers that they are solid bodies, possibly largely composed of ice, with a cold opaque atmosphere which below the cloud layers is approximately isothermal. It does not seem impossible that such may be the case, although no conclusive evidence is advanced. Dr. Jeffreys' paper will at least make it clear that the temperatures of the outer planets may be much lower than has commonly been supposed, though it must not be thought that he has in any sense proved that such must be the case.

The Atmosphere of Venus.—One important source of our knowledge of the planetary atmosphere is derived from a study of their spectra. The spectra of the sun and of stars are always found to contain certain dark bands in the red which are known to be produced by absorption in the earth's atmosphere. The planets shine by reflected sunlight, and therefore their spectra should be similar to that of the sun except in so far as their own atmospheres may give rise to additional absorption lines. It does not, of course, necessarily follow that, if additional absorption lines are absent, the planet must be without an atmosphere. Certain substances such as nitrogen do not produce any appreciable absorption lines, and, on the other hand, it may be that the light is reflected in

the outer layers of the atmosphere and does not penetrate sufficiently far for appreciable absorption to be produced in any case.

Two important studies of the spectrum of Venus have recently been made. At Mount Wilson, some excellent spectra have been obtained by St. John and Nicholson with a dispersion of 3A per mm. (*Astroph. Jour.*, **56**, 380, 1922). The spectra were secured at a time when the relative velocity of the earth and Venus was sufficient completely to separate the corresponding lines produced by the two atmospheres. These spectra failed to show any trace of lines which could be attributed to the atmosphere. The meaning of this result is carefully discussed by the authors. For some of the spectrograms secured, the path of the light was as much as five times the radial depth of the layer penetrated. For the light coming from near the terminator the path was longer still. From considerations of the intensities of the lines which may be expected under the conditions of the observations, it is concluded that, in the path traversed by the solar light through the atmosphere of Venus, there must be less than the equivalent of one metre of oxygen, which is less than the one-thousandth part of that in our atmosphere, and less than one millimetre of precipitable water vapour. There is therefore no appreciable amount of water vapour above the visual surface of Venus. The question is, where is the visual surface with regard to the actual surface of the planet? This is a question upon which very little light can be thrown by the evidence at present available. St. John and Nicholson discuss it carefully without reaching any definite conclusions. They hope later to obtain evidence as to whether light reflected from the continually evaporating surfaces of clouds will give absorption lines of observable intensity. They also point out that important evidence might be obtained from the relative colour indices of Venus and terrestrial clouds. It cannot at present even be stated whether the reflecting layer is composed of cirro-strati, of haze, or of clouds of dust produced by violent atmospheric circulations. This much at least is, however, certain, that there is not any evidence at present that Venus has an atmosphere as dense or denser than that of the earth, as many of the earlier investigators believed.

The same question has also been dealt with by Slipher (*Lowell Obs. Bulletin*, No. 84). Venus spectra were obtained with a high-dispersion single-prism spectrograph, in the winter when the atmosphere at Flagstaff is exceptionally free from water vapour. These showed no trace of water-vapour absorption due to Venus. Slipher states that the high albedo and telescopic appearance of Venus seem to imply that our

view of her is mainly a super-surface one, which may not be appreciably affected by light returned from her surface. In view of the concentration of the water vapour in our atmosphere to its lowest stratum, caution is necessary about concluding that water is absent from Venus. He points out that, although there is adequate evidence of extensive atmospheres about Jupiter and Saturn, their spectra show only moderate absorptions. Moreover, a previous study of the Earthshine spectrum led to the conclusion that the greater part of the light reflected by the earth is reflected by its atmosphere and not by its surface.

It is worth noting that not only have these investigations failed to detect oxygen and water vapour in the atmosphere of Venus, but they have also failed to reveal any absorption bands due to other substances.

The Luminosity of Nebulæ.—The account of an important investigation by Hubble into the source of luminosity in nebulæ appears in *Astroph. Jour.*, **56**, 400, 1922. Starting from the fact that there appears in general to be a definite relationship between the spectra of the nebulæ and those of the stars associated with them, in the sense that if the stellar spectra are earlier than B1 the nebular spectrum is emission, whilst if the stellar spectra are later than B1 the nebular spectrum is continuous, Hubble was led to the consideration of the supposition that the nebulæ owe their luminosity to radiations from the associated stars. If each part of the nebula reflects or re-emits all the starlight incident upon it, then a being the measured maximum angular extent of the nebulosity and m the apparent magnitude of the star, a relationship of the form $m + 5 \log a = \text{const.}$ should hold, and for a standard exposure and a given type of plate for which the exponent in the reciprocity law is known, the constant can be calculated. From photographs of many nebulæ taken with widely varying exposures, Hubble finds that this relationship is very closely adhered to, and that the experimentally found value of the constant is in good agreement with the calculated value. The principal outstanding cases can be explained by supposing that the apparent brightness of the stars is diminished by nebular absorption between the star and the observer. This is confirmed in many of the cases by the large colour excesses shown by these stars.

The close association between stars and nebulæ has long been known. In particular many of the so-called dark nebulæ found by Barnard are associated with stars which are surrounded with nebulosity. But it is well to have this proof of the very close relationship existing between the two and to have it established that the nebular luminosity is in most cases at any rate due directly to reflected starlight.

PHYSICS. By J. Rice, M.A., University, Liverpool.

Atomic Structure.—In No. 65 (July 1922) a brief account was given in these notes of a statical model of the atom proposed by Lewis and Langmuir. The feature of this model, which is of great service in describing the chemical properties of the elements, lies in the postulate that the electrons are held at or near certain positions of equilibrium with regard to the nucleus. The electrons are arranged in layers or "shells"; each shell is divided into "cells," and any one electron is confined to a cell. As we proceed through the periodic table the cells are occupied one by one, and on arriving at an inert gas a layer is completed. Thus helium has 2 electrons occupying the first shell; neon has 8 more each occupying the first layer of the second shell; argon still 8 more occupying the second layer of the second shell. The next shell contains two layers each with 18 cells to be successively filled, the inner layer being filled in the case of krypton and the outer in the case of xenon. Then there is a layer of 32 cells which are just fully occupied in the case of niton. The remaining elements are formed by adding electrons to a few cells of the outer layer of the fourth shell.

Despite the great service which this geometrical model has performed for the chemist in describing the phenomena of "chemical statics," it is, in the opinion of physicists, inadequate to explain such important facts as spectroscopic phenomena, for example; and it bears no resemblance to the planetary models, with each electron describing an orbit around the atom centre, which the physicist has been led to construct in his attempt to satisfy dynamical laws. The difficulty which meets the physicist, on the other hand, consists in discovering in the principles of dynamics any satisfactory explanation of the remarkable stability which his models must possess in view of the permanence of the elements. In his endeavour to do so, he has favoured models with coplanar orbits for the electrons; but it is easy to see that such models cannot meet the demands of the chemist, and yet the adoption of orbits in different planes cannot on the older theory be reconciled with stability and permanence. It was this which led Bohr, in 1913, to introduce an entirely new idea into atomic physics, *viz.* that it is useless to search for a principle of stability for electronic orbits in the body of laws which had been based on observation of the solar system and of movement of matter in bulk; it was necessary to generalise our laws of dynamics in a rational manner and add to them the smallest number of postulates which would meet the new conditions. In keeping with this idea he suggested his principle of stationary states. Out of the infinity of orbits which an electron could pursue

round a nucleus on the classical theory, only a limited number possess the property of permanence; *i.e.* an electron can travel in them without the gradual loss of energy which should accompany accelerated motion. In fact, energy cannot be added to or removed from the atom except by a process involving a transition of the atom from one stationary state to another, in which at least one electron would pass from one of the discrete orbits possible to it to another of these orbits. In order to define more precisely what these orbits are, there is available first of all the fact that they agree with the ordinary dynamical laws; in addition Bohr postulated that the frequency of the radiation emitted or absorbed when an electron jumps from one orbit to another is given by the quantum condition

$$h\nu = E' - E'' \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where h is Planck's constant and E' and E'' are the energies of the two states. Now a considerable body of knowledge concerning the radiations emitted and absorbed by various elements is summarised in the laws of optical series, of which the Balmer series for the hydrogen lines is one of the best known and simplest. Thus (1) combined with such series formulæ gives us the difference of energies for any two of the admissible states. By taking one of the states to correspond to the removal of the electron to a great distance from the nucleus and ascribing an arbitrary constant value to the energy of that state we can obtain the energy values for the others. This defines the orbits to some extent, and in the case where there is only one electron in the atom, *i.e.* the case of hydrogen or *ionised* helium, the mathematical operations are sufficiently simple to enable one to give great precision to the sizes and forms of the orbits. Thus in the case of hydrogen it can be shown that one group of orbits consists of circles whose radii, r_n, are given by

$$2r_n = n^2 . e^2/hR$$

where e is the electronic charge and R is the Rydberg constant which occurs in series laws; n is an integer, which has the value unity for the normal condition of the hydrogen atom, and is called the quantum number of the orbit. For *ionised* helium, the circular stationary orbits are a quarter of the size of the hydrogen, *i.e.*

$$2r_n = n^2 . e^2/4hR.$$

Another group of hydrogen orbits consists of ellipses with the nucleus as focus, whose major axis, $2a$, and latus rectum, $2p$, are given by

$$\left.\begin{aligned} 2a &= n^2 . e^2/hR \\ 2p &= k^2 . e^2/hR \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where n and k are a pair of integers, the principal and secondary quantum numbers. For a given value of n, k may take any value from 1 to n (when $k = n$, the orbit is one of the previous circles). For ionised helium R is replaced by $4R$ in (2).

When it comes to applying these ideas to atoms with several electrons revolving round the nucleus, the mathematical difficulties involved in a complete solution are so formidable that only very general conclusions can be drawn. But it is significant that the theory suggests that the orbits of the inner electrons which are concerned in the X-ray spectra should approximate to ellipses given by

$$\left.\begin{aligned} 2a &= n^2 . e^2/N^2hR \\ 2p &= k^2 . e^2/N^2hR \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (3)$$

where $+ Ne$ is the nuclear charge, or N the atomic number; the series laws of X-rays are certainly consistent with the energies of these orbits.

Recently Bohr has made a further step towards a clearer idea of orbital paths by studying successively, as far as they are known, the arc and spark spectral series of the elements one by one as we proceed through the periodic classification. The idea on which his work is founded is as follows: Suppose we are dealing with an element of atomic number N, and suppose that we consider the system made up of the nucleus $+ Ne$ and $N - 1$ electrons moving in steady orbits (*i.e.* "bound," let us say), with the Nth electron free. This Nth electron, in returning to one of its admissible orbits or in jumping from one of these orbits to another, will give rise to a series of lines. Now if we consider the nucleus and $N - 1$ "bound" electrons as approximately equivalent to a unit positive charge at the nucleus, the resulting series ought to bear some resemblance in its mathematical formulation to the Balmer series for hydrogen. Now it is well known that this is true for the arc spectra of the elements. So the formulæ for such spectra should give us information concerning the energies of the orbits possible to the last electron captured in the formation of the atom. On the other hand, let us consider a system made up of the nucleus $+ Ne$ and $N - 2$ bound electrons. This is equivalent roughly to a nucleus with double charge, and the $(N - 1)$th electron, in returning to one of its admissible orbits or leaping from one to another of them, should give rise to a spectrum having points of resemblance with the spectrum of ionised helium. Now this is known to be the case for the spark spectra of the elements. Thus Bohr suggests as a guiding principle that the arc spectra are connected "with the last stage in the formation of a neutral atom consisting in the capture and binding of the Nth electron," while "the spark

spectra are connected with the last stage but one in the formation of the atom, namely the binding of $(N-1)$th electron." With the assistance of the formulæ for these spectra he proceeds to pass in review the elements in each period, basing himself on reasoning of a general nature, and to frame a picture of the orbits of the various electrons which, while not detailed, is plausible on dynamical grounds and seems to conform more closely with the demands of the chemist than previous physical models. To each orbit is attached a pair of quantum numbers such as n and k in (2) and (3), and these numbers serve to give some definition to its size and shape (the shapes are not in general the simple ellipses referred to above, but may approximate to them in certain parts of the orbits). Thus to take the case of sodium, which is the 11th element in the periodic classification, its arc spectrum gives the data necessary to give an approximate picture of the orbit of its last electron to be captured, *i.e.* the 11th. It is then assumed that this orbit resembles the orbit of the 11th electron in any succeeding element. An important feature of Bohr's new ideas is the fact that his principle of stability no longer compels us to adopt coplanar orbits. The normals to the orbital planes are, in fact, supposed to be arranged in space somewhat like the lines joining nucleus and electron in the Lewis-Langmuir model. This certainly offers a closer approach to the possibility of meeting the chemist's requirements. Another feature worth mentioning lies in the possibility of an electron, which might be called an " outer electron " (because a large part of its orbit passes out of the region occupied by the orbits of the other electrons), and yet travels close into the nucleus for a part of its path, resembling a comet of our solar system. This is quite distinct from the Langmuir idea of successive layers of approximately static electrons. Another important difference lies in the grouping of the electrons in each period. This can best be indicated by a table. (The Langmuir grouping is indicated at the beginning of this article and is given in the previous article referred to.) In this table the number of electrons in an n, k orbit is given, where n and k are the quantum numbers mentioned earlier. An orbit with $k = n$ is circular; the orbits with the same n but decreasing k are approximately ellipses with the same major axis, but increasing eccentricity. The atoms quoted belong to the inert elements.

These ideas were first propounded in an address to the Physical and Chemical Societies in Copenhagen, October 18, 1921. They appeared in a paper to the *Zeit. für Physik* of 1922. A translation of that paper forms the last of three essays by Bohr on the *Theory of Spectra and Atomic Constitution* recently published by the Cambridge University Press. A long article

$n =$	1	2	2	3	3	3	4	4	4	4	5	5	5	5	5	6	6	Total
$k =$	1	1	2	1	2	3	1	2	3	4	1	2	3	4	5	1	2	
Helium .	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2
Neon .	2	4	4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	10
Argon . .	2	4	4	4	4	—	—	—	—	—	—	—	—	—	—	—	—	18
Krypton .	2	4	4	6	6	6	4	4	—	—	—	—	—	—	—	—	—	36
Xenon .	2	4	4	6	6	6	6	6	6	—	4	4	—	—	—	—	—	54
Niton . .	2	4	4	6	6	6	8	8	8	8	6	6	6	—	—	4	4	86

with some very interesting diagrams has been published as a Supplement to *Nature* just as this note goes to press (July 7). Bohr appears to be still engrossed on elucidating the dynamical points involved, as he has recently contributed to the *Zeit. für Physik* (Bd. 13, Heft 3, 1923) a paper which is the first of a series whose purpose is to explain systematically the problems which one has to treat in dealing with atomic structure.

ORGANIC CHEMISTRY. By O. L. Brady, D.Sc., F.I.C., University College, London.

Synthetic Methods and Reagents.—Davies and Perkin (*Trans. Chem. Soc.*, 1922, **121**, 2196) have described a satisfactory method of preparation of the phthalaldehydic acids, $C_6H_4(COOH)CHO$. These compounds, which offer many interesting possibilities as starting materials for the synthesis of ring compounds, have been little used owing to the difficulty of preparing reasonable quantities of them. The above authors find that by the chlorination of the three toluoyl chlorides (readily obtained from the toluic acids by means of thionyl chloride) in bright sunlight at from 160°–210°, 80 per cent. yields of ω-dichlorotoluoyl chlorides are obtained; these compounds on hydrolysis with an aqueous suspension of calcium carbonate in an inert atmosphere give good yields of the corresponding phthalaldehydic acids.

$$CH_3 . C_6H_4 . COOH \rightarrow CH_3 . C_6H_4 . COCl$$
$$\rightarrow CHCl_2 . C_6H_4 . COCl \rightarrow CHO . C_6H_4 . COOH.$$

The preparation of ω-chloro-toluic esters, $CH_2Cl . C_6H_4 . COOEt$, and other compounds of this type, all likely to be of importance in synthetic operations, is also described.

The Rosenmund method for the preparation of aldehydes by the reduction of acid chlorides by means of hydrogen in hot xylene solution in the presence of palladium has been extended. Rosenmund and others (*Ber.*, 1921, **54**, [B], 2888; 1922, **55**, [B], 609, 2357) have found that sebacyl, suberyl, isophthalyl, and

terephthalyl chlorides give 75–85 per cent. yields of the corresponding dialdehydes, but *s*-*o*-phthalyl chloride under these conditions gives mainly phthalide. By the reduction of triacetyl-galloyl chloride good yields have been obtained of triacetyl-gallaldehyde, and from this, by boiling with an alcoholic solution of potassium acetate, the very reactive gallaldehyde.

Unusual difficulties have been encountered in obtaining the hydroxy-cinnamaldehydes, as the ease with which they are resinified by alkalies inhibits their preparation from the hydroxy-benzaldehydes by condensation with acetaldehyde in the presence of alkali. Tiemann and Kees (*Ber.*, 1885, **18**, 1955) synthesised a number of these compounds by using a glucoside; for example, helicin ($C_6H_4(O.C_6H_{11}O_5).CHO$) was condensed with acetaldehyde by means of alkali and the resulting compound ($C_6H_4(O.C_6H_{11}O_5).CH:CH.CHO$) hydrolysed with emulsin when *o*-cumaric aldehyde ($HO.C_6H_4.CH:CH.CHO$) was obtained. Pauly and Wäscher (*Ber.*, 1923, **56**, [B], 603) have introduced a more satisfactory method. The hydroxy-benzaldehyde is converted into the methoxy-methoxy-compound by the action of chloromethyl ether, this is condensed with acetaldehyde in the usual way, and the methoxy-methoxy-group removed by boiling for a short time with 50 per cent. acetic acid containing 0·3 per cent. of sulphuric acid.

$$NaO.C_6H_4.CHO \xrightarrow{CH_3O.CH_2Cl} CH_3O.CH_2O.C_6H_4.$$
$$CHO \xrightarrow{CH_3.CHO} CH_3O.CH_2O.C_6H_4.CH:CH.CHO$$
$$\rightarrow HO.C_6H_4.CH:CH.CHO.$$

The production of methyl-ketones of the phenolic ethers by the action of acetic anhydride containing concentrated sulphuric acid (sulphoacetic acid) has been studied by Schneider and his co-workers (*Ber.*, 1921, **54**, 1484, 2298, 2302; 1922, **55**, 1892). Anisole gives *p*-methoxyacetophenone, *α*- and *β*-naphthyl-methyl ethers give 4-methoxy-*α*-naphthyl-methyl ketone and 2-methoxy-*α*-naphthyl-methyl ketone respectively, while guaiacol, $MeO.C_6H_4.OH$, gives 3-acetoxy-4-methoxy-acetophenone or acetyl-iso-acetovanillone, $MeO(AcO).C_6H_3.CO.CH_3$. More prolonged action of the reagent gives rise in a number of cases to pyrylium compounds which can be isolated as perchlorates.

Dibromomethylal, $CBr_2(OMe)_2$, obtained, together with methyl bromide and other products, by the action of bromine on methylal cooled in a freezing mixture, has been found to be an energetic methylating agent (Feist, *Z. angew. Chem.*, 1922, **35**, 489). Aniline in ethereal solution is converted by means of it into methylaniline hydrobromide, *β*-naphthol into

β-naphthyl-methyl ether, sodium cinnamate into methyl cinnamate and ethyl sodiomalonate and ethyl sodioacetoacetate in light petroleum suspension into the corresponding methyl derivatives.

Vinyl chloride, $CH_2 : CHCl$, can be prepared in almost quantitative yield by the action of concentrated hydrochloric acid at 60°–95° on calcium carbide in the presence of a mixture of a mercury and a copper salt, a stream of hydrogen chloride being passed through the reaction mixture during the operation (Brit. Pat. 156120).

A new method of ethylation which promises to be of great use has been discovered by Gilman and Hoyle (*Jour. Amer. Chem. Soc.*, 1922, **44**, 2621). They find that organomagnesium haloids in which the group MgX is attached to carbon, oxygen, or nitrogen when acted upon by diethyl sulphate have the MgX group replaced by ethyl. The Grignard reagents prepared from, for example, bromobenzene, *p*-bromotoluene, benzyl chloride, *a*-bromonaphthalene, and bromocyclohexane gave yields of 33 per cent. ethyl benzene, 45 per cent. *p*-ethyl toluene, 100 per cent. *n*-propyl benzene, 70 per cent. *a*-ethyl naphthalene, and 80 per cent. of ethyl cyclohexane respectively.

Davies (*Trans. Chem. Soc.*, 1922, **121**, 715) has devised a method by which an 80 per cent. yield of pure *p*-nitro-phenylhydrazine may be readily obtained from *p*-nitraniline. The method consists in the reduction of *p*-nitrobenzene diazonium chloride with ammonium sulphite, the essential feature being that the reaction mixture must be kept neutral or alkaline throughout by the addition of ammonia, otherwise resinous products are formed in quantity. A satisfactory method for the preparation of this useful reagent for aldehydes and ketones has long been wanted.

A nitrating agent which can be used in a basic medium has been employed by Battegay and Brandt (*Bull. Soc. chim.*, 1922, [iv], **31**, 910). This consists of anhydrous pyridinium nitrate in excess of pyridine, and it is found that anthracene and naphthalene with this mixture give 9-nitroanthracene and *a*-nitronaphthalene.

Gilman, Meyers, and their collaborators (*Jour. Ind. Eng. Chem.*, 1923, **15**, 61; *Jour. Amer. Chem. Soc.*, 1923, **45**, 150) have done some very necessary work on the Grignard reagent. Though this reagent has such an extended application, but little is known about the best experimental conditions for its preparation with optimum yield. Studying first the analytical methods for the estimation of the Grignard reagent, it has been found that the measurement of the gas evolved on decomposition with water or the titration of the residual basic magnesium halide with standard acid after decomposition of the reagent

magmas; on the surface very complete chemical segregation is brought about by the processes of weathering and sedimentation. On the other hand, the processes of metamorphism and metasomatism tend to bring about a certain uniformity, which is evidenced by the monotonous chemical character of ancient metamorphic formations over wide areas.

In his Presidential Address to the Geological Society of America (*Bull. Geol. Soc. Amer.*, **33**, 1922, pp. 231–54) Prof. J. F. Kemp discusses the after-effects of igneous intrusion, especially from the viewpoint of the origin of mineral veins. He reviews the phenomena of pre-Cambrian impregnated strata, *lit-par-lit* injection, contact-zones, and mineral veins, and gives powerful support to the theory of derivation of most ore-deposits from igneous rocks.

Dr. R. H. Rastall's paper on "Metallogenetic Zones" (*Econ. Geol.*, **18**, 1923, pp. 105–21) follows out some of the lines of thought implicit in Prof. Kemp's address. He shows that in the principal ore-deposits of the British Isles there is evidence for the existence of a definite arrangement in space of metallic compounds in relation to igneous intrusions; and that it is closely correlated with the order in time of the different minerals, both ore and gangue, the later minerals being the more distant from the parent igneous rock. The metallogenetic sequence in Britain, summarised as tin, tungsten, copper, zinc, lead, silver, and iron, is, on the whole, similar to that accepted in America by recent workers on the subject.

R. J. Colony has also pointed out the powerful effects produced by the adjustments of equilibrium at the end-stage of the consolidation of igneous rocks, between the highly concentrated mother-liquor with mineralisers, and the almost wholly crystallised rock (*Journ. Geol.*, **31**, 1923, pp. 169–78). In these effects he includes the soaking of earlier minerals with quartz and albite, the transformation of pyroxenes into fibrous amphiboles, and the formation of micropegmatite and serpentine.

In numerous recent publications P. Niggli, and his colleagues and students at Zürich, have taken up the problems of the geographical distribution of rocks and minerals. Rocks of all kinds are viewed as aggregates of minerals which are the products of definite physical and chemical processes, conditioned chiefly by temperature, pressure, and initial chemical composition. These processes are geological, since both the energy and material involved are of natural origin; and they find expression in the geological arrangement and association of the minerals and mineral aggregates (rocks) produced.

The rocks are relatively invariable mineral associations which form large units, and take an essential part in the

structure of the earth's crust. So relatively uniform a mineral association as a rock is evidence of considerable uniformity of conditions over a large area, and all mineral associations of the same age in such an area will show consanguineous relations. Hence Niggli recognises not only igneous provinces (with which vein minerals and many ore deposits are associated), but also sedimentary and metamorphic rock provinces (" Der Gesteins-association und ihre Enstehung," *Verh. d. Schweiz. Naturf. Gesell. Neuenburg*, 1920, pp. 1–25). A review of the whole field of research from this viewpoint is given in Niggli's book, *Lehrbuch der Mineralogie*, 1920, Chap. IIIB, " Die Mineral-lagerstätten und ihre Enstehung," pp. 467–637. Its fertility in ideas and thoroughness of execution can scarcely be over-estimated.

Turning now to the igneous rocks in particular, with which Niggli is now dealing in a work on the grand scale [*Gesteins und Mineralprovinzen*, Band I, *Einführung; Zielsetzung; Chemismus der Eruptivgesteine* (von. P. Niggli); *Der Chemismus der Lamprophyre* (von. P. J. Beger): Berlin, 1923, pp. 602], he recognises that in order to study their geographical distribution some systematic major grouping of the rocks is required, also some means of exhibiting the chemical character of individual rocks and rock series. Hence he has invented a new way of calculating the chemical analysis of a rock, on lines somewhat similar to those of Osann and F. Becke. It is claimed that by this method, which is too long to be explained here, the chemical relations of rocks and rock series are displayed better than by any other method (a claim which the writer of these notes is disposed to question). Niggli produces with the aid of the units thus obtained a diagrammatic representation of igneous rock assemblages very similar to the ordinary silica variation diagram.

In addition to the usual calc-alkali or Pacific, and the sodic or Atlantic kindreds, Niggli also recognises a relatively potassic or Mediterranean kindred, which contains the leucite-rich types, and the orthoclasic rocks such as potassic granites, syenites, shonkinites, and monzonites. Each of the three main groups is divided into about twenty *magmas*. The methods of calculation and the classification referred to are set out in full detail in the Introduction to Niggli's new book above cited.

The views and methods above stated have been applied to the special cases of the petrographic provinces of Switzerland ("Heim Festschrift," *Vierteljahr. d. Naturf. Gesell. Zürich*, 1919, 64, pp. 179–212), and to the igneous fields of the young Mediterranean mountain chains (" Der Taveyannazsandstein und die Eruptivgesteine der jungmediterranen Kettengebirge," *Schweiz.*

Min. u. Petr. Mitth., Bd. 2, Heft 3–4, 1922, pp. 169–275). The latter investigation shows that in the building of the Alpine orogen, magmas covering practically the whole range of the igneous field have been produced. Thus a recent great orogenic belt presents the same aspects of magmatic differentiation in miniature as the crust of the earth does as a whole. This shows that the igneous field is chemically limited, conditioned by law, and has suffered no restriction of range since the earliest times.

In detail the investigation shows that along and within the orogenic belts, in the pre-folding stages, calc-alkali and potassic provinces are predominant; but in the inner and outer subsidence regions sodic provinces are more frequent. The conclusion is reached that the kind of magmatic differentiation in any region is a function of the general tectonic conditions with which it is in connection.

Several petrological investigations on the lines laid down by Niggli have been carried out by his colleagues and students, amongst which may be mentioned the following: H. Schuppli, "Petr. Unters. im Geb. des Pic Languard (Oberengadine)," *Inaug. Diss., Univ. Zürich*, 1921, pp. 116; A. Sonder, "Unters. u. d. Differentiationsverlauf d. Spätpaläozoischen Granitintrusionen im zentralen u. westlichen Gotthardmassiv," *Inaug. Diss., Univ. Zürich*, 1921, pp. 72. By a consideration of the lamprophyres satellitic to the plutonic masses of Lausitz and the Odenwald, P. J. Beger ("Die Bildung des lamprophyrischen Restmagmas in Lausitz und Odenwald als Prüfstein fur die Bowen'sche Differentiationhypothese," *N. J. f. Min.*, 1923, Bd. 1, Heft 2, pp. 220–6) is able to show that Bowen's hypothesis of differentiation by the separation of early crystals from the residual melt, with the addition of remelting of the sunken crystals in depth, fully explains the origin of these lamprophyres. The general difference in composition between the Lausitz and Odenwald lamprophyres is also explained with attention to the different geological conditions under which the intrusion of the massifs took place. Beger extends this theme, together with the consideration of all types of lamprophyre and their differentiation, in the latter half of Niggli's book ("Der Chemismus der Lamprophyre," previous reference, pp. 217–574).

Prof. V. M. Goldschmidt has made a notable study of some of the more common igneous rock kindreds in relation to the definite tectonic-geological milieus with which they are connected (Stammestypen der Eruptivgesteine," *Vidensk.-selsk. Skr. I. Math.-Nat. Kl., Kristiania*, 1922, No. 10, pp. 12). He instances the "normal" gabbro-diorite-granite kindred established by Bowen, and the "mica-diorite" or tonalite kindred of the Caledonian chains, the Alps, and the Andes. The early

crystallisation of biotite in the latter is ascribed to the "wetness" of the magma. The abundant water-content may be due to the absorption of volatile matter derived from the argillaceous sediments of the geosynclinals into which the magmas are intruded, and with whose folding they are connected. On the other hand, the anorthosite-charnockite kindred may originate from a "dry" magma, and is generally found intruded into water-poor granites and acid gneisses.

The summary and review by Hibsch of the Tertiary alkaline province of the Bohemian Mittelgebirge is of extreme importance to all students of the geographical distribution of igneous rocks ("Stoff und Masse der tertiären Eruptivgebilde des Böhmischen Mittelgebirge," *Tscherm. Min. u. Petr. Mitth.*, Bd. 35, Heft 3–4, 1921, pp. 89–110). It is probably the most detailed estimate yet published of the relative and absolute masses of the rocks comprising a limited igneous field. The Bohemian region is usually taken as one of the most typical of alkaline or Atlantic provinces. The properly weighted mean of all the analyses shows that the average rock is an alkaline basalt with SiO_2, 44·10; Na_2O, 3·80; K_2O, 2·04, in spite of the prominence given to trachytes, phonolites, essexites, etc., in the descriptions of this field. This study yields powerful support to the view that magma of basaltic composition is the starting-point for many differentiated series of igneous rocks.

F. F. Grout has plotted the alkalies against the excess or defect of silica in numerous igneous rock series (*Bull. Geol. Soc. Amer.*, **33**, 1922, pp. 617–38). He thereby shows that there are many diverse differentiation series, and that no particular initial composition is necessary for differentiation. Further, the trend of differentiation may be diverse in different localities, even if the initial magmas be of similar composition. The diagrams show that "primary magma" approaches average basalt in composition, but may vary rather widely.

By a statistical method based on Washington's tables, W. A. Richardson has studied the frequency-distribution of igneous rocks in relation to their chemical composition (*Min. Mag.*, **20**, 1923, pp. 1–19). He finds that all visible igneous rocks are normally distributed about two types, basaltic and granitic. Divergences from the normal distribution are slight, and are due in part to errors in sampling, in part to magmatic mixing. He concludes that neither Daly's nor Bowen's petrogenic theories account for the origin of the two primary magmas, but that the normal suites with their characteristic dispersions can be produced by crystallisation-differentiation as defined by Bowen.

In a paper on "Differentiationsvorgange in Natronmagmen" E. Lehmann (*N. J. f. Min.*, 1923, Bd. 1, Heft 2, pp. 226–32)

exhibits the different mineral development of certain East African trachydolerites and tephrites, although the rocks are approximately of the same chemical composition. He shows that the mineral combination of trachydolerite has a much smaller molecular volume than tephrite, and concludes that the formation of trachydolerite from a magma of suitable chemical composition is favoured by high pressure and retention of gaseous constituents, whereas the formation of tephrite requires lower pressures and escape of the volatile constituents.

In discussing the nomenclature of the spilitic suite A. K. Wells (*Geol. Mag.*, **59**, 1922, pp. 346–54) suggests that the term *keratophyre* should be applied to intermediate members of the suite, both intrusive and extrusive, characterised by a high percentage of albitic felspar accompanied by a small amount of dark minerals now represented by chloritic or serpentinous pseudomorphs. Some keratophyres closely resemble bostonites in mineral composition and texture; but the latter term should only be used for rocks of normal alkaline lineage, restricting keratophyre to the equivalent members of the spilitic suite. In a second paper Mr. Wells summarises our knowledge of the characters and origin of the spilites (*Geol. Mag.*, **60**, 1923, pp. 62–74). He adduces reasons for believing that these rocks are not merely altered basalts, but are the products of basic magmas initially rich in soda.

A re-examination of the Traprain Law phonolite (East Lothian) by A. G. Macgregor and F. R. Ennos has resulted in the discovery of olivine and sodalite in this rock (*Geol. Mag.*, **59**, 1922, pp. 514–23). These minerals, along with the nepheline and analcite present, were subjected to ingenious microchemical tests, and a new complete chemical analysis was performed by Mr. Ennos. Calculation reveals approximately 4·2 per cent. nepheline, 20·5 per cent. analcite, 2·7 per cent. sodalite, and 1·8 per cent. olivine.

The analcite-bearing igneous rocks of Scotland have claimed renewed attention. D. Balsillie has begun the detailed petrographic study of the numerous doleritic intrusions of Fifeshire (*Geol. Mag.*, **59**, 1922, pp. 442–52). These rocks belong to two groups: quartz-dolerites and analcite-bearing dolerites. Amongst the latter olivine-dolerites (? crinanites) and teschenites are distinguished.

Dr. F. Walker (*Geol. Mag.*, **60**, 1923, pp. 242–9) classifies the Scottish and Moravian teschenites into (*a*) porphyritic or basaltic types; (*b*) ophitic or doleritic types; (*c*) non-porphyritic or gabbro types. Each of these groups is further subdivided into varieties with hornblende (barkevikite) and varieties devoid of this mineral. There is a close resemblance between

the Scottish and Moravian types, and the highly analcitic rock *lugarite* is represented in the latter province.

Dr. Walker has also described the igneous geology of the Dalmeny district (*Trans. Roy. Soc. Edin.*, **53**, pt. ii, 1923, pp. 361–75). The outstanding igneous rocks here are the Mons Hill teschenite, the Hound Point quartz-dolerite, and the typical Dalmeny basalt lava of the Lower Carboniferous. A theralitic modification of the Mons Hill teschenite at Whitehouse Point is described.

In a paper on "The Classification and Age of the Analcite-bearing Igneous Rocks of Scotland" (*Geol. Mag.*, **60**, 1923, pp. 249–60) the writer divides these rocks into two tolerably distinct petrographic groups, called respectively the *teschenite* and the *crinanite* series. The rocks of the teschenite series are richer in analcite than the crinanites, whilst orthoclase and olivine are relatively more abundant in the latter. A subsidiary difference is the greater abundance of barkevikite and apatite in the teschenitic rocks. The teschenites are wholly Late Palæozoic in age; the crinanites are partly of that age and partly Cainozoic. The intrusion of analcite-bearing rocks thus appears to have ended the Palæozoic cycle of eruption in the west of Scotland, and to have begun the Cainozoic cycle. The Cainozoic crinanites may be regarded as derived from *relic* Palæozoic magmas.

N. Magnusson has made a close petrographic study, with several new chemical analyses, of the typical *särnäites* (cancrinite-bearing nepheline-syenites) and cancrinite-tinguaites of Särnä, Sweden (*Geol. För. Stockholm Förh.*, **45**, Heft 3–4, 1923, pp. 295–334). The occurrence belongs to the highly alkaline group of plug-like intrusions of probable pre-Cambrian age which have been described by Brögger (see Science Progress, **16**, 1922, p. 549).

N. H. Kolderup has made an intensive study of a small region of Caledonian folding and eruptives in western Norway ("Der Mangeritsyenit und umgebende Gesteine zwischen Dalsfjord und Stavfjord in Søndfjord im westlichen Norwegen," *Bergens Mus. Aarbok*, 1920–1, *Naturvid. Raekke*, No. 5, pp. 71). Mangerite-syenite is a syenitic member of the lime-rich microperthite-bearing monzonites which C. F. Kolderup first described under the name of mangerite. There are also quartz-bearing members of the series, and a uralite-gabbro. This assemblage is intrusive into a Cambro-Silurian series of quartzites and phyllites, with greenstones.

Another series of nepheline-syenite and phonolite occurrences in the Transvaal is described by Prof. S. J. Shand, with several new chemical analyses ("The Alkaline Rocks of the Franspoort Line, Pretoria District," *Trans. Geol. Soc. S.*

Africa, **25**, 1922, pp. 81–100). They appear as four plugs and numerous dykes along a belt of country 23 miles in length, which is believed to be a line of weakness in the Bushveldt igneous complex and its floor.

According to F. Dixey (*Quart. Journ. Geol. Soc.*, **78**, pt. iv, 1922, pp. 299–347) the great norite mass of Sierra Leone differs from other well-known norite intrusions in its great size and apparent batholithic character. The main intrusion of norite was invaded in succession by a series of related minor intrusions, coarse norites, norite-pegmatites, beerbachites, norite-aplite, and dolerite. The rocks exhibit good flow-banding, and interesting binary and ternary intergrowths between their principal minerals.

The famous Beemersville (N.J.) occurrence of nepheline-syenite has recently been investigated by Aurrousseau and Washington (*Journ. Geol.*, **30**, 1922, pp. 571–86). The mass is interpreted as a lenticular sill or flat laccolith of foyaite intruded by a dyke-like mass of nepheline-porphyry. The latter rock was formerly described as *sussexite* (a nepheline-rich end-member of the grorudite-tinguaite series of Brögger), but the authors show that the Beemersville rock does not belong to this type.

BOTANY. By E. J. SALISBURY, D.Sc., F.L.S., University College, London.

Genetics, etc.—In a paper on Linkage in the Sweet Pea, Punnett describes experiments designed to test whether the number of characters or groups of characters showing independent inheritance is the same as the haploid number of chromosomes as was found by Morgan and his collaborators to obtain in *Drosophila*. This haploid number in *Lathyrus odoratus* is seven, whilst the apparent number of linkage groups would seem to be eight; since, however, there is probably a low-grade linkage of about 50 per cent. between some of these the number of linkage groups is probably equal to the haploid number of chromosomes (*Jour. of Genetics*, March 23).

In the same journal Anderson describes prothalli raised from spores of variegated *Adiantum cuneatum*, all of which are variegated but are of two types. One is dark green at first and eventually variegated, the other is at first pale green with small chloroplasts and ultimately becomes white; the latter are always more numerous, whether raised from the spores from the plant as a whole or from those in a single sporangium. All the sporophytes are also variegated, or white, and the sporangia which they bear do not develop properly when arising from white tissue, and this is even true of part of the sorus situated on white tissue at the junction with green tissue.

Gates finds that when large-petalled *Œnothera rubricalyx* is crossed with small-petalled *O. biennis* the F_1 individuals show a more or less intermediate size of petal; but, whilst germinal segregation occurs in F_2, somatic segregation would also appear to take place since a wide range in size of petal is found on the same individual and even on the same flower.

In *Genetics* for November Sax gives an account of the chromosomes of Wheat. The haploid number is seven for *T. monococcum*; 14 for *T. dicoccum*, *T. durum*, *T. polonicum*, and *T. turgidum*; and 21 for *T. vulgare*, *T. compactum*, and *T. spelta*. Hybrids between the different groups thus often have an irregular number of chromosomes in the haploid nuclei and gametic incompatibility associated with this may account for the sterility of many such crosses; there appears to be a high degree of correlation between chromosome number and the adaptability of the wheat variety.

Cryptogamic.—An important paper by Brooks and Hansford on the moulds on cold storage meat appears in the *Trans. Brit. Mycological Soc.* for March. Three new fungal species are described from this habitat, *viz.* *Sporotrichium carnis*, which occurs on meat, *Torula botryoides*, which is found on fish, and *Wardomyces anomala* on rabbits' flesh. The last-named is the type of a new genus. Several so-called species of *Cladosporium* are interpreted as strains of the well-known *C. herbarum*. All the fungi concerned are superficial in their development and do not render the flesh unfit for food unless associated with putrefactive bacteria.

In the same journal Mehta discusses the annual outbreak of Wheat Rust. This investigator finds that the uredospores cannot survive the winter, nor can the fungus survive in the mycelial condition. Direct infection by soredia is not possible, and there is no evidence for mycoplasm. In the Cambridge district fresh infection by æcidiospores from *Rhamnus* would appear to be essential. Uredospores of *P. triticina* and *P. glumarum*, on the other hand, survive the cold of winter, whilst the former agrees with *P. graminis* in being resistant to the heat of summer.

Gymnosporangium bermudianum is peculiar in that both the æcidiospores and teleutospores occur not only on the same host but on the same gall. In *Puccinia podophylli*, where a similar condition obtains, the two types of mycelia both apparently perennate in the tissues of the host. Thurston brings forward evidence to show that in *G. bermudianum* there is a perennial mycelium producing teleutospores which on germinating cause a reinfection with the production of a mycelium bearing æcidiospores (*Bot. Gaz.*, vol. lxxv, No. 3).

Pylaiella litoralis, a member of the Ectocarpaceæ, has been

the subject of investigation by Miss Knight (*Trans. Roy. Soc. Edinb.*, 28, p. 343), who finds that one and the same individual at various times assumes the habits to which varietal names have been given. This polymorphism is associated with the periodic formation of sporangia at intervals of 2–3 weeks followed by a process of natural pruning, when the intercalary sporangia become empty, and a subsequent proliferation. When growing as an epiphyte on *Fucus* this species is more vigorous than when attached to *Ascophyllum*, and on the latter pleurilocular sporangia are most frequently produced whilst when epiphytic on *Fucus* unilocular sporangia are the rule. Both types of sporangia may, however, occur on the same plant. In the unilocular sporangium each cell exhibits a reduction division followed by further divisions which result in the formation of a number of zoospores. These zoospores can give rise to new plants. In the pleurilocular sporangia the divisions are normal, and in each constituent cell a spore is formed which may either behave as a gamete or develop parthenogetically. The plants which bear pleurilocular sporangia are of two kinds, however; some are haploid and the spores which they form either fuse and give rise to a diploid plant or, if parthenogenetic, to another haploid plant. Other plants bearing pleurilocular sporangia are diploid and the spores which they form develop into other diploid plants without fusion. The plants which bear unilocular sporangia are diploid.

Ecology.—An account of the interglacial forests of Glacier Bay, Alaska, is given by G. W. S. Cooper in *Ecology* for April. The last glacial advance is estimated to have culminated about 150–200 years ago, but prior to this the shores of the bay were covered by forest from the water's edge up to 2,500 ft. The dominant species were those of S.E. Alaska at the present day, *viz. Picea sitchensis* and *Tsuga* spp. and thirteen species of musci from the peat have been identified which are all common in the existing forests. This young climax forest is in some parts in direct continuity with ancient climax forest at a higher altitude which was not destroyed during the latest glacial advance. This older forest, though similar in character, is distinguished by the absence of cottonwood and alder. It is of interest to note that locally more than one buried forest level occurs attributed to deflection of the streams responsible for the gravel deposits under which the forest remains are preserved. The various localities investigated show differing proportions of *Picea* and *Tsuga* indicative of a succession in which Tsuga predominates in the final phase.

The distribution of plants is the subject of a paper by Ridley in the *Annals of Botany* for January in which he combats

the idea that extended area is a necessary concomitant of age within that area. *Galinsoga parviflora*, long present in this country, is still confined to a small area around its original habitat, whilst *Matricaria discoidea*, though a relatively recent introduction, is already very widely spread. The species which exhibit a wide area of dispersal are held to be referable to one of four categories, *viz.* they are either weeds dispersed by human agency, species dispersed by sea-currents, swamp plants dependent on bird agency, or, in the case of a few species, have a wide temperature range (*e.g. Phragmites communis, Cynodon dactylon*) or are widely dispersed on mountain chains in the Tropics (*e.g. Sanicula europæa*). There is not, the author suggests, any evidence for regarding these widespread species as sharing a common antiquity, as is required by the age and area hypothesis.

An important ecological paper has been published as a serial in the *Revue générale de Botanique* by Pierre Allorge on that part of the Paris basin between the Oise, the Seine, and the Epte, known as the Vexin Française. The vegetation, apart from the considerable area of cultivation, consists in the main of *Quercus sessiliflora* coppices; heaths, sometimes planted with pines, and siliceous and calcareous grasslands of a xerophytic type. Included in the flora of the area are several southern species which are restricted, however, to the calcareous slopes (*e.g. Coronilla minima, Astragalus monopessulanus, Fumana procumbens*). The biological types presented are classified according to Raunkaier's system, but the author draws attention to the intermediate character of many species. The buds of *Euphorbia amygdaloides*, for example, are produced in part above the ground-level and in part below.

Of the native species over half are hemicryptophytes and 25 per cent. are cryptophytes, but whilst only 9 per cent. of the native flora are annuals, such comprise nearly 70 per cent. of the naturalised species.

The association of *Scirpus fluitans* and *Potamogeton polygonifolius* found in siliceous marshes is the first stage in the passage to *Erica tetralix* heath and *Quercetum sessilifloræ* with *Vaccinium myrtillus*; a sequence which we may note is also encountered in this country. A second succession series is characterised by the successive dominance of *Limnanthemum*, *Scirpus lacustris*, and *Bidens* leading up to Alnetum with *Cardamine impatiens* and finally Oakwood dominated by *Q. pedunculata*.

In waters rich in mineral salts the association of *Schœnus nigricans* presents a composition strikingly similar to that of the East Anglian fens. The Schœnus association gives place to a drier community dominated by *Molinia*, and this in turn

to thickets of Alder, Willow, and Buckthorn. Two meadow types are recognised, one being characterised by *Festuca arundinacea* and *Silaus pratensis*, the other by *Arrenatherum*. The calcareous pastures are dominated, as in this country, by *Festuca duriuscula* and *Sesleria cærulea*, whilst the siliceous grasslands are characterised by *Corynephorus canescens*, *Koeleria gracilis*, and many of the species particularly met with in this country in the Breckland area.

The woodlands comprise Alneta, on inundated soils, Beechwood on calcareous slopes, with a similar flora to those of Southern England. Oak-ash coppice on chalky and marly slopes with both *Q. pedunculata* and *Q. sessiliflora*; the latter, however, only becomes common on the drier and poorer soils. Dry types of wood with both *Q. pubescens* and *Q. sessiliflora* occur, the former being absent on the siliceous soils.

Taxonomy.—T. and T. A. Stephenson in the *Journal of Botany* for March give an account of *Orchis prætermissa.* Amongst other features on which this is held to differ from *O. incarnata* are the length and breadth of the lip, which are nearly equal in the latter, but in the former the length is appreciably the greater; moreover, the markings are fine dots or streaks in place of the definite line-pattern of *O. incarnata.* In *O. latifolia* the leaves are spotted or ringed and the lip-pattern consists of continuous lines.

In the same journal Mr. Salmon adduces evidence that *Gentiana suecica* Froelich occurs in North Britain.

During the past year several papers of interest to systematists have appeared in the *Annals of the Missouri Botanic Garden*, notably the monograph on the Isoetaceæ by Pfeiffer in which 64 species are enumerated, of which two are new, namely, *I. lithophila* from Texas and *I. ovata* from British Guiana. Although the genus as a whole is a cosmopolitan one, the area of individual species is generally very restricted, only a few like *Isoetes Braunii*, in North America, exhibiting a wide range.

In the *Rep. Bot. Soc.* and Exchange Club, Dahlstedt describes nine British Dandelions to which he gives specific rank.

ZOOLOGY. By Reginald James Ludford, Ph.D., B.Sc., University College, London.

Protozoology.—*Rhinosporidium seeberi* is found in certain polypoid growths of man, in which it usually occurs between the connective-tissue cells. Up to the present it has been regarded as having affinities with the Sporozoa; however, Prof. J. H. Ashworth has made a special investigation of its method of sporulation, as the result of which he has come to the conclusion that its systematic position is with the Fungi. He

places it in the Phycomycetes, with the sub-order Chytridineæ. ["On *Rhinosporidium seeberi* (Wernicke, 1903), with Special Reference to its Sporulation and Affinities," *Trans. Roy. Soc. Edin.*, vol. liii, Part II, No. 16, 1923.]

The division of the nucleus of *Amœba proteus* has been investigated by M. Taylor. Although a search was made through "innumerable specimens at all times of the year, and at all hours of the day and night, in many cultures of amœba, some of them so luxuriant that the bottom of the glass trough appeared whitish because of the enormous numbers of amœbæ lying on it," the writer failed to find one single example of a mitotic figure. Hence it would seem that the published figures of mitosis in *A. proteus* belong to the sporulation cycle of the life-history. The non-mitotic type of nuclear division which was found to occur involves division of the clumps of chromatin which occur just beneath the nuclear membrane, and division of the centrally disposed karyosome. The two parts of the divided karyosome separate and become surrounded by chromatin granules which constitute the daughter nuclei. The process is apparently initiated by the division of the chromatin granules which lie just beneath the nuclear membrane.

Experiments carried out by B. Sokoloff on the regeneration of starved ciliates have led him to conclude that "regeneration power is something entirely different from energy of growth. The former has been observed even in infusoria which have suffered considerably from hunger; and when hunger has not too greatly weakened these infusoria, the regenerative capacity (up to its limit) is not lower, but on the contrary, greater. Specimens very much affected by hunger, having entered a state of depression, have a regenerative ability with a tendency to be realised, though they are unable to overcome the destructive power of depression" (*Jour. Royal Micr. Soc.*, 1923, Part II).

Cytology.—The effects of carbon dioxide on the consistency of protoplasm have been investigated by M. H. Jacobs (*Biol. Bull.*, vol. xlii, No. 1). It was found that a short exposure of various cells to this gas causes a decrease in the viscosity of the protoplasm, while a longer exposure results in an increase. The writer suggests that carbon dioxide may be an important factor in producing many of the natural changes in protoplasmic consistency which have hitherto been unexplained.

T. S. Painter has made a study of the spermatogenesis of man. The material used consisted of the freshly preserved testes of two negroes and one white man. In each case forty-eight chromosomes were observed in the spermatogonia. Two of these chromosomes are sex chromosomes (X and Y components

of the X-Y sex-chromosome complex). The X and Y chromosomes separate during the first maturation division and travel towards opposite poles of the spindle. It was impossible to make decisive chromosome counts during the second maturation division, but the writer infers that the X and the Y components divide equationally at this time, and as the result half of the sperms will carry an X and half will carry a Y chromosome.

In view of the controversy that has waged as to the occurrence of parasynapsis or telosynapsis during the prophase of the heterotypic division of gametogenesis, it is of interest to note that H. B. Yocom, who has studied synapsis in the male germ cells of an hemipteran, *Leptocoris*, finds that telosynapsis occurs in this insect. ("The Occurrence of Telosynapsis in the Male Germ Cells of an Hemipteran, *Leptocoris trivitattus*," *Jour. of Morph.*, vol. xxxvii, No. 2.)

The important work in genetics which has been carried out with the fruit-fly, *Drosophila*, gives special interest to a paper by A. F. Huettner on "The Origin of the Germ Cells in *Drosophila melanogaster*" (*Jour. of Morph.*, vol. xxxvii, No. 2). The "germ-cell determinants" in the egg of this insect seem to appear *de novo* in the posterior polar plasm of the late oöcyte and also in the blastula at the time of differentiation of the polar cells. The writer assumes that they are by-products of the posterior polar plasm, which have nothing to do with the causal differentiation of the germ cells. The deciding factor which determines whether a nucleus shall become somatic or germinal appears to be the posterior polar plasm, which is to be regarded as a differentiated oöplasm. "Any nucleus of the developing egg may be differentiated into a polar nucleus if it comes accidentally into the region of the polar plasm. If one daughter nucleus enters the posterior polar plasm, and the other remains in the general oöplasm, the latter becomes a somatic nucleus, while the former, surrounded by polar granules, becomes a polar nucleus."

Embryology and Histology.—As the result of the examination of a large number of frog embryos, Prof. A. Meek finds that in all cases the "segmentation cavity" is converted, by fusion with the secondary or neurenteric enteron, into the forward part of the enteron. These observations "demonstrate that the frog and its allies come into line with the meroblastic Amphibia and with the rest of the terrestrial Vertebrates" (*Q.J.M.S.*, vol. lxvii, Part I).

"The Action of Repeated Doses of X-rays upon the Developing Chick Embryo" is the subject of a joint paper by H. A. Colwell, R. J. Gladstone, and C. P. G. Wakeley (*Jour. of Anat.*, vol. lvii, Part I). It was found, in chick embryos

which were irradiated immediately before incubation and then subjected to daily irradiations for several days, that growth was retarded, the irradiated embryos being invariably smaller than the controls. Within the limit of radiation investigated, the effects seemed rather to depend upon the total amount of radiation reaching the embryo than upon its quality or "hardness." Investigation of the susceptibility of individual tissues and structures showed that the surface ectoderm, the central nervous system, and the eye were most affected. Other experiments which were carried out yielded results which indicate that exposure of embryos which have been allowed to incubate normally for a period of ninety hours and which have been exposed to X-rays for three successive days produces in some cases a certain degree of stimulation, as indicated by a slight increase in size.

M. F. Weymann has investigated by histological methods the beginning and development of function in the suprarenal medulla of pig embryos. The earliest indication of the chromaffin reaction (the formation in the medullary cells of chromium oxide as the result of the reduction of potassium bichromate by adrenalin) that could be detected was found in embryos of about 40 mm. At this stage of development the principal organs and systems of the embryo are well established, so that "it does not seem probable that any primary malformations can be traced to disturbances in the epinephrin production of the embryo itself." ("The Beginning and Development of Function in the Suprarenal Medulla of Pig Embryos," *Anat. Rec.*, vol. xxiv, No. 5.)

The discovery by Prof. J. P. Hill of a true allantoic placenta in *Perameles* was of the greatest importance in regard to the phylogeny of the marsupial group and its relationship to the remainder of the Mammalia. In a paper on "The Yolk-sac and Allantoic Placenta in *Perameles*" (*Q.J.M.S.*, vol. lxvii, Part II), T. T. Flynn confirms and extends Hill's observations in general, although differing on minor points. Placentation in the Marsupialia is considered to represent a degeneration from a more complex condition which existed in the original protoplacental group. A comparison is drawn between the early stage of the allantoplacenta in *Perameles* and that of the Carnivora, and it is pointed out that the cumulative type of placenta is evidently the more primitive.

An important contribution to our knowledge of the pituitary body is a paper by F. J. Wyeth and the late R. W. H. Row on "The Structure and Development of the Pituitary Body in *Sphenodon*" (*Acta Zool.*, Häft 1, Arg. 4). For this work the investigators had access to Prof. Dendy's collection of *Sphenodon* embryos. They have been able to correct certain

inaccuracies in earlier accounts, and a number of important points, previously doubtful or unknown, have been determined.

From experiments conducted on the effect of X-rays of different wave-lengths on the tissues of the rat, S. Russ finds that about six times as much short wave-length energy as long wave-length energy must be expended in a layer of skin in order to produce equal reaction and that this factor falls to about 2·6 in the case of tumour growths (*Proc. R.S.*, vol. xcv, No. B 665).

H. B. Goodrich and J. A. Scott have carried out experiments to determine whether light has an influence on the growth of tissues cultivated *in vitro*. They found that polychromatic light from an incandescent electric bulb of not over 270 foot-candles has no deleterious effect in the initial growth of cultures of the heart of the chick embryo. "Thus the illumination used in incubators and in ordinary microscopic observations is harmless. ("The Effect of Light on Tissue Cultures," *Anat. Rec.*, vol. xxiv, No. 5, Dec. 1922.)

General and Experimental Zoology.—C. Lehmann has investigated the sense organs of certain medusæ ("Untersuchungen über die Sinnesorgane der Medusen," *Zool. Jahr.*, Bd. 39, Heft 3). He finds that the tentaculocysts are not statocysts. Medusæ react towards gravitation not by a reflex action, but as the result of a variation in specific weight of the subumbrella and manubrium on the inside, and of the gelatin of the exumbrella on the outside. The tentaculocysts serve the function of nervous sensitivity; they have great influence upon the frequency and strength of contractions. From a critical survey of the literature of the function of the sense organs of the *Ctenophores*, it seems that the tentaculocysts of the medusæ and the sense organs of the *Ctenophores* subserve approximately the same functions.

C. W. M. Poynter and A. Moritz have carried out investigations on the influence of ultra-violet light on pond snails. They find that snail embryos present a wide range of individual resistance to the action of ultra-violet light. Resistance increases with age. The effect of the rays upon the cells of the body is to produce coagulation of the cytoplasm, and this coagulation has the same appearance as that produced by heat. "It seems possible that this coagulation is secondary to a chemical change set up by the rays. Evidently the effects of the rays are cumulative in their action, which is directly proportional to the length of the time of exposure to them. No evidence was found to indicate that the rays act on live protoplasm to produce a heat sensitivity, but, on the contrary, embryos died as quickly in the refrigerator as in the

incubator." [" The Effects of Ultra-violet Light on Pond Snails (*Limnaeus*)," *Jour. Expt. Zool.*, vol. xxxvii, No. 1.]

A joint paper by A. Subba Rau and Prof. J. Bronté Gatenby on Bidder's organ deals with the distribution and structure of this organ in genera more or less allied to *Bufo*, some experimental work on the excision of the organ, and some cytological aspects of it. As the result of this work the writers conclude that in male toads, at least, Bidder's organ is to be regarded as a group of abnormal oöcytes. This is regarded as further evidence that the embryonic tendencies of the Amphibia towards a true hermaphroditism are persistent even in the adult, and that sex is very unstable in the Amphibia (*Jour. Roy. Micr. Soc.*, 1923, Part I).

" The Pigmentary Effector System " has been the subject of a series of papers contributed by L. T. Hogben and F. R. Winton to the *Proc. R.S.*, vols. xciii, xciv, xcv. As the result of their experimental work they put forward the hypothesis that the colour changes of the intact frog are due to fluctuating activity of the secreting mechanism of the posterior lobe of the pituitary gland in response to natural factors such as temperature, light, humidity, or to the rapidity with which the melanophore stimulant of the pituitary secretion is eliminated under the influence of similar agencies. " If this hypothesis is correct, the frog's appearance is an indicator of its own state of pituitary secretion."

In the first of a series of " Studies in Internal Secretion," L. T. Hogben describes the effect of pituitary (anterior lobe) injection upon normal and thyroidectomised axolotls (*Proc. R.S.*, vol. xciv, No. B 660, Jan. 1923). " While pituitary feeding was found to have no influence on the metamorphosis of medium-sized or sexually mature axolotls larvæ of *Amblystoma tigrinum*, injection of anterior lobe extracts into axolotls of the same ages and dimensions was followed by the assumption of the adult characteristics, with a rapidity comparable to metamorphosis induced by thyroid administration, and beginning about two to three weeks after the initial injection. Transplantation of the thyroids of a large sexually mature axolotl into a medium-sized individual was accomplished successfully without any production of metamorphic changes."

E. Uhlenhuth has compared the rate of growth resulting from the feeding of metamorphosed salamanders (*Amblystoma*) on beef liver, on anterior lobe of cattle hypophysis, and on earthworms. He finds that the liver diet causes a rate of growth higher than that produced by earthworms and as high as that produced by anterior lobe, but that anterior-lobe diet maintains growth for a longer period and produces a larger size than beef liver. " Animals fed on anterior lobe may reach a

size 25·4 per cent. in excess of the size of the largest known normal animal of this species, while the largest liver-fed animal exceeds the normal maximum size of the species by only 5 per cent." ("Further Facts regarding the Influence of Feeding the Anterior Lobe of Hypophysis on the Rate of Growth and the Size of *Amblystoma tigrinum*," *Jour. Expt. Zool.*, vol. xxxvii, No. 1.)

ARTICLES

THE INTERPRETATION OF ELECTRICAL STIMULATION IN TERMS OF CHANGES OF HYDROGEN-ION CONCENTRATION, AND THE PRODUCTION OF PERMEABILITY IN THE PLASMA MEMBRANE

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It is believed that a considerable part of the resistance which living tissues offer to the passage of an electric current is of the nature of an opposing potential, and that the study of bio-electrical phenomena has led physiologists to conceive of the plasma membrane as polarised even when unstimulated. While the nature of the membrane which is thus polarised remains still unknown, no theory having met with general acceptance, considerable advances have been made towards the solution of the problem of bio-electric phenomena, and it has been found possible, without the use of either metallic electrodes or strong acids and alkalies, to construct a number of systems which show potential differences comparable with those arising in living tissue. It is among these systems that the physiologist who seeks a mechanical model of the plasma membrane must make his choice, and the choice will be justified in so far as the system chosen is capable of exhibiting other characteristics of the living membrane of the cell. It is generally recognised that changes of permeability are the natural accompaniment of changes of polarisation, and any theory of the plasma membrane must therefore take account of the relation of these phenomena, which in their turn should be capable of furnishing an explanation of the phenomena of excitation and the transmission of stimulus.

It will be useful to review briefly from this standpoint the more recent theories which have been put forward to account

for the electrical polarisation of the living cell. They are of two classes :

1. Theories which derive bio-electrical phenomena from chemical action, and regard them as the result of oxidation processes.

2. Theories which derive bio-electrical phenomena from the polyphasic structure of the cell in virtue of which living tissues behave as concentration cells of complex type.

The evidence for the first point of view has been put forward very clearly by Lillie.[1] He has shown that there is much reason to believe that the transmission of stimulus is " essentially a case of secondary electrical stimulation," and he regards electrical stimulation as an alteration of the surface film by oxidation processes which are accelerated by the polarising effect of the current upon the membrane. Lillie[2] supports his view by a very interesting comparison with metallic surfaces in the active and passive states, and he shows that both stimulation and the propagation of stimulus may be accounted for on this hypothesis. Nevertheless there are great difficulties in its acceptance. Not only does it involve a specialised assumption—that of an " unstable readily reducible substance " in the film which can be readily altered and " recovers its original constitution and physical properties " during the refractory state—but also, like all other hypotheses of this class, it fails to explain the absence of any recognisable heat effect in stimulation. This serious difficulty has led many physiologists to look with more confidence to the second type of theory as an explanation of bio-electric phenomena.

But even among those who accept some type of concentration cell as the analogue of the living cell in respect of its electrical behaviour there is much difference of opinion, and further knowledge not only of biological but also of chemical processes is necessary before such differences of opinion can be finally resolved. Nevertheless, there is much that is common ground to all or most of the rival hypotheses of this class, and it would seem time to attempt to ascertain how far such common assumptions will carry us beyond the interpretation of the polarising process and to discover what further assumptions are necessary in order to account for the changes of permeability which accompany changes in polarisation, for excitation in irritable tissues, and for the transmission of stimulus.

A number of hypotheses agree in correlating the polarisation of the membrane with a difference of hydrogen ion con-

[1] *Amer. Journ. of Physiol.*, 1920, **8**, 40.
[2] *Physiol. Rev.* 1922, 2.

centration within and without the cell—polarisation of the membrane will then arise either as a consequence of a special permeability to one ion, as Bernstein suggested for the hydrogen ion, or as a consequence of special impermeability to one ion, as in the case of a colloidal electrolyte giving rise to the Donnan equilibrium. Loeb[1] has shown that in the case of gelatine chloride, hydrolytic dissociation of the protein salt will produce a difference of hydrogen ion concentration on the two sides of a membrane impermeable to the protein ion, from which the potential difference can be calculated with fair accuracy.

It may, however, also be assumed that the polarisation of the plasma membrane is due to its own content of electrolytes. The writer has attempted elsewhere[2] to explain the phenomena of permeability in terms of a membrane consisting of a dense aggregation of particles composed of substances of ampholytic character in equilibrium with a definite concentration of hydrogen ions; the polarisation of the membrane will in this case be determined by the acid reaction of the medium bathing it. A scheme of more definitely diphasic character has been advocated by Beutner,[3] who regards the plasma membrane as a lipoid film holding free acid in solution.

This reintroduction of the lipoid theory would make potential difference determined by the salt content of the aqueous phase, but, it is to be remarked, that even in this case the potential difference which has been observed in systems of this kind must be correlated with a difference of hydrogen ion concentration in the two phases.

We have, then, a variety of ways in which differences of hydrogen ion concentration may be correlated with a polarisation of the plasma membrane, and if it be assumed that the cell has a negative charge the effect of an increase of hydrogen ion concentration on the outer side of the plasma membrane may be expressed with precision. On Bernstein's theory it will drive hydrogen ions into the cell and reduce potential. If the difference of potential is due to a Donnan equilibrium arising from the presence of the anions of a hydrolysable colloidal salt within the cell a similar effect will be produced. An ampholytic membrane dissociating hydrogen ions below its iso-electric point will absorb hydrogen ions and reduce potential in the same circumstances; while a lipoid membrane containing free acid and having a negative charge owing to reaction with salt in the aqueous phase will also have its charge

[1] *Journ. of Gen. Physiology*, 1921, **3**, 667.
[2] *Biochem. Journ.*, 1921, **15**, 440.
[3] *Die Entstehung Elektrische Ströme in lebenden Geweben*, 1920.

reduced when the hydrogen ion concentration of this aqueous phase is increased by increase of salt concentrations.

On each of these hypotheses, then, an increase of hydrogen ion concentration outside the cell will produce a decrease of potential, and we have now to consider how such a change may be supposed to influence permeability. If the difference of potential upon the two sides of the membrane is derived from a source outside the membrane either by selective permeability or by Donnan equilibrium, such a relation cannot easily be formulated, although in the latter case some small difference of salt distribution will result, but where the membrane itself carries a charge it is easy to imagine that a sharp change of physical properties may accompany the neutralisation of the charge. For example, McBain[1] has shown that the sols formed by colloidal electrolytes consist of micelle containing charged particles and that the water content of these particles varies with their charge. At the point of minimum charge, which may be identified with the iso-electric point for a given ampholyte, permeability may be supposed to be a maximum owing to the dehydration of the particles. In a lipoid film change of potential must be associated with changes of surface tension and a continuous film would almost certainly break down at some definite potential. It appears to the writer quite probable that both effects may come into play and that the plasma membrane may consist of an aggregation of charged particles carrying lipoid films which in the least permeable condition of the membrane become continuous from particle to particle. An increase of hydrogen ion concentration might then have a double effect, altering surface tension and breaking the continuity of the film at the same time that it caused dehydration of the particles and enlarged the free channels by which water soluble substances could pass into the cell.

It will be assumed in the sequel that the plasma membrane contains substances which dissociate hydrogen ions and that the reaction of the medium with which the membrane is in contact defines the condition of equilibrium. It has been shown that increase of permeability would be likely to result when the charge upon such a membrane is neutralised, and we have now to consider the matter in further detail in relation to the mechanism of stimulation. This is in effect to examine by what means changes of hydrogen ion concentration at the surface of the membrane may be brought about.

It is important to remember that in a complex system such as that presented by the living cell change of reaction may be due to a variety of causes. It is believed by physiologists

[1] *Proc. Roy. Soc.*, A. 1920, 97, 44.

that the reaction of protoplasm is controlled by buffer mixtures containing acid and alkaline constituents which dissociate according to equations of the type

$$H_2X \rightleftarrows HX' + H^{\cdot}$$
$$MHX \rightleftarrows HX' + M^{\cdot}$$

The acidity of such a mixture is defined by the ratio $(H_2X)/(HX_1)$ and is normally increased by the addition of a neutral salt, since this decreases the dissociation of MHX. The writer[1] has studied the action of salts upon buffer mixtures in some detail and has shown that the action of balanced mixtures such as Ringer's solution may be explained on the assumption that such mixtures react with the buffer mixtures of the cell to produce the optimum hydrogen ion concentration in relation to permeability, while the antagonism of such ions as sodium and calcium may be explained on somewhat similar lines.

These effects are of great importance in connection with the theory of stimulation. Not only does the "current of rest" due to the direct action of salts receive a simple explanation, but it is also possible to explain the more complex phenomena of electrical stimulation. The following discussion is confined to the action of salts upon buffer mixtures, as it appears to the writer that this is likely to be the predominating effect in most cases of stimulation; but it should be remarked that much of the argument may be equally applicable to the effect of salt upon hydrogen ion concentration when this is conditioned by the establishment of a Donnan equilibrium as a consequence of the presence within the cell of the non-diffusible ions of a partially hydrolysed salt.

Nernst's well-known formula[2] is an expression for the quantity of electricity necessary to produce excitation in a given case, but the basis of this theory has been somewhat obscured in English physiological literation. Hill[3] (1910) and Keith Lucas[4] (1912) both tend to express excitation as due to the separation of ions to which the membrane is impermeable, and they omit all consideration of the differences of salt concentration which are produced. Nernst's theory was, however, derived from a consideration of the manner in which such differences of concentration are brought about, and it is not unnecessary to lay stress upon this point since, though the

[1] *loc. cit.*

[2] $i = \dfrac{\lambda}{1 - \mu\theta^t}$,

where i is the smallest current which will excite,
t is the duration of the current,
λ, $\mu\theta$ are constants.

[3] *Journ. of Physiol.*, 1910, **40**, 190.

[4] *Proc. Roy. Soc.*, B. 1912, **85**, 495.

physical basis of the theory is unimportant for its mathematical development, it is of great significance in the development of a general theory of stimulation.

In order to make the present discussion clearer, it will be advisable to recapitulate briefly the considerations put forward by Nernst[1] and demonstrated by him experimentally.

The apparatus employed by Nernst consisted of a "U" tube containing phenol with a layer of water in each arm. Both liquids contained electrolyte and were in chemical equilibrium with one another. When a current of electricity is passed through a series of liquids arranged in this manner a change of concentration of electrolyte is produced at the two surfaces of contact in every case in which the transport number of the electrolyte is different in the two liquids. Let n_1, n_2 be the transport numbers for a given electrolyte in water and phenol respectively, and suppose the current to pass from water to phenol. At the boundary next the anode there will be an import of n_1 cations and $1-n_2$ anions, and an export of n_2 cations and of $1-n_1$ anions. The change of concentration will be the algebraic sum of these quantities and will be equal to n_1-n_2 for cations and anions alike. There will, therefore, be increase or decrease of concentration at the anode according as $n_1 >$ or $< n_2$. At the cathode the process is reversed, and there is increase or decrease of concentration according as $n_2 >$ or $< n_1$. If the partition coefficience between phenol and water is small, the whole change of concentration may be regarded as taking place in the aqueous phase.

Nernst assumed that similar conditions prevail in the living cell, and that there is a semi-permeable membrane at the boundary of protoplasm of a similar character to that which exists, *e.g.*, at the boundary between benzene and water. On this theory the semi-permeability of protoplasm is a consequence of the relative insolubility in it of certain salts, just as the semi-permeability of a film of benzene is due to its inability to dissolve salts, although organic acids are readily soluble in it. It is not, however, necessary to Nernst's theory that semi-permeability should be produced in this manner. The essence of the theory is the existence of a semi-permeable membrane at which differences of concentration will arise when a difference of potential is artificially produced. If this is itself polarised under normal conditions any change of concentration at the membrane will tend to increase or diminish the existing potential difference, and to this change stimulation is to be ascribed.

Before considering the special difficulties which are attached to this theory, the effect of the presence of buffer solutions in

[1] *Ann. d. Physik.*, 1932, 8, 600.

the protoplasm must be discussed. It will be assumed that the sodium chloride is the principal electrolyte present and that the cation has the relatively greater velocity in the cell membrane. In this case the concentration of sodium chloride will increase at the cathode, and if buffer salts are present an increase of hydrogen ion concentration will result. At the anode there will be a corresponding decrease in the concentration of sodium chloride with a resulting decrease of hydrogen ion concentration. Cathodic excitation can thus be correlated with increase of hydrogen ion concentration and the production of permeability, and as hydrogen ions are driven into the membrane at this point, the cathode region will be that at which the current enters the tissues and will thus be negative.

There are two points of difficulty in connection with Nernst's theory of excitation :

(1) The fact that a slowly increasing current will not excite.

(2) The occurrence of anodic excitement at break.

The first of these difficulties has been met by the hypothesis of an "accommodation" of the membrane—a depolarising process taking place at a rate which makes it ineffective when polarisation is rapidly induced. Some attempt has been made to explain "accommodation" as a consequence of the colloidal character of the membrane, but without much success, and the hypothesis remains little more than a statement of the depolarising process. A better explanation is afforded by the leakage of salts across the membrane during the period of partial permeability—that condition of the membrane which becomes evident during the "relative refractory state." During this condition the time factor is of great significance. Given sufficient time, any difference of concentration produced by the current will equalise itself by the process of diffusion. If we abandon the simple assumption made above that sodium chloride carries the whole current and take into consideration that the salt NaHX will also tend to concentrate at the cathode and diminish at the anode, we have an intensification of this effect. Concentration of this salt will tend to diminish hydrogen ion concentration, and since it will diffuse more slowly owing to its greater complexity and less ionised condition it will produce a greater effect the longer the time available for diffusion. Anodic excitement seems to be susceptible to a similar explanation.

Any theory of stimulation must take into account not only those phenomena which have been discussed above, but also those relating to the existence of a "refractory state" to the propagation of stimulus and to the production of narcosis. It is suggested here that the refractory state and the propagation of stimulus are due to one and the same cause—

the production of permeability. We have hitherto assumed that the polarisation of the membrane is due to the chemical composition of the membrane itself, and we have left unconsidered the reaction of the cell contents. If, however, we assume that the liquid inside the cell is slightly more acid than that which bathes the outer surface of the membrane it is possible to suggest an explanation of these phenomena. The order of events will then be as follows. Concentration of salt at a point on the outer surface of the membrane will produce an increase of hydrogen ion concentration at this point; permeability will ensue and the cell contents will tend to diffuse outwards; the hydrogen ion concentration will then be raised above the permeability point and will induce a "refractory state." From this point of maximum hydrogen ion concentration acid will diffuse and at a certain distance from this point the reaction corresponding to permeability will again be reached and the process set in train once more. On this hypothesis the medium bathing the membrane in its refractory state is at a hydrogen ion concentration above that corresponding to permeability—while in the normal state the hydrogen ion concentration is below this point; the medium will therefore pass through the concentration corresponding to permeability in recovering its normal condition—and, in order to avoid this dilemma, it is necessary to suppose that the membrane itself undergoes some change when it is brought into contact with the cell contents. Such a supposition is not improbable, for the membrane must be in chemical equilibrium with the aqueous layer adjacent to it, and may well absorb certain of the soluble constituents of the cell contents in these circumstances.

Narcosis.—The hypothesis outlined above receives some support from a consideration of the action of narcotics. There is much reason to believe that narcotics produce a decrease of permeability. They should therefore, in accordance with the present theory, produce also an increase in the ionisation of the membrane. The writer has shown that the action of alcohol on buffer solutions is to produce a marked increase of alkalinity and that this is likely to be true of many water-soluble non-electrolytes. Since, however, most narcotics are only very slightly soluble in water this effect will be very small in most cases and the characteristic action of narcotics must be otherwise explained. The peculiar physiological effects of alcohol may quite possibly be due in part to its effect upon the water soluble constituents of the cell, but the more general effect of narcotics must be due to their absorption by the cell membrane. This will affect the dissociation of the electrolytes of which the membrane is composed. It is desired to

draw attention here to two effects which are likely to be of especial importance in connection with the theory of narcosis.

(1) The action of non-electrolytes upon ampholytes. Michaelis has shown that at the iso-electric point

$$[H^{\cdot}] = \sqrt{\frac{Ka}{Kb}}\,K$$

where Ka Kb are the acid and basic dissociation constants of an amphoteric electrolyte, and K is that of water. The addition of a non-electrolyte will modify these values, and Arrhenius [1] has shown in his work on the action of non-electrolytes in reducing the conductivity of electrolytes that the less an electrolyte is dissociated the greater will be the effect of a non-electrolyte upon its degree of dissociation—other things being equal. It is therefore probable that Ka and Kb will be differentially affected and that if the membrane consists of acid ampholytes for which $Ka > Kb$, the value of $[H^{\cdot}]$ will be increased. This effect has not yet been experimentally investigated even for simple amphoteric electrolytes, and in the case of colloids the matter is rendered still more uncertain by our ignorance of the manner in which proteins and other colloidal ampholytes are dissociated and of their condition at the iso-electric point; it must therefore be left as a suggestion which at present is beyond the reach of verification.

(2) There is, however, another effect more easily verified and possibly of greater importance. If the lipoid film which constitutes the plasma membrane contains free acid this content of acid will be modified by the absorption of non-electrolytes. Where substances such as ether are in question it will generally be increased—it is worth noticing that the acids most important in animal and vegetable metabolism, lactic and malic acids, are especially soluble in ether—and the effect of this increase of acid content may be to increase the polarisation of the membrane and thus to reduce permeability.

If narcotics decrease permeability, and the production of permeability is the condition for the transmission of stimulus, their effect upon transmission can be easily understood. For a given stimulus the area of the membrane rendered permeable will be smaller and it may be only where the increase of hydrogen ion concentration due to the original excitation is still felt that the stimulus will be propagated.

A few words are necessary in conclusion as to the assumptions which have been made in connection with the foregoing theory. The concept of a semi-permeable membrane has been

[1] *Zeitsch. f. Physik. Chem.*, 1892, **9**, 487.

left undefined. If a membrane is impermeable to electrolytes this may be due to complete impermeability either to both ions or to one. In the latter case the polarisation of the membrane will be affected, and if hydrogen ions are those which pass through, they will tend to form a double layer and to neutralise a negative charge upon the membrane when they are present in higher concentration within the cell than outside it. Such a concept has much to recommend it and is in complete conformity with the present theory of stimulation. This theory has been elaborated for the particular assumption of a membrane in contact with a medium at a hydrogen ion concentration below that corresponding to permeability. This medium has been supposed to be relatively more permeable to cations than to anions, and it has been shown that in these circumstances excitation and production of permeability will occur at the cathodes. If the hydrogen ion concentration is above that corresponding to permeability and the membrane is more permeable to anions than to cations, cathodic excitation will again occur, for in this case there will be diminution of hydrogen ion concentration at the cathode. It may be suggested that differences in the condition of the plasma membrane afford a simple explanation of the phenomenon of positive and negative reaction to such stimuli as that of gravity. In the root and stem, for example, it may be supposed that the root cells are maintained above the permeability point by the action of salts while those of the stem are below this point, in which case the condition for maximum permeability in the root would be the condition for minimum permeability in the stem.

Summary

A number of theories postulate a difference of hydrogen ion concentration between the protoplasm of the living cell and the medium which bathes its outer surface. It is pointed out that the electrical polarisation of the plasma membrane can be explained in terms of this difference of concentration, and that if the hydrogen ion concentration also determines the physical condition of the plasma membrane changes of hydrogen ion concentration will also condition changes of permeability.

According to Nernst's theory the excitation produced by the passage of an electric current through living tissue is due to the changes of salt concentration which it produces at the surface of the plasma membrane. It is shown that in a buffer mixture these changes of salt concentration will produce corresponding changes of hydrogen ion concentration, and on the above hypothesis will increase or decrease the permeability

of the membrane. Excitations may thus be explained as due to the production of permeability at a point of the plasma membrane by a raising or lowering of the hydrogen ion concentration at this point.

The propagation of stimulus may be explained by the same hypothesis thus : If increase of hydrogen ion concentration produces this effect and the liquid inside the cell is slightly more acid than that outside it, the production of permeability will allow acid to leak through the membrane and will raise the reaction to a hydrogen ion concentration at which the membrane is again impermeable. From this point of hyper-acidity hydrogen ions will diffuse and at a certain distance from the point the concentration corresponding to permeability will again be reached and the process once more set in train. The condition of hyper-acidity may be identified with the " refractory state."

Narcosis can be interpreted as a condition of lessened permeability resulting from a decrease of hydrogen ion concentration in the liquid in contact with the membrane, or more generally to greater polarisation of the membrane as a consequence of an alteration of its acid content or of the degree of dissociation of the acid substances which it contains.

RECENT ADVANCES IN PHOTOGRAPHIC SCIENCE

By F. C. TOY, D.Sc., F.Inst.P.

Physics Department, British Photographic Research Association Laboratories

Nearly everyone who is interested in photography has, at some time or another, handled a modern photographic plate, but probably only a few have any idea of its structure or of the problems facing the research worker in photographic science. In the photographic industry—as in every other—the last few years have witnessed an increasing application of scientific thought and research, and it is proposed to give here an outline of some of the recent advances which such research has made in our knowledge of the theory of the photographic process.

When a photographic plate is exposed in the camera and then developed, a " negative " is obtained, *i.e.* a picture of the original in which white is reproduced as black and black as white. The extent of the blackening which is produced in the negative at any particular point depends on the exposure which that part of the plate has received. The scientific name for this blackness is the photographic density, which is strictly defined as the logarithm of the optical opacity, *i.e.* of the ratio of the intensity of light incident on the plate to that transmitted.

The greater the quantity of light energy which a photographic plate receives, the greater is the density produced on development, up to a certain limiting value varying with the kind of plate. The curve obtained by plotting the density against the logarithm of the amount of light energy which produces it is called the Characteristic Curve, a name originated by Hurter and Driffield, the pioneers of modern photographic science. As the name implies, the actual shape of the curve depends on the emulsion. To those in the industry this curve is of immense importance, because an examination of it indicates to them the suitability of the emulsion for any particular purpose. Naturally, therefore, the main problems in which photographic research workers are interested are the causes of the formation of such a curve.

Let us first examine its significance from a theoretical point of view, and see what is the real cause of the density or blacken-

ing which is produced in a photographic plate on development after exposure to light. The light sensitive emulsion consists essentially of minute crystals, composed mainly of silver bromide, iodide, or chloride, and embedded in a layer of gelatin. The linear dimensions of these particles, or grains, are of the order of a wave-length of visible light, *i.e.*, about one two-thousandth part of a millimetre. Their size depends on the kind of emulsion; on the average the most sensitive, or "fastest," emulsions consist of large grains up to as much as one-hundredth of a millimetre across, while small grains are generally characteristic of the "slower" kinds of emulsions. The photographs in Fig. 1 are typical examples of these two kinds. It will be seen that the large grains are mostly of definite crystalline shape and the smaller ones, though appearing as spherical particles, have been shown to possess the same crystal structure.

It has been known for a very long time that the blackening produced in a photographic negative is due to the chemical action of the developer on the silver haloid, resulting in its reduction to metallic silver. The greater the density, the greater is the fraction of the total quantity of silver bromide in the film which is changed to silver, the maximum density corresponding to the case where there is no silver bromide left unreduced.

Now, having realised that the emulsion consists of individual and distinct particles, the reader can at once conceive of two ways in which this total quantity of reduced silver can be made up. In the first place, it may be that a *part of each and every grain* becomes opaque, *i.e.* is changed from silver bromide to silver, on development. Thus, for example, if the density were one-tenth of the possible maximum, this would mean that one-tenth of *each grain* had been reduced; if it were one-half, that one-half of each grain had been reduced, and so on. In this case the fraction of each grain which was made reducible would increase continuously with the exposure. Secondly, it may be that a certain number of all the grains present is made reducible, each of the remaining grains being absolutely unchanged. In this case a grain is either completely reducible, or not reducible at all. There is no intermediate possibility, and at some moment during the time that the plate is exposed each grain which becomes developable must undergo some sudden change in its character.

It is very easy to find out which of these processes takes place. All we have to do is to examine a photograph of an emulsion after it has been exposed and developed. One of these is shown in Fig 2, where the emulsions are the same as those in Fig. 1. This tells us at once that it is the latter process which occurs, for the grains are either completely

reduced by the developer, or absolutely unchanged, as in the first figure.

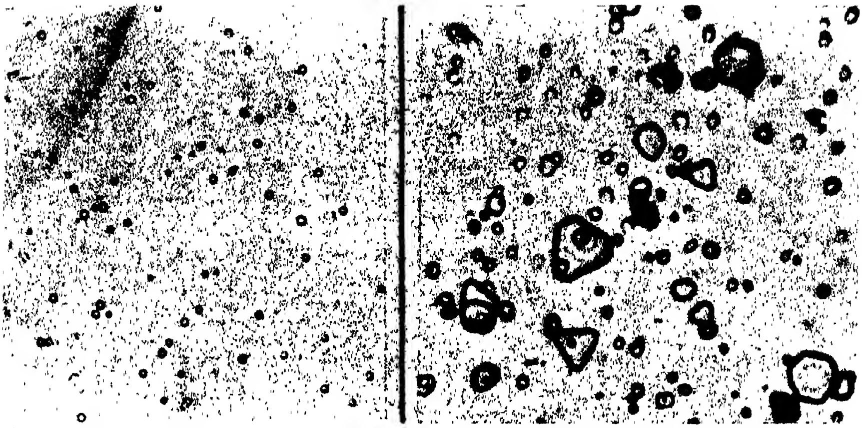
It follows from the above that a big density means a large percentage of grains changed, a small density corresponds with a small percentage changed, and so on. Thus the ordinary characteristic curve, which shows that there is a continuous increase of density with exposure, is essentially an expression of the fact that each individual grain behaves differently under the influence of light, for they all become developable at different times.

These facts provide sufficient evidence for it to be said that the characteristics of a photographic emulsion must depend, first of all, on the behaviour of the individual grains in it, and that a complete theory of the photographic process can be built up only on a knowledge of the way in which these single grains behave under the influence of radiation. The search for the true law of the behaviour of these grains is one of the most fascinating problems of present-day research.

The fact that the individual grain is the fundamental unit and that its behaviour in relation to the exposure is of primary importance was recognised in the years immediately following the war. Professor The Svedberg, of Upsala University, Sweden, seems to have published the first paper in this connection in the *Zeitschrift für Wissenschaftliche Photographie* in 1920. About the same time R. E. Slade and G. I. Higson of the British Photographic Research Association were working along the same lines, and A. P. H. Trivelli and others of the Eastman Kodak Company, under the direction of C. E. K. Mees, were investigating the microscopic structure of emulsions.

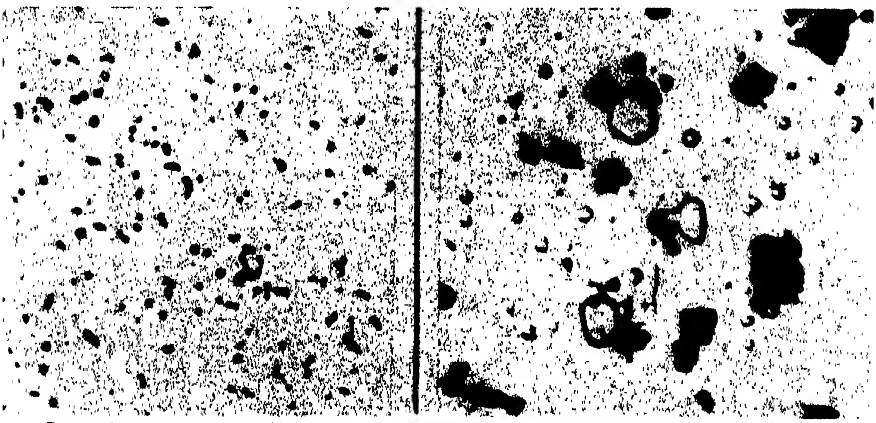
The problems which confronted all these workers were these: What is the nature of the sudden change which occurs in grains which are made developable? What is the process by which the light causes it? Why is it that the change occurs at different times in different grains? Can the conditions in the emulsion be made such that the change takes place in every grain simultaneously?

Svedberg suggested certain possibilities in answer to this last question. In effect he said that the fact that all the grains were not changed simultaneously might be explained thus: In the ordinary commercial plate the layer of emulsion has quite an appreciable thickness, so that grains "deep down" are very often covered by other grains nearer the surface (see Fig. 3). This means that some receive more light than others. Svedberg suggested that if every grain were fully exposed to the incident radiation, they would all be changed simultaneously. This, however, he easily proved not to be true by making a special plate with such a thin coating of emulsion that all the grains



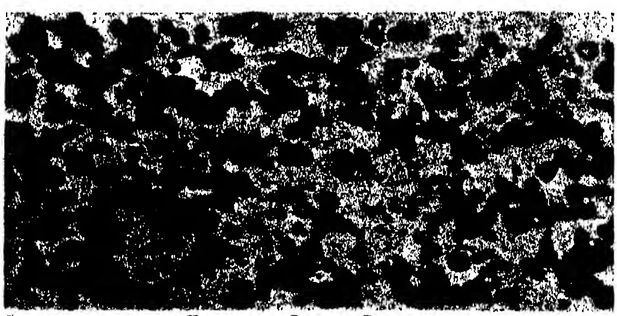
Slow Process Emulsion. Unexposed. Fast Emulsion. Unexposed.
Magnification, 2,000.

Fig. 1.



Same Emulsions as in Fig. 1, but developed Fully after Exposure to Light.
Magnification, 2,000.

Fig. 2.



Section through Emulsion Layer Perpendicular to Plate.
Magnification, 1,500.

Fig. 3.

were in a single layer; he found that they still became developable at different times. Fig. 2 is, indeed, an illustration of this fact. Next he suggested that the difference in behaviour might be due to the fact that the grains vary in size and that if they were all of the same dimensions they would behave identically. But as the result of further experiments he came to the conclusion that lack of uniformity was not the cause of the varying behaviour of the grains; Slade and Higson came to the same conclusion independently about the same time.

The only difficulty about accepting these conclusions as unquestionable was that in no case had these workers experimented with grains that were really the same size, for in the most uniform emulsions they had used there was a variation in volume of several hundred per cent. However, towards the end of 1921 their conclusions were confirmed beyond doubt by further experiments in the laboratories of the British Photographic Research Association, the grains used being so nearly identical geometrically that their behaviour in the theoretically perfect case of absolute uniformity could no longer be doubted.

Early in 1922, then, the position was this. It had been proved that if a photographic emulsion could be made consisting of a single layer of grains which were absolutely identical both in size and shape and in every way indistinguishable from one another, and which did not overlap and which were also similarly oriented to the incident light, they would still behave differently.

Actual experiments indicated that some grains required roughly two hundred times as much light energy, *incident on unit area of the plate*, as others, in order to make them developable.

There is no *a priori* reason why the actual change which takes place should not be the same in every grain, but without considering the nature of the change, we may proceed to discuss the attempts which have been made to explain the facts set out above.

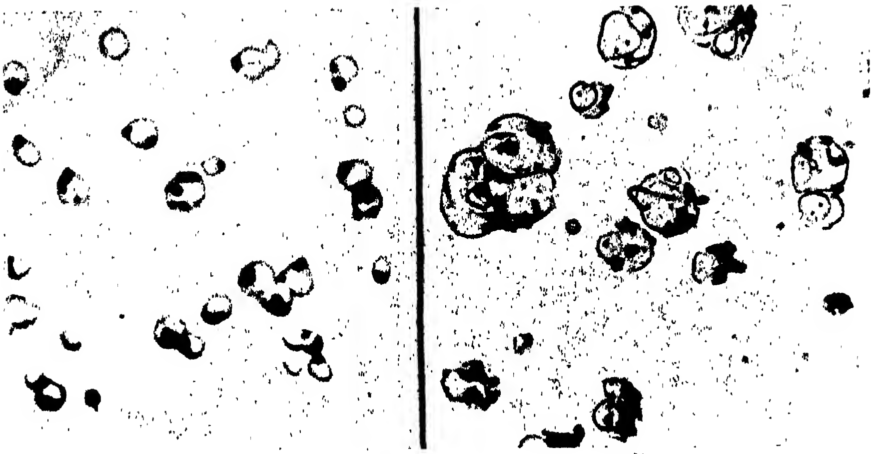
A little consideration will show that there are two fundamentally different points of view from which the problem can be approached. We have to explain why grains which are *apparently* identical in every way, behave differently. The significance of the italicised word lies in the fact that the grains may not really be identical at all, but very complicated and heterogeneous systems. From this view-point it may easily be that the amount of light energy required to make a grain developable is different in every case, so that even though every grain has received the same amount of light energy in a given time only a certain number will be developable. It

will be shown later that there is a great deal of evidence in favour of some kind of heterogeneity of the grains.

On the other hand, we can start with the assumption that the grains are, as they appear, identical in every way, but that the light energy is not distributed equally amongst them. Such a thought leads the physicist at once to Einstein's view of Planck's Light Quantum theory, in which the light energy is considered as emitted, transmitted, and perhaps absorbed, in finite discrete quantities. From this point of view each grain becomes developable when it is *hit* by one, or more, of these light quanta, the number of grains which are changed under given conditions being governed by the *probability* of their being hit.

Before considering in detail the evidence in favour of each of these theories, let us study briefly the mode of development of the grains.

It has been generally believed that the primary action of the light on the grains is to form in or on their surfaces certain "centres" or "points of infection," which are actually the points from which development starts. As long ago as 1911 Chapman Jones showed that by stopping development almost immediately after it was started, there could be obtained on the grains particles of silver which, though so small that they cannot be seen in the microscope, can be shown to be present by enlargement to visible dimensions by the deposition on them of mercury. In 1917 Hodgson, in America, showed that by stopping the development at a somewhat later stage, silver, formed by the developer, could be observed around certain points only in the grain. Working along the lines suggested by Hodgson, Svedberg, early in 1922, showed that the centres of development occur evenly over the surface of small spherical grains such as those shown in Fig. 4A (which is a reproduction from one of Svedberg's papers), and are distributed amongst the grains according to the laws of chance. Independently and about the same time, it was shown in the British Photographic Research Association laboratories that the same laws held if the grains were flat plates such as occur in highly sensitive emulsions, only in this case the centres were distributed mostly around the edges of the crystals. (*cf.* Fig. 4B.) Svedberg's proof that the centres occur on the surface in the case of spherical grains is most ingenious and is worth describing in his own words: "A number of photomicrographs . . . as in Fig. 4A were prepared. . . . The clearest parts were sought out and examined at high enlargement on a screen. A series of circles were drawn on paper and the image of each of the halide grains containing a developed centre was made to coincide with one of these circles, and the position of each developed centre



(A) (B)

FIG. 4.

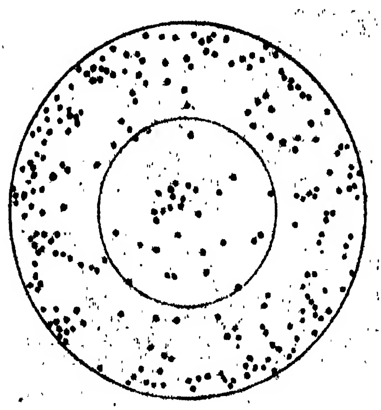


FIG. 5.

marked on the paper. In this way I had the position of 244 centres indicated on six circles. By means of an ordinary photographic enlargement apparatus the six circles were reduced to the same size, and all printed on the same surface (see Fig. 5). An inner circle was drawn with half the radius of the outer one, and the number of centres without and within this circle counted. The values found were 212 and 32 respectively. Thus the ratio between the number of centres on both sides of the border line is 6·6. Then calculating the ratio to be expected according as the centres are distributed on the surface of a sphere or throughout its volume, he obtained the values of 6·5 and 1·9 respectively. This is conclusive proof that the centres are distributed on the surface. Since in the case of flat grains the majority of centres are on the edges (and must therefore be on the surface) it is fairly safe to assume that for all kinds of grains the centres which cause development are located on the surface.

It is easy to prove that the centres are distributed amongst the grains according to the law of probability. This tells us that the chance (P_r) of the distribution of N centres between a grains resulting in any one grain having r centres is

$$P_r = (N/a)^r \cdot e^{-N/a} \cdot /r! \quad . \quad . \quad (1)$$

This formula was tested by direct counting of the centres. Svedberg found that it held just as well when the grains were exposed to X-rays as when they were exposed to light.

Now for a grain to be developable it must have *at least* one centre, and it can easily be shown that the chance (P) of this happening is

$$P = 1 - e^{-\nu} \quad . \quad . \quad . \quad . \quad (2)$$

where $\nu = N/a =$ average number of centres per grain. But P is also the fraction of grains changed, which we will denote by k/N, so that

$$\log_e N/(N-k) = \nu \quad . \quad . \quad . \quad (3)$$

which gives the relation between the fraction of grains changed and the corresponding number of centres.

There is one point in regard to the centres which may be dealt with here. It is a well-known fact that many chemical reactions between solids and liquids commence at corners and sharp edges where the solution pressure is highest, and this fact alone might at first sight appear to be the explanation of the initial localisation of development at certain points. But evidence given in a recent paper by the author shows that it is practically impossible for this to be the case.

We are now in a position to discuss the rival theories brought

forward to explain the behaviour of the grains under the influence of radiation.

THE LIGHT QUANTUM THEORY OF PHOTOGRAPHIC EXPOSURE

The idea that the difference in the behaviour of the grains is due to the structure of light is being made the subject of an exhaustive theoretical and experimental investigation by the Eastman Kodak Company. The theory has been thoroughly worked out by S. L. Silberstein in recent papers in *The Philosophical Magazine*. The essential idea is that a grain must be hit by one or more than one light quantum in order to become developable. The light energy is assumed to be transmitted from the source to the plate in finite discrete "darts," the energy contained in a single dart being directly proportional to the frequency of the light. Silberstein has shown that for grains of a given area of cross-section, the fraction of them which will be hit in a given exposure is given by

$$k/N = 1 - e^{-na} \qquad . \qquad . \qquad (4)$$

and since by equation (3)

$$\log_e N/(N-k) = \nu$$
$$\therefore \qquad \nu = na,$$

where k is the number of grains developable out of a total of N per unit area of the plate, a is the cross-section of a single grain and n the total number of light quanta which impinge on the plate.

When Svedberg's paper on the distribution of the developable centres was published, the suggestion was put forward that these points represent the actual places where the light quanta strike the grains. But this is almost impossible, since if it were true, the centres, in the case of spherical grains, should not be distributed equally over the *surface*, but over its *projected area*, which is a very different thing. Similarly in the case of flat grains they should be distributed evenly over them, and not concentrated on the edges. It seems to the writer that the idea of the centres being the actual points of bombardment may be definitely ruled out of court. This, of course, in itself does not disprove Silberstein's equation, since it is just possible, though highly improbable, that the grains, while subjected to bombardment by quanta, do not start to develop at the points where they have been struck. Equation (4) cannot therefore be rejected until it has been tested by direct experiments.

Such tests have already been commenced by A. P. H. Trivelli and L. Righter in the Kodak Company's Laboratories. Now obviously the simplest possible case to take is that of sets

of grains all of the same size and shape, so that a has a perfectly definite exact value in each set, and is not an average of a large number of widely different values. Unfortunately they did not do this, considering, instead, this very special case:—It is found for some emulsions in which the grains are not all separate but are grouped together in clumps, the clump develops as a unit, the only condition necessary and one that is sufficient for its developability being that one of the grains in it is in itself developable. Using such an emulsion, Trivelli and Righter tested equation (4) for various sized clumps, a being the projected area of a clump instead of that of a single grain. They did this in order that they might get a much bigger variation in a than was possible by using single grains, and so provide a more severe test of the theory. The results of their main experiments are shown by the curves in Fig 6. The smooth curve represents the theoretical values of k/N for a fixed exposure (*i.e.* n = constant) and varying values of a. It will be seen that only in one case is the agreement fairly good. There is certainly room for doubt if these experiments really support the theory.

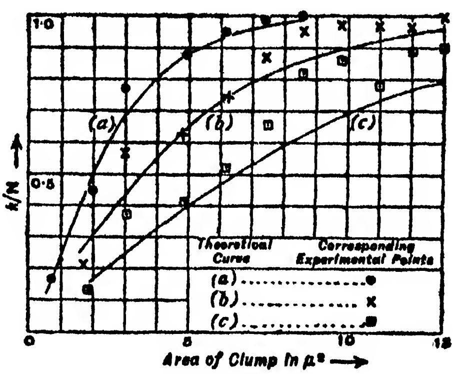


FIG. 6.

When the equation is tested in the simplest case of single grains, in which every grain in a set has the same size and shape, it breaks down completely. The evidence on which this statement is based is fully described in a recent paper by the author in *The Philosophical Magazine*; lack of space prevents a discussion of it here.

Silberstein discusses in a second paper the possibility that only a part of the grain is vulnerable to the bombardment of the light quanta, *i.e.* he assumes that all parts of the grain are not equally sensitive to the light. This idea cannot be tested without knowledge of the actual quantity of light energy incident, and experiments to ascertain it are now being undertaken in the Kodak laboratories.

Any experimental evidence bearing on this theory is exceedingly important, not so much for its photographic significance—though this is very important—as for its contribution to the solution of one of the greatest problems of modern physics, the structure of radiation. Silberstein and his co-

workers claim that their experiments support their theory to a considerable extent. But the divergencies between theory and experiment which have been referred to are very great, so much so that they seem to show definitely that Silberstein's equation, or any other of the same general type, is fundamentally wrong as expressing the true mechanism of photographic exposure.

The Heterogeneity of the Silver Haloid Grain

There is now a mass of evidence, the accumulation of years of research, in favour of the view that the grains of a photographic emulsion are not pure silver haloid crystals, but are very complicated and heterogeneous systems. It is impossible in a short space to describe these researches in detail; only a brief summary of those which are most important can be given.

Luppo-Cramer has carried out a great number of experiments which have convinced him that the grains of a photographic emulsion contain a certain amount of silver. W. Reinders has shown experimentally the possibility of the existence of gelatin in the grains, and he attributes the high sensitivity to the presence of gelatin. W. D. Bancroft, Quincke and others apparently hold the same view—that the high sensitivity of photographic plates depends on the system silver haloid-gelatin. Trivelli and Sheppard, in a recent monograph on the Silver Bromide Grain of Photographic Emulsions, say "the crystalline structure of silver bromide in photographic emulsions is more complicated than that of silver bromide alone, not in so far as that one has to do with another lattice structure, but rather that the crystal lattice contains more or less regularly distributed foreign bodies which greatly affect the optical properties of silver bromide and, as Reinders has demonstrated, probably exert a very great influence on the light-sensitivity of the bromide." There is no doubt at all that the gelatin plays a very important part, and every manufacturer knows that different gelatins may give absolutely different kinds of emulsions.

After an emulsion is first precipitated it is subjected to the process of "ripening," a treatment which very largely increases its sensitivity. Now in this ripening process the grain size very often increases and one might expect a big grain to be more sensitive than a small one. But there are cases in which the ripening is accompanied by very little change in grain size, and yet the light sensitiveness of the emulsion is enormously increased. Such cases are very difficult to explain on the assumption that the grains consist of pure silver haloid.

In this connection some very important experiments have

recently been performed in the British Photographic Research Association laboratories by W. Clark. It is well known that certain chemical reducing agents have an effect on the photographic plate similar to that of light, *i.e.* in such a way that some of the grains can be reduced to silver by treatment with a developer. Such a substance is sodium arsenite. Clark found that when a plate which has been immersed in this solution for a suitable time is partially developed, the centres from which development starts are not only distributed amongst the grains according to the same law of chance as holds in the case when the exposure is to light, but they have actually the same topographical distribution, *i.e.* both the light and the sodium arsenite appear to act *on the same points* in the grains. Are these points silver haloid? Clark answered this question by an investigation of the action of sodium arsenite on pure silver bromide, and concluded that there was none. From these experiments one is forced to the conclusion that there is a substance in the grains which is not silver bromide and that it is this substance on which both the light and the chemical reducer first act. This means that the light sensitivity of the grains is due chiefly to the presence of this substance. If, therefore, the small traces of it can be removed the sensitiveness of the grains will be reduced enormously—reduced, in fact, until their new sensitiveness is that of pure silver bromide itself. The relative reduction in sensitiveness will be greater in the case of a specially sensitive emulsion than in the case of a comparatively insensitive one, since in the latter case the sensitivity due to the centres is more nearly equal to that of the silver bromide than it is in the former. How were these "vital points" to be removed? Clark made use of the fact that the latent image, *i.e.* the product of the light action in the grains, can be removed by chromic acid. Two pieces of a highly sensitive plate were taken, which for simplicity we will call A and B. A was exposed to an intensity such that, if developed, all the grains would be changed. The result of this was that all the specially sensitive points in A were changed by the light, whereas those in B which had not been exposed had not been affected in any way. A and B were then placed together in chromic acid (without developing A), so that all the special points in A were dissolved out while those in B were not.

After being washed and dried A and B were then exposed together in the ordinary sensitometric apparatus which is used for comparing the sensitiveness of different plates, and it was found that B was always much more sensitive than A. Further, it was found, exactly as predicted, that the plate sensitiveness could not by this means be reduced actually to zero, but to some constant small value which was very nearly the same for all

plates, corresponding with the sensitivity of the silver haloid itself. A few of the figures which were obtained in these experiments may be given. Those given below are known to the technical man as true H. and D. speeds, *i.e.* the speed of the plate as measured in the way originated by Hurter and Driffield.

We may take these numbers as being the relative sensitivities of the different plates.

Case 1

(*a*) Sensitiveness of untreated plate = 215.
(*b*) Sensitiveness after treatment with chromic acid = 68.
(*c*) Sensitiveness on treatment with chromic acid after preliminary exposure = 5.

Case 2

(*a*) Sensitiveness of untreated plate = 215.
(*b*) Sensitiveness on treatment with chromic acid = 105.
(*c*) Sensitiveness on treatment with chromic acid after preliminary exposure = 5.

These results are only some of those obtained and which were all of the same order. It will be seen that the mere fact of giving a preliminary exposure, followed by treatment with the acid, reduces the sensitiveness of the plate to one-fifteenth or one-twentieth of its value. This is because the most vital points in the grains have been removed. In the case of a slow plate, Clark still found a difference between (*b*) and (*c*), but, as we expected, it was not nearly so big as for a fast emulsion.

These experiments show that the centres are particles of a substance which is not silver bromide, and that the extreme light sensitivity of the emulsion is due mainly to their presence. The similarity between this conclusion and the phenomena of phosphorescence and fluorescence is very striking, for some substances which, when pure, are not luminescent possess this property to a remarkable degree when minute traces of an impurity are present.

The discovery of these reduction centres opens out many new lines of research. Of what are the centres composed? Do they consist of one definite substance, which is always the same whatever the emulsion may be? How and when are they formed and what exactly happens in the ripening process? What is the part they play in the reduction of the silver haloid? What is the nature of the change which is brought about by the light? The solution of these problems is of tremendous importance to those who are engaged in the manufacture of photographic emulsions.

THE LEVELS OF LAND AND SEA

GREAT BRITAIN

By SIR CHARLES CLOSE, K.B.E., C.B., F.R.S.

I. Sea-level

The study of the relative vertical movements of the land and of the surface of the sea is one which concerns both geology and geodesy. Long ago, Lyell pointed out that " Every page of the geological record proves to us that the relative levels of land and sea, and the position of the ocean and of continents and islands, have been always varying." And again, " A large portion of the earth's crust is always undergoing a change of level, some areas rising and others sinking at the rate of a few inches, or a few feet, or perhaps sometimes yards, in a century," and, " It is consistent with human experience that land should rise gradually in some places and be depressed in others." For geodesy the matter is one which concerns the datum surface to which all measurements are referred, and is connected with the problem of the adjustment of isostatic equilibrium, and the determination of the shape of the geoid.

There is also a practical side to the question. Thus, in the year 1911, the Royal Commission on Coast Erosion recommended that the Ordnance Survey and the Geological Survey should take steps, from time to time, to ascertain what changes of relative level of land and sea are taking place. The Royal Commission had specially in mind the erosion of portions of the East Coast of England, and the possibility that this might be caused, at least in part, by a sinking of the land with reference to the sea.

It is, of course, easy to fix marks on the surface of the land which we can watch and from which we can measure ; but with regard to the sea the matter is not so simple. What do we mean when we talk about sea-level ? This question much exercised the authorities of the Ordnance Survey about a hundred years ago and it became an urgent one shortly after the commencement of the Survey of Ireland in 1825. Colonel Colby, who was then Director of the Survey, consulted Airy, then Plumian Professor of Astronomy, and Whewell, shortly to

be Master of Trinity. The discussion as to the most suitable datum lasted for some years; meanwhile the datum for use in Ireland was not fixed as mean sea-level, but as the level of a spring tide, at low water, observed at Poolbeg Lighthouse, in Dublin Bay, on April 8, 1837. This idea of taking a low water-line as the datum appears to have been due to the desire to avoid negative values for the space between the high and low water-lines, and also to the erroneous idea that the low and high water-lines were "level."

Captain Larcom, afterwards Sir Thomas Larcom, who was, under Colby, in charge of the Survey of Ireland, thus put the matter in a lecture to the British Association at Cork in 1843: "The datum [for land heights] most commonly used is the level of the sea, doubtless from the shore-line being the limit of the land, and the point at which roads must cease, as well as from an impression that it was itself a level line, and, therefore, as the first contour, the most appropriate and natural zero from which to take the others. . . . It has been a point much discussed whether the high water, the low water, or the mean state of the Tide offers the most level line."

But already in 1838 Whewell had written a valuable report on "A level line measured, from the Bristol Channel to the English Channel during the year 1837–8, by Mr. Bunt," the opening sentence of which reads, "At several meetings of the British Association it was suggested that the exact determination of the relative level of three points, considerably distant from each other, on the coasts of this island might throw light upon several important questions," particularly how far the earth's surface is permanent and "what ought to be understood by the level of the sea." He remarks that surveyors and naval men are in the habit of assuming the surface of low water to represent the level of the sea, whereas it is not even approximately a level surface. He then advises the adoption of mean water, "the means of low and high water," as the level of the sea. It is a pity that this report appeared after the adoption of a low-water datum for Ireland. All the heights on Irish maps are, in consequence, about 8 ft. too great to this day.

Whewell's "mean water" is not, however, itself a perfectly satisfactory datum, although it is nearly so. What we wish to find out, nowadays, is the mean position of the sea surface as determined over a considerable period of time, at all states of the tide, and not merely at high and low water. By this means we hope to arrive at an almost invariable datum. The procedure is equivalent to assuming that the amount of water in the ocean, and the contours of the ocean-bed, remain substantially the same over periods of a few years, ignoring minor changes which will not materially affect the level of the surface.

For about eighty years all the heights on the Ordnance maps of Great Britain have been referred to an approximate mean-sea-level at Liverpool, and it may be of interest to record how this was arrived at. Tidal observations were made at the Victoria Dock at Liverpool, in 1844, by fixing a graduated pole at the old entrance and noting the heights of the water surface, at five-minute intervals, for about half an hour each side of high and low water. These observations were continued from March 7 to 16 inclusive. The mean tide level thus obtained was found to be 43·14 ft. above a provisional datum which had been adopted in order not to delay the levelling. This figure was rounded off, and mean-sea-level was taken as exactly 43 ft. above the provisional datum. We shall see, later on, that, as it happens, this value is very fairly accurate.

Leaving out of consideration, for the moment, the question of the tides, the height of the sea-surface is affected by waves, by the addition of river water and rain, by evaporation, by alterations in salinity, by changes of temperature, by variations in barometric pressure, and by wind. Measurements would not usually be made where the influx of river water could affect the result. Rain, evaporation, and alterations of salinity and temperature will have an effect, though a small one, on the mean height over long periods; and, as is well known, the wind may have a marked influence on the local height of the tide, and variations in barometric pressure also have their importance.

The most satisfactory way of studying the variations in the level of the sea-surface is by means of self-recording tide-gauges. Such tide-gauges, though devised for the purpose of providing the data for predicting the times and heights of the tides, meet very efficiently the simpler needs of our present inquiry. The paper on which the recording pencil marks the height of the tide is usually ruled in straight lines which represent height intervals of one foot; and, at right angles to these, the time intervals are shown by lines at intervals representing one hour. If the harmonic curve which results from the combined motions of the height of the water shown by the pencil and of the clock-rotated drum on which the paper is fixed were symmetrical, then the mean of high and low water would be the same as mean-sea-level, for the period taken. But the tide-curves are not, in general, symmetrical, and the correct method is to integrate the tide-curve, that is, to ascertain its area between certain ordinates and divide this area (which is a product of heights in feet by time intervals in hours) by the total elapsed time in hours. This will give the mean-sea-level during the period in question.

But, in practice, a result free from arithmetical errors will

be obtained if the hourly heights are measured over a long period, and the mean taken. On the Ordnance Survey it is the custom to ascertain the monthly and yearly values in this way, the yearly mean being determined from more than 8,700 measurements.

In the year 1911 it was decided by the Ordnance Survey to execute afresh the primary level net-work of England and Wales, and, in connection with this operation, to determine the mean-sea-level round our coasts with greater accuracy than had been done in the past. For this purpose three tidal stations were chosen and self-registering tide-gauges were set up there; the stations, which were selected with the approval of the late Sir George Darwin, were Dunbar, Newlyn, and Felixstowe. The record at Dunbar commenced on May 1, 1913, at Newlyn on May 1, 1915, and at Felixstowe on May 1, 1917. The tide-gauges at these three stations are still at work, and, it is hoped, will continue to be at work for many years to come. We have thus, at present, about ten years' records available at Dunbar, eight years' at Newlyn, and six at Felixstowe. Of these stations, Newlyn, which is practically an open ocean station, is the most important, and we have there the record of the height of the sea surface for each hour during some 70,000 hours.

The following table gives, as far as at present available, the variations in annual mean-sea-level, *in inches*, at the three stations mentioned, taking the values for the year 1917–18 as zero. The values are positive except where otherwise marked:

Year.	Newlyn.	Dunbar.	Felixstowe.
1913–14 . . .	—	2·29	—
14–15 . . .	—	1·63	—
15–16 . . .	2·33	0·68	—
16–17 . . .	1·64	0·54	—
17–18 . . .	0·00	0·00	0·00
18–19 . . .	0·96	0·37	−0·50
19–20 . . .	0·50	0·79	0·34
20–21 . . .	0·77	0·88	−0·02
21–22 . . .	0·85	0·74	0·31

Here we have a range in the height of annual mean-sea-level at Newlyn of 2·3 in. and at Dunbar of 2·3 in., and a range from the *mean* of the tabulated period, of 1·3 and 1·4 in. respectively.

Here, then, is evidence, if such were needed, that the mean height of the surface of the sea during the period of a year is not a final value of mean-sea-level, yearly mean-sea-level being itself liable to long-period fluctuations. At Karachi, for example, there are accurate tidal records extending over

40 years, and it is found that the probable annual fluctuation of mean-sea-level, as compared with the general mean for 40 years, is 0·6 in. Prof. D'Arcy Thompson has collected the records of high and low water for Aberdeen for 52 years, and for Dundee for 16 years; it is found that the difference between the greatest and least annual mean-tide-level at Aberdeen is 4·6 in., and the probable fluctuation 0·7 in. At Dundee the probable fluctuation of annual mean-tide-level is also 0·7 in.

Mr. H. L. P. Jolly, of the Ordnance Survey, who is in charge of the reductions of the observations for levelling and mean-sea-level, remarks, with regard to the long period changes in the level of the sea: "For a few years the changes may appear to be following a certain roughly harmonic law. Then they break up into a different sequence and a new set of observations, independent of the first set, lead to quite a different tentative law of change. The conclusion is that many influences are at work. . . . That these influences are climatic in the broad sense of the term is a matter for confident assertion."

In addition to any long-period meteorological tides that may exist, we should expect to find evidence of a "latitude variation" tide with a period of 431 days and a lunar tide of about 18·6 year period. At the suggestion of the late Sir George Darwin, the Survey of India examined its many and excellent tidal records for evidence of the "latitude variation" tide, but without result. Professor D'Arcy Thompson, however, quotes investigations into the fluctuations of mean-sea-level at nine Danish stations for the years 1891–1911, which show the 431-day period, with a total amplitude of half an inch. And Professor Thompson has himself found evidence of this tide both at Aberdeen and Dundee, with a total amplitude of about 1 in. He quotes Sir George Darwin on the subject of the 18·6 year tide to the effect that its amplitude must be small; at Aberdeen the total amplitude would be about 0.7 in., but it has not yet been found possible to disentangle this tide from the meteorological fluctuations.

An interesting branch of the subject is the study of the effect of variations in barometric pressure, and of wind, on the level of the sea surface; and much evidence on this matter will be found in recent Ordnance Survey publications. There is a considerable literature on the subject, commencing with the observations made at Brest by Mr. Daussy, the French Hydrographer, from 1817 to 1831, and including those of Sir James Ross made at Port Leopold, latitude 74° North, in 1848. Other things being equal, it would be expected that a variation of x inches in the height of the mercury in the barometer would produce a variation of $-13·25\,x$ in the height of the sea-water, 13·25 being the ratio of the densities of mercury and sea-water.

And, if we could eliminate all other causes of variation of level, we should, without doubt, find this relationship, which sometimes appears very clearly.

Mr. Jolly has analysed the records of the Newlyn tide-gauge for the years 1915–20, the object of the analysis being "to try to represent the monthly mean-sea-levels as the sum of a long period term, a latitude variation term, an annual term, a semi-annual term and a term, proportional to the excess of local barometric height above an arbitrary zero." The "latitude variation" term was so small that it evaded detection, as did the semi-annual term. The period taken was May 1915 to January 1920, inclusive, and 57 monthly mean heights were dealt with. The best equation for representing x, the height of the sea-surface at Newlyn, for the period indicated, was found to be $x =$ a constant $+ 13{\cdot}2 \sin \frac{2\pi}{12} (t - 2{\cdot}32) - 13{\cdot}82 \ (\mathrm{H} - \mathrm{Ho})$ where t is measured in months from May 1, 1915, H is the mean monthly height of the barometer in inches, and Ho an arbitrary barometer zero. This expression is quoted to show the influence at Newlyn of the barometer term. It was used to predict the monthly fluctuations for 1920 with good results.

But it is clearly incorrect to deal with the effects of variations in the height of the barometer as if they were purely static and local. In a report of the United States Coast Survey for 1871, Prof. Ferrel remarks that changes of level appear to anticipate the barometric pressure and he attributes this to the effect of wind; and other observations of a similar character could be quoted. It is certain that the correlations between sea-level and barometric height can be improved by including wind factors; the effect of wind being, in general, to increase the variation due to the barometer.

Mr. Jolly has investigated the effect of the wind on the sea-level at Newlyn, the most important of the three Ordnance Survey stations, and has done this by taking into account, not the local barometer only, but also the barometric readings at four points 300 miles distant from Newlyn, and due north, south, east, and west of it. Calling T the height of the barometer in millibars at Newlyn, and N., S., E., W., the barometer heights at the four points mentioned, he finds that the most probable expression for the height of the sea at Newlyn is: a constant $- 13{\cdot}38$ T $+ 6{\cdot}07$ (E.–W.) $+ 0{\cdot}67$ (S.–N.), (other oscillations being eliminated). It is clear that at this port the E.–W. gradient is the important one, the N.–S. gradient having little effect. A prediction by this formula, for a month not used in the computations, showed an excellent resemblance between the two curves. But clearly this investigation does not take account of the larger oceanic effects.

Enough has now been said about mean-sea-level to show the kind of datum that is being dealt with. Mean-sea-level, undisturbed by meteorological variation, is the geoid—that is, the spheroidal surface modified by local attractions. The geoidal surface at any port may be some metres above or below the surface of the reference spheroid chosen for the national survey. But the value of mean-sea-level as a datum depends not on its height, but upon its stability; or, rather, as it is not stable from year to year, its value will largely depend upon the possibility of calculating its long-period oscillations.

It has been shown that mean-sea-level may vary from year to year by some inches, and that the probable variation of the height of any one year from the mean of a large number of years will be of the order of half an inch. It has also been shown that, making allowance for the "latitude variation" tide, and the 18·6 year tide, the remaining fluctuations are larger and almost certainly meteorological in character. And it may be taken as certain that the meteorological variations, which are of unknown phase and amplitude, are in part local, and in part world-wide, or oceanic. It follows, therefore, that, if it is desired to ascertain accurately the relative vertical movements of land and sea, with regard to Great Britain, it is very desirable that the three tide-gauges at Newlyn, Dunbar, and Felixstowe should be continued at work for many years to come.

II. Land-levels

In the operation of levelling, the observer looks along a line of sight which is tangent to the geoid; where there is no "local attraction" the line of sight will be tangent to the spheroid. The inclination of the two surfaces to each other will, in the ordinary case, be about a second or two of arc; but it may, in exceptional cases, amount to 30 seconds or more. The first point, therefore, to note is that levelled heights cannot conveniently be referred to the spheroid of reference in any national survey.

The next general consideration is that, even if it were possible to level along spheroidal surfaces, the spheroidal surface at sea-level is not parallel to a spheroidal surface raised above this datum, since the distance between these surfaces increases from the pole to the equator. In the Ordnance Survey report on the recent geodetic levelling, a good illustration of this fact is given, as follows:

Let us suppose that the lower levels of France were flooded so that the water just reached the base of the Eiffel Tower in Paris, and suppose that an exactly similar tower were built at

Marseilles so that its base was just touched by the water. Now suppose that the flood-level were raised so that the top of the tower at Marseilles were just covered, then there would still be 6 in. of the Eiffel Tower at Paris showing above the water. That is to say, that the two water surfaces, about 1,000 ft. apart, converge by about 6 in. in the length of France.

There are thus two distinct ways of measuring differences of level, one called the *orthometric* and the other the *dynamic* system. In the orthometric system the heights of the two Eiffel Towers would be described as equal; in the dynamic system the Paris tower would be described as 6 in. higher than the other. Again, the dynamic system may be used to denote equal increments of work at any two points in question, or equal increments of *height* at some standard parallel of latitude. It is, as a fact, of little importance which of the methods is used, provided that it is clearly stated which *is* used; dynamic heights are readily convertible into orthometric heights, and vice versa. But uncorrected levelled heights are neither orthometric nor dynamic.

The old primary levelling of Great Britain was executed between the years 1840 and 1860, so that it is, on an average, some seventy years old. In those days nothing was known of the distinction between orthometric and dynamic differences of height. In those days, also, very little care was taken to provide stable bench-marks, the marks being cut indifferently on houses, churches, walls, gate-posts, mile-stones, and generally on any convenient accessible structures. No distinction in the character of the marks was made between primary marks and those of less importance. It has been found, also, that during the course of the last seventy years a large proportion of the original marks have disappeared—more than half, in fact. Then, from a modern standpoint, the accuracy of the old work leaves much to be desired. It was, therefore, obviously desirable to execute a new level network of high precision, and this new geodetic levelling was carried out during the years 1912 to 1921. It will be seen that this period included the four years of the war, and it became necessary to reduce the programme to the minimum which would ensure a stable framework for the future.

This idea of a stable framework for future reference has been the principal preoccupation of those responsible for the work. And, in this connection, it was considered desirable to provide geodetic, or "fundamental," bench-marks, of a far more permanent character than had hitherto been made use of in any country. There are in England and Wales 86 of these new fundamental marks, their average distance apart being about 31 miles. Each mark is founded on solid rock, or chalk, below the soil and subsoil. The design of the mark and selection of

its site are intended to ensure that it shall not move unless the crust of the earth moves also. Mining areas and areas of clays and soft rocks have been avoided.

There is a class of error, which is the accompaniment of all levelling, known since 1837, and not yet satisfactorily explained. Mr. Bunt, who carried out some important levelling between Axmouth and the Bristol Channel in that year, observed that "with a few partial exceptions, the heights of all the points came out less by the levels returning than by the levels going," or, in Whewell's words, "in proceeding with the levelling operation, along a line which is really level, the further end constantly appears to be the lower end." This is known as the "systematic error," to distinguish it from the ordinary accidental errors of observation. In adjusting the errors of a level network it is of no small importance to decide properly how to treat the "systematic" error. As all precise levelling is carried out in both directions along each line, it is, as Mr. Jolly observes, important to distinguish between the systematic *discrepancy* between the backward and forward levellings and the systematic *error* in the mean of the two. A careful discussion of the discrepancies in all the routes of the new Ordnance Survey Levelling resulted in the conclusion that the so-called "systematic" errors should be treated as accidental and, in consequence, weights were given inversely proportional to the lengths of the lines.

To give an idea of the precision of the new work, the following figure is interesting: From the tidal station at Newlyn to the tidal station at Dunbar the distance is 641 miles by one route and 761 miles by the other levelled route; the probable error of the mean height of Dunbar, as determined from Newlyn through these two routes, is under 2 inches.

The probable error, per mile, of the new work is ·092 in., whereas the probable error of the old work (of which the mean date was 1850) was ·360 in. per mile; the new probable error is, thus, about one-quarter of the old. Such a result was only arrived at by the use of refined methods and greatly improved instruments; invar staves were used instead of wooden ones; a micrometer attachment on the levels enabled intersections to be made instead of estimations; the position of the bubble was read by means of a reflecting prism, so that the observer was not obliged to walk round the instrument; the length of the space between two staves was limited; carefully thought-out instructions were issued as to the order of the readings; and generally every precaution was taken that experience could indicate or forethought suggest.

III. Relative Vertical Movements of Land and Sea

If a network of levelling were carried over the surface of a country, if the reference-marks were permanent and stable, and if the operation were errorless, we should then be able, at any future period, to carry out a similar operation, using the same marks, for the purpose of ascertaining any displacements of the crust. But whilst the comparison would give us information as to any tilts or inclinations of the surface which might have occurred during the interval, it would not give definite evidence of vertical movements unless a satisfactory independent datum had been established. And if, as indicated above, a good value of mean-sea-level were chosen as the external datum, we should be able to determine the relative vertical movements of the land and sea, in addition to any tilts or contortions of the crust.

The earliest operation undertaken for this express purpose was that alluded to above, namely, the levelling carried out in 1837–8, under Whewell's direction, between Axmouth on the English Channel and East Quantockshead and Portishead on the Bristol Channel.

In 1915–17 the Ordnance Survey re-levelled this line, in order to ascertain if any changes of level had taken place during the elapsed 78 years. The mark at Portishead had disappeared, but the old copper bolts, in granite blocks, were found at Axmouth and at East Quantockshead. The distance, as the crow flies, between these two points is about 35 miles. Taking the new levelling, for this purpose, as errorless, and taking the Axmouth mark as the datum, it is found that the East Quantockshead mark has apparently risen 0·92 in. But, as a fact, this quantity is probably due chiefly to errors in the old work (which, however, was very good for its date). The probable error of the old work is about 0·4 in., so that an actual error of 0·9 in. would not be surprising. It must, therefore, be said that there is no evidence of any tilt of the land surface having taken place along this line, between the English Channel and the Bristol Channel, during the 78 years in question.

It might have been expected that a comparison of the heights of the old primary levelling of England and Wales (mean date 1850) with the new levelling (mean date 1916) would give evidence of any vertical movements of the surface that might have taken place during the 66 years' interval that separated the two operations.

The comparison has been made, and a map has been printed by the Ordnance Survey showing graphically the lines of equal discordance between the two levellings. Broadly speaking, if zero is taken in the neighbourhood of Liverpool, there is an

increasing discordance across England till the neighbourhood of Ipswich is reached; in this latter region the discordance amounts to no less than 21 in., the new values being lower than the old. If both operations had been errorless this result would imply that a tilt downwards of the surface of England had taken place from north-west to south-east.

But further investigation has shown that the facts will not bear this interpretation. The old levelling in the Eastern Counties was carried out in 1854, and as early as 1859 a discordance of 22 in., in the same direction, was discovered between mean-sea-level at Harwich and mean-sea-level derived from the levelling; and in 1896 a similar discordance of 20 in. in the same direction was discovered. In 1920 the old work at Felixstowe was found to differ, in the same direction, by 20·4 in. It is, therefore, abundantly clear that the differences between the results of the old and the new levelling are not due to vertical movements of the crust, but to errors in the old work.

So far, then, as the evidence of the levelling goes, it has not yet been found possible to detect any vertical movement, or tilt, of the surface of this country. Indeed, such evidence as there is is in favour of the hypothesis that there has been no measurable movement during the last sixty or seventy years.

It may be added that the statement, which has appeared for many years on the Ordnance Maps of Great Britain, that "altitudes are given in feet above the assumed mean level of the sea at Liverpool, which is 0.650 ft. below the general mean level of the sea," is incorrect. The old Liverpool datum happens to be about 1⅛ in. above the mean-sea-level at Newlyn and not very far from the general mean-sea-level. But the matter is unimportant, as the old Liverpool datum will disappear in favour of mean-sea-level at Newlyn.

With regard to the question of *general* mean-sea-level round our coasts, the following facts are of interest. Taking mean-sea-level at Newlyn for the six years, May 1916 to April 1921, as zero, the mean-sea-level at Felixstowe, July 1917 to April 1921, was found to be ½ in. lower, an insignificant amount when the length of levelling between these tide-gauges is considered. But the apparent difference of mean-sea-level between Newlyn and Dunbar is 9·7 in., the sea-level at Dunbar being the higher, the means in each case being those for the period May 1915–April 1921. The probable error of the levelling between the two places is about 2 in.; the difference between the mean heights of the barometer at Newlyn and Dunbar was 0·108 in., and this would raise the sea-level at Dunbar by 1·4 in. But there is still 8·3 in. of difference to be accounted for, more than is likely to be due to errors of levelling. The conclusion is that there is possibly a real difference of

mean-sea-level of some three or four inches, and this, if it exists, is probably due to meteorological causes. Prof. D'Arcy Thompson found the mean-tide-level at Aberdeen to be 4 in. above that at Dundee; but this comparison depends on the old levelling, which is itself probably some inches in error.

So far as Great Britain is concerned the whole matter may be summed up thus: Since the level of the sea is subject to long-period fluctuations it will be most desirable to keep the Ordnance Survey tide-gauges at work for many years to come, in order to watch the changes in our only available datum surface; the old levelling of 1840–60 is not reliable enough to base any conclusions upon with regard to variations of land-levels; the new levelling is of great accuracy and should afford a sure foundation for future discussions; there is possibly a real increase of a few inches in the height of the sea surface above the geoid from south to north, along the coast of England and Scotland; and, finally, there is no evidence that the levels of England have altered since the earliest exact observations (about 1840), such evidence as there is tending to show that, if there have been any vertical movements of the crust, they must have been very small. But fifty or a hundred years hence it ought to be possible to investigate these, and allied problems, with much greater accuracy than can be done at present.

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THE EVOLUTIONARY HISTORY OF THE VERTEBRATE LIMB

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Two of the main subdivisions of the animal kingdom—the Arthropoda and the Vertebrata—are above all characterised by the possession of limbs—movable projections of the body serving for its propulsion, and accordingly morphological students of these groups are faced by the insistent problem which deals with the evolutionary history of these limbs, their specialisation for different purposes, and above all their origin. It is the aim of this present paper to deal more particularly with this last question so far as regards the Vertebrata. It will give first an outline sketch of the two old-established theories of the origin of the vertebrate limb and will then outline the reasons which in the writer's opinion make it necessary to drop these older views and adopt one more in accord with present-day knowledge. What follows has regard mainly to the evolutionary origin of the vertebrate limb, and not to its subsequent specialisation; but it may be well, before proceeding further, to indicate certain general considerations to which the author attaches weight. He takes the view that in all probability the primitive Vertebrate, although aquatic, was not pelagic. He believes evolutionary progress to be a function of variety of environmental conditions. He believes that a pelagic existence—the most homogeneous of all types of environment—is that which is least of all favourable to the opening up of new evolutionary paths. He believes, on the other hand, that the marginal zone bordering the land—with its unequalled variety of environmental conditions—is the great nursery for new types of animal organism, and there he would locate the home of the original Vertebrates. From that zone he believes the Vertebrates have spread in two different directions—the one set becoming independent of the land and the solid bottom, becoming more and more highly specialised as swimmers, the others becoming increasingly independent of the water and increasingly specialised as land animals. A necessary corollary to these general considerations is the opinion that the pentadactyl limb of the land animal has not evolved

directly out of the paired fin of the fish, but that both of these two types of limb have evolved out of a primitive ancestral type by a gradual process of specialisation, in the one case for support and movement on a solid substratum, in the other case for swimming.

The Gill Septum Hypothesis.—The first of the current hypotheses as to the origin of the limbs is associated with the name of the great anatomist Gegenbaur. In studying the skeleton of the gill-region in Elasmobranch Fishes he was struck by a remarkable resemblance which that skeleton presented in certain cases to the skeleton of the paddle-like limb of the Australian Lung-fish *Ceratodus*. This thick, leaf-shaped limb has a skeleton of a very characteristic kind, consisting of a jointed rod of cartilage running down its centre like the mid-rib of a leaf, giving off on each side a series of jointed rays of cartilage, and attached at its inner end to the skeletal hoop embedded in the body-wall and known as the limb-girdle. Gegenbaur regarded this type of skeleton, which he termed the biserial archipterygium, as the most primitive of all the types of limb skeleton known to occur in the Vertebrata. As a matter of fact this form of skeleton, although to-day restricted in its typical form to the single genus *Ceratodus*, was, as the data of palæontology show, quite common in the early days of the evolutionary history of fish. If we trace back the Lung-fishes, or the Crossopterygian Ganoids, or the Elasmobranchs, to the palæozoic period of geological history we find that in each case the biserial archipterygium was then the normal type of limb-skeleton. If we take a broad view of the facts of palæontology, more especially when along with this we consider the physiological clumsiness of the *Ceratodus* limb as compared with the far more highly specialised paired fins of ordinary fish, there is in the present writer's opinion no escape from the acceptance of Gegenbaur's opinion that in so far as the skeleton of paired *fins* is concerned the biserial archipterygium is actually the most archaic type known to exist amongst present-day fish.

The gills by which a fish breathes are attached to a series of partitions or septa, separated by slits through which water is rhythmically swished out by the respiratory movements. Each septum is supported by an arch of cartilage along its inner margin, from which project a series of gill-rays passing outwards embedded in the thickness of the septum. Now Gegenbaur in the course of his study of these arrangements in Elasmobranch Fishes came across, particularly among Rays such as the Sawfish (*Pristis*), a departure from the normal in which a central gill-ray had undergone considerable enlargement and carried its neighbours attached to its basal portion in pinnate fashion, presenting an appearance very like an in-

cipient archipterygium of the *Ceratodus* type. Gegenbaur concluded that the biserial archipterygium had actually arisen in this way in evolution, and this conclusion involved the further conclusions that the vertebrate limb itself originated from a gill-septum, and that the shoulder girdle was originally a cartilaginous branchial arch.

Certain obvious objections to the acceptance of the Gegenbaur hypothesis as a true theory may be brushed aside as of no particular weight. Such is the absence from the palæontological record so far as we know it at present of any transitional stages between gill-septum and limb. Palæontology—the fragmentary knowledge of fragments of extinct animals—when it produces positive evidence is of the greatest importance in evolutionary discussion: when its evidence is merely negative it may be safely ignored.

So also with another very obvious objection to the Gegenbaur hypothesis—the fact that the limbs, more especially the hind limbs, are commonly far removed from the branchial region in which Gegenbaur believes them to have originated. As will be indicated later in this article, this particular objection, when subjected to critical examination, loses all the weight which on merely superficial consideration it appears to possess.

The most formidable difficulty in the way of accepting the Gegenbaur hypothesis is, as the present writer believes, neither of these: it is to be found in the general consideration that the forerunner of the limb, whatever it was, must have been freely movable and must have projected freely beyond the surface of the body, to have had in it the potentiality of evolving into a limb, whereas the gill-septa in all the more archaic and primitive fishes are flush with the general surface and firmly fixed in the thickness of the body-wall.

The Lateral Fin Hypothesis.—The second view as to the origin of the limbs is based upon certain similarities between the paired fins and the unpaired fins of fish. It is a fact established beyond dispute that the separate unpaired fins seen in many fishes have arisen in the course of evolution from an originally continuous fin which extended along the dorsal side round the tip of the tail on to the ventral side of the body. Embryological evidence demonstrates this beyond all question. The hypothesis now under discussion regards the paired fins of fish similarly as persisting parts of a, hypothetically, once present continuous fin which extended along each side of the body. This view was originated by the great embryologist Francis Balfour. In his work on the embryology of Elasmobranch Fishes he observed that the pectoral and pelvic fin rudiments were during an early stage of embryonic development continuous with one another, and argued that this fact could

bear only one interpretation, namely, "that the limbs are the remnants of continuous lateral fins."[1] Almost synchronously with Balfour, the American anatomist Thacher enunciated the same view, basing it upon his comparative anatomical work which brought out the exceedingly close similarity in adult structure between the unpaired and the paired fins of Sharks and Sturgeons—a similarity so close as, in his opinion, to point clearly to an identity in morphological nature of the two types of fin. "As the dorsal and anal fins were specialisations of the median folds of Amphioxus, so the paired fins were specialisations of the two lateral folds."[2]

Each of these bases on which the lateral fin hypothesis was founded by Balfour and Thacher respectively has been sapped and weakened by the advancement of knowledge. It turns out that the continuous ridge connecting the embryonic rudiments of the pectoral and pelvic limbs in the embryo Elasmobranch makes its appearance secondarily, being preceded by a period during which the rudiments are quite discontinuous. Further, it turns out that this continuous ridge, although it does occur in *Torpedo*, where Balfour observed it, is *not* characteristic of Elasmobranchs in general. It does not occur in the more primitive Sharks, and is therefore in all probability to be looked on as a secondary modification in the development of those Ray-like Elasmobranchs in which the pectoral fin has undergone an enormous extension along the side of the body.

The resemblances in adult structure between the paired and unpaired fins are certainly very striking, but they too carry far less weight in these days when we have learned to appreciate the extraordinary lengths to which homoplastic or convergent resemblance can go in organs constructed to carry out similar functions.

There is again, however, a general consideration, in this case physiological, which is, as the present writer believes, sufficient by itself to rule out the lateral fin theory. One of the most fundamental features in the structure of the vertebrate body is the fact that its muscular system consists of longitudinal fibres, arranged on each side of a central flexible and elastic rod, the notochord, and segmented up into blocks, the myotomes, arranged one behind the other. The physiological significance of this arrangement is clear: it is such that by the serial contraction of the myotomes from the head end backwards waves of lateral flexure can be passed tailwards along the body. From the fact that this arrangement of the muscular system—still clearly evident in the adult stage of fish—is present in the

[1] *Journ. Anat. and Phys.*, vol. xi (October 1876).
[2] *Trans. Conn. Acad.*, iii (February 1877).

embryo of every vertebrate we may, I believe, take it as certain that the primitive vertebrate was a swimmer, moving by flexure of the body, just as an eel or an Amphioxus moves to-day. This being so we may take it as overwhelmingly probable that the primitive vertebrate would *not* proceed to evolve a lateral fin, which in its early stages must not only be of very slight efficiency as a motive organ, but must be an actual hindrance to the already existing swimming mechanism. It has been suggested that the lateral fins were in their incipient stages not actual locomotive organs, but served either for balancing on the bottom or as "planes" or bilge-keels in shooting through the water. A very little consideration of the physical factors involved will show that neither of these possibilities deserves serious attention.

Each of the two evolutionary hypotheses that have just been outlined have had and still have important bodies of supporters. The great names of Gegenbaur and Balfour still necessarily carry much weight and influence, while those morphologists who maintain a robust faith in Amphioxus as representing more or less closely an ancestral type of vertebrate have naturally a leaning towards a theory whose supporters make use of this animal as Thacher does in developing the lateral fin hypothesis.

When the present writer first devoted himself to the general problems of vertebrate morphology he felt greatly attracted by the interest and elusiveness of this particular problem of the origin of the limbs. While, however, not wanting in respect for those who had gone before, he was soon led to the conviction that neither the Gegenbaur nor the Balfour-Thacher solution of the problem could be regarded as satisfactory in view of the new facts which had become known since their day. The question then arose: "If the vertebrate limb is derived neither from a gill-septum nor from a continuous lateral fin, from what did it arise; what type of organ was actually its evolutionary forerunner?"

The External Gill Hypothesis.—In recent studies on the embryology of some of the more archaic vertebrates special attention has been attracted by peculiar breathing organs known as external gills—not to be confounded with the filamentous prolongations of the gill-lamellæ occurring in many Elasmobranchs, and in a few Teleosts, which are occasionally given this name by careless writers. These organs have long been known as occurring in the larval stages of many Amphibia, where they form conspicuous organs projecting from the sides of the body just behind the head. Each projects from one of the branchial arches (Visceral Arches III, IV, and V most usually); it is commonly pinnate in form; it is richly supplied

by blood—the aortic arch being diverted out into it as a loop; and it is provided with muscles by which it can voluntarily be flicked backwards so as to change the water bathing its surface. Such external gills with their characteristic movements are well seen, and may be readily studied, in live Axolotls.

So long as these organs were known only in a single group of vertebrates it was natural to regard them as merely adaptive organs which had evolved within that group and accordingly not of any wider morphological importance. The whole position is altered, however, by the fact that the somewhat similar-looking structures occurring in the larvæ of existing Crossopterygians (*Polypterus* and *Calamichthys*), and of two out of the three surviving Lung-fish (*Lepidosiren* and *Protopterus*) are actually morphologically the same organs, agreeing precisely in their mode of development with the external gills of Amphibians. When we find the same morphological organ present in these three groups of relatively archaic vertebrates, the Amphibia, the Dipnoi and the Crossopterygii, the conclusion is unavoidable that they have come down from the common ancestors of these groups—in other words, we are forced to conclude that external gills were a possession of vertebrates in a very far back stage of their evolutionary history. It may be argued against this conclusion that there is no trace of true external gills in many of the lower vertebrates where on this view we might expect to find them. As a matter of fact, however, their absence in such cases does not constitute a serious difficulty, for observation shows that external gills are exceedingly liable to injury and on that account particularly liable to be eliminated in the course of evolution. Amongst the Amphibia many cases are known where they have been eliminated from the development of particular species or genera. And in the case of Elasmobranchs and other vertebrates with a large yolk-sac we have a further reason for the disappearance of the external gills, namely, that they have ceased to be necessary, owing to their breathing function having been taken over by the highly vascular surface of the yolk-sac.

External gills are known in connection with all six visceral arches. In certain Urodele Amphibians they are present on the first or mandibular arch in the modified form of simple leg-like "balancers" on which the young larva rests. In Crossopterygians they are present on the second or hyoid arch, in various Amphibians on arches III, IV, and V, and in the two Lung-fish on arches III, IV, V, and VI. It is plainly suggested by this fact that not only were external gills present in very early vertebrates, but that they were present on each visceral arch. But we may go further. It is generally agreed

that the short series of gill-clefts with their intervening arches met with in modern vertebrates is the remains of what was once a much longer series extending back along the sides of the body. But if so, the same would apply to the external gills—the presently existing ones being the last survivors of a more extensive series, the posterior members of which have disappeared. According to the external gill theory the vertebrate limbs are simply two pairs of these more posteriorly placed external gills which have been saved from disappearance by their having taken on a new function, and in correlation with this have undergone profound modification in form and structure.

The physiological *possibility* of external gills evolving into limbs has to be granted at once. Limbs are projections of the body used for support and propulsion. The balancers of Urodeles show us a pair of external gills actually converted into simple leg-like supports. Observations of the flicking movements of the external gills in the live animal at once shows how they could readily become organs of propulsion.

What, however, are the probabilities of this having actually happened? Certain obvious difficulties at once suggest themselves. There is the difficulty that the pelvic limbs as we see them in present-day vertebrates are far removed from the branchial region where on this theory they are supposed to have originated. This objection is seen to be devoid of weight if we inquire into the position of the limbs in different groups of vertebrates. Within the group of Amphibia, for example, we find that the position of the pelvic girdle fluctuates between the level of the 6th vertebra (*Hymenochirus*) and that of the 63rd (*Amphiuma*). In an ordinary Teleostean Fish like the Haddock we see that the pelvic limb has actually migrated so far forwards as to be in front of the pectoral, right under the animal's throat. The limbs of vertebrates have in fact in the course of evolution been shifted about on the body in the most surprising way, taking up in each case the position in which according to the form of body and method of movement they are most effective.

It has also been suggested that the characteristic absence of any supporting skeleton in the external gills is an argument against homologising them with the limbs. The present writer does not regard this difficulty as serious. He regards the stiffening of connective tissue to form skeleton as a phenomenon which is exceedingly apt to come about secondarily in response to mechanical strain. It would be therefore, in his view, entirely to be expected that the external gills as they came to be used as supporting or propelling organs would tend to develop supporting skeletons in their interior. Apart from

this consideration it has been found that in the bulky external gill of *Polypterus* there actually is developed a supporting axis of cartilage in its basal portion,[1] while there appear also to be remains of skeletal supports in the external gills of certain fossil Amphibians.

As regards the limb-girdle, all that need be said is that this is interpreted by the modern theory, as it was by that of Gegenbaur, as being simply the modified cartilaginous branchial arch.

It will be realised that the external gill hypothesis is in one important respect upon an entirely different footing from its two predecessors, inasmuch as it for the first time demonstrates the actual existence of an organ in the Vertebrata possessing the two essential qualifications for evolving into a limb, namely, free projection beyond the surface of the body and the possession of a special muscular equipment for producing the backward flexure necessary to give a forward push to the body.

It also falls into line with a general consideration the importance of which is apt not to be fully appreciated, namely, the extreme improbability of the primitive vertebrate with its beautiful adaptation to swimming by lateral flexure, as shown by the segmentation of its myotomes, starting to evolve a new *swimming* apparatus. It seems practically certain that the limb in its initial stages must have been an organ for producing a type of movement other than swimming.

The most weighty evidence for the truth of the external gill hypothesis is afforded by that in many respects archaic vertebrate *Lepidosiren*. In this animal the knob-like rudiment of the limb is in its early stages identical in appearances with the early stage of an external gill.[2] In later stages this resemblance is masked by the vascular filaments which grow out from the external gill, but if these are absent the resemblance becomes in these later stages still more striking. Budgett records [3] the case of a Protopterus—close ally of *Lepidosiren*—in which one of the external gills failed to develop its filaments and in which the " bare shaft so much resembled the pectoral limb that the larva appeared to have two pectoral limbs on one side."

This remarkable resemblance between early stages of limb and of external gill is by no means confined to Lung-fish. It is also very striking in various Urodele Amphibians.[4]

A unique peculiarity of *Lepidosiren* among vertebrates is that during the breeding season the pelvic limb actually takes

[1] Budgett, *Trans. Zool. Soc., London*, xvi. 1902.
[2] Keibels, *Normentafeln z. Entwicklungsgeschichte*, Heft x, figs. 25 *l*. and 31.
[3] *Trans. Zool. Soc., London*, xvi, August 1901.
[4] *Cf*. figs. in Ekman, *Morph. Jahrb.*, xlvii, 1913.

on the form and function of an external gill, richly vascular respiratory filaments sprouting from it precisely as in an ordinary external gill. Such a phenomenon would be in itself merely an extraordinary mystery, but the mysteriousness vanishes in face of the external gill theory, according to which it is interpretable as merely a temporary reversion of the limb to its primitive ancestral condition. The probability that this is a correct explanation is enormously strengthened by the fact that the pectoral fin also frequently shows distinct traces of the assumption of the gill character,[1] though in this case the modification is usually so slight as to be of no appreciable functional significance.

According to this theory the vertebrate limb was in the early phases of its evolution a simple styliform structure, adapted for pushing against solid objects as its owner clambered about among the vegetation of its environment. As time went on the primitive vertebrates spread from their swampy home in two directions. Some, venturing farther and farther from the solid ground, took to a free-swimming existence: their bodies assumed the stream-line form, their lung became specialised as a float, their limbs became paddle-like—passing through the clumsy archipterygial form seen to-day in *Ceratodus* and reaching eventually the thin flattened form, with its fine spread-out skeletal supports and its muscular mechanism concentrated entirely at its base, that we see in the highly evolved fish of to-day.

The other vertebrates which forsook their ancestral swampy home made for the dry land. Their skin, losing its heavy plates of bone and its elaborate glandular mechanism for the reduction of skin friction, became specialised for the diminution of evaporation and later of loss of heat from the surface of the body; and the limbs became the legs characteristic of the Tetrapoda, or Land-animals. It is fascinating to watch a young *Lepidosiren* as it clambers about, and to see how its simple styliform hind-limb, as it pushes itself forwards, becomes sharply bent at two levels exactly corresponding to those breaks in continuity of the skeleton which we call the knee and the ankle joints of the typical Tetrapod, while the tip, becoming slightly flattened by pressure, simulates the beginning of a foot. It is indeed difficult for the observer of this performance to resist the vivid impression that he is witnessing the repetition of what occurred normally amongst vertebrates in the far-back days when the external gill was still in the course of its evolution into the tetrapod limb.

In conclusion, it should not be forgotten that the external gill theory frees vertebrate morphology once for all from an

[1] Agar, *Anat. Anzeiger*, xxxiii, 1908.

unpleasant bugbear in the form of the problem of correlating the various skeletal elements of the pentadactyl limb with those of the paired fin of the fish. Morphologists, dominated by the crude generalisation that land animals have evolved from aquatic, have found themselves saddled with the belief that the tetrapod limb is necessarily an evolutionary derivative of the paired fin, and accordingly have had to attempt to determine the homologies between the skeletal elements in the two cases. How thoroughly unsatisfactory these attempts have been is realised by most of those who have troubled to go into the matter critically.

POPULAR SCIENCE

YELLOW FREEBOOTERS

By HERBERT MACE, F.E.S.

I

WHETHER regarded from the older, almost obsolete view-point, of separate creation of species, or that of gradual evolution, the parallel lines upon which bees and wasps run, are very remarkable.

By common consent, three groups stand at the head of the hymenopterous order—ants, bees, and wasps—and it would be difficult to say which of the three is the most highly developed. Granting a common ancestral form, the divergence of these three main lines, is almost certainly based on the nature of their food, for all agree on another fundamental matter, that of taking immense pains to ensure the welfare of their young. Indeed, this feature is common to the whole order, for the less highly developed, take much more precaution for the safety of their offspring, than insects of any other order. The saw-flies cut slits in leaves and place their eggs in them, and the gall-flies, by puncturing a stem or leaf, depositing their eggs at the same time, cause a swelling, sometimes of monstrous size, in the centre of which the larvæ feed in safety amidst abundance of food.

These lower families feed on living vegetable matter, and in this respect they differ profoundly from the three groups at the head of the order. Fossil remains of Hymenoptera are extremelyscanty, and afford no clue to the nature of the ancestral form at the root of these three lines; but if we adopt the not unreasonable view, that it was an active insect of almost omnivorous habits, we have a very good starting-point for the three divergent groups. The ants—it is singular that almost the only hymenopterous fossil known is regarded as an ant—may be considered as having followed in the direct line, for they are as nearly omnivorous as any race of animals could be. The bees and wasps have diverged on each side, the bees

ultimately adopting flower pollen as their sole nitrogenous food, and the wasps devoting their attention to animal matter.

It is quite easy to understand, that constant devotion to a special type of food would, in the course of ages, cause great modification of structure and habit. The development of special apparatus for collecting and conveying pollen, is alone responsible for most striking differences between the anatomy of bees and wasps. It is, for example, a distinct advantage for bees to be hairy, as hairs assist greatly in the collection of pollen, and many species appear to take no special pains to gather pollen, but merely scrape off that which accidentally adheres to their hair, when they need it. Wasps are, however, almost entirely naked, and their active, predatory habits, are certainly better served by clean limbs and bodies.

In another sense the ants have pursued a direct path in their upward progress, for all the species live either in the ground or in decaying trees, excavating more or less elaborate galleries therein, while the bees and wasps have, at various stages in their progress, branched off into experimental directions, some of which have proved satisfactory and permanent.

II

As in the case of bees, the insects popularly called wasps, are divided into two families by structural differences. The Vespidæ, which include all the social species and some of the solitary ones, are distinguished by a striking feature of the forewings, which are capable of being folded longitudinally down the middle. Wing folding is not uncommon among insects, for the earwigs and beetles can double their flight-wings into several folds and pack them away, but the Vespidæ are the only *Hymenoptera* which are able to do so. The feature is a distinct advantage, for it enables the wings to be very much more out of the way when the insect enters its narrow cells. The mandibles, or biting jaws, are more prominent and stronger than in bees, but the maxillæ and labium cannot be made to co-operate in the formation of a pumping apparatus. As to outward appearance, the Vespidæ, very numerous though they are in species, and widely differing in habitat, are remarkably uniform, their livery of black and bright yellow, marking them out almost at a glance, and being rather noteworthy when we remember how much even one individual species of bee may vary in colour in different districts. The other family, Sphegidæ or Crabronidæ, comprises solitary species only. Their habits do not greatly differ from those of the solitary Vespidæ, and the chief structural differences are, inability to fold the forewings, and the absence of prolongation of the prothorax to the base

of the forewings, which is a marked character of the Vespidæ. Many of them wear the same black and yellow coat as the typical family, but there are a large number of red, brown, and even blue marked species.

The wasps have not advanced so far in specialisation as the bees, for there are no species in which the female is devoted solely to egg production, while the colonies are annual and are founded by a solitary female. In this respect, they correspond closely with the bumble-bees, but their engineering capacity, and the size of their colonies, are far ahead of the bumble-bees. The most advanced of the social species, are the members of the genus *Vespa*, and a description of the commonest species, *Vespa vulgaris*, the wasp which, by its persistent attacks on our sweets, our fruit and meat, and the fearlessness with which it ventures into our more or less perturbed presence, is a very great nuisance to us, will give a very good idea of the extraordinarily painstaking and highly ingenious labours, of this wonderful race.

Generally speaking, the common wasp makes its nest in a hole in the ground, but will sometimes select a hollow tree, or even build under the false roof of a house or outbuilding. In the more general case, the deserted home of some burrowing animal is selected, usually with a tunnelled entrance a foot or more long, and almost always in a bank or similar situation which is thoroughly dry and not likely to be much affected by heavy rains. The bank of a river, especially if it has been artificially made up by dredgings from the stream, is a very favourite place and, during the past summer, I located no less than forty nests, along about a mile length of the river Stort, whose banks are of this nature.

Having found a suitable chamber, the wasp first clears away any loose rubbish and, if necessary, enlarges the chamber somewhat. Commencing her work at the top, she attaches a stalk strong enough to support three cells. Both stalk and cells are constructed of the familiar paper, which, in the case of this species, is made of sound wood scraped from posts or the abraded surface of a tree trunk, and masticated with the saliva, which is extremely adhesive, dries hard, and forms an exceedingly tough compound with the fibrous material. The three cells hang downwards, and the entrances of wasp cells, at any rate in this genus, are always below, while the upper side is smooth, and forms, when the nest is in an advanced stage, a very suitable platform for the insects to move about on. Above the three cells, the insect next places a covering of the paper material. There are several layers of this, and they are not made adherent, but a small air-space is left between them. In some cases there are as many as sixteen of these paper coats, and, as a means of

conserving the warmth of the nest and preventing any soakage of rain from above, it is very effective. The wasp next deposits an egg in each cell, and then proceeds to construct other cells adjacent to the first, placing an egg in each as it is made. As the cells increase in number beyond the capacity of the original stalk to support, another stalk is attached to the roof, while over every additional cell the dome-like covering is extended to protect it. As soon as the first eggs hatch, the wasp desists from further cell construction for the time, and devotes her attention to the young. She collects honey from flowers, and insects, preferably aphis or tender caterpillars. Neither then nor at any time is food stored in cells, as in the case of bees. Normally speaking, it would, of course, be out of the question to store animal matter for any length of time, and it is somewhat singular that the habit of many of the solitary wasps, of paralysing their captives with poison and placing them alive in the cells, for the larva to consume as it requires them, has not extended to the social species ; for the presence of a reserve of food, would make a very greatdeal of difference to a wasp colony in some seasons. Daily foraging for fresh food is, therefore, very necessary, and unless the weather is consistently fine, many incipient wasp colonies must certainly perish owing to the inability of the mother to secure supplies daily. As well as collecting food, the wasp must also prepare a certain amount of pulp in order to lengthen the cells, for these are never raised above the height of the larva. Their original depth is about one-tenth of an inch, and a daily addition is necessary to keep them at the required level. As the larvæ grow, they become more hungry, and wave their heads impatiently above the cell, until their ration is given them. It should be noted that each cell is treated as a separate unit in construction, the wasp not having learnt, like the bee, that a partition wall is adequate for two cells. The shape is hexagonal, however, and economy of material is scarcely less perfect than in the case of the bee. No cap is placed over the cell by the mother, but when the larva has reached maturity it spins a strong silken cocoon, which is attached to the upper edge of the cell and serves the purpose of a cap.

III

Within a very short time of completing their metamorphosis, the young wasps set to work in the nest, assisting in feeding the later arrivals, and in a very little while, going on expeditions in search of food and material for cell construction. When the first comb has almost reached its full size, by which time the mother has a fairly large band of assistants, a second comb is begun beneath it, being attached to the first by columns, as the

first was to the roof. The upper side of the second comb is about half an inch below the cell mouth of the one above. At intervals, spaces are left between the cells to facilitate communication with the upper comb. As many as fifty paper columns may be used to support a single comb, especially in the centre of the nest, where the combs are usually wider than those above and below. The size ultimately attained by the nest, depends on the favourableness of the conditions. I have seen one with as many as eleven combs, the total circumference outside the protective covering being over five feet. As each successive comb is built, the paper coating is carried down the walls of the chamber, until the nest finally takes the form of a large oval body, with one or more holes at the bottom for the passage of the inhabitants.

The reproduction of the various sexes in the social wasps is not very dissimilar to that in the bumble-bees, though to some extent observation of this point is replaced by logical analogy with the conditions in the beehive, which have been more accurately observed. The first eggs laid by the queen produce workers, as do by far the larger proportion of the eggs laid throughout the season. Among the earlier hatched wasps there appear, however, a few small females, and these take part in the work of the nest, but also lay eggs, which, being unfertilised, produce only male wasps. Later on, larger females appear, and these are, at the end of the season, mated to males in the open, as in the case of bees. There seems little room for doubt, that the production of perfect females is brought about in the same way as in the beehive—that is, by special feeding of certain individuals. It has not been established, as in the case of bees, that specially digested food is supplied to the infant larvæ, but it is undoubtedly the case that the younger ones are fed with a greater proportion of sugary matter, while the older larvæ are given what can only be described as considerable chunks of flesh to consume. To satisfy the enormous demand which arises within the nest towards the end of summer, wasps are driven to resort to other places than the nectaries of flowers. These are gradually becoming less, and with few exceptions, there is no bountiful supply of blossom after the end of July. The bees have, by this time, filled their combs to overflowing and are almost idle, while the wasps, with no stores whatever on hand, have a larger population than ever to provide for. Ripe fruit takes the place of honey to some extent, and the wasp is a great plunderer of orchards, plums and pears being its favourites. Some have declared that bees also attack fruit, but although they may occasionally be found sucking the juice of a plum, it is almost certain that they never actually cut open a fruit, but are only attracted by the juice

exuding from one which has already been opened by wasps. The latter, of course, have no difficulty whatever in cutting the rind of a fruit. Jaws which can remove flakes of wood from hard oak posts, are not likely to be baulked by the skin of a plum or apple, and when wasps are abundant a plum-tree will soon be denuded of its crop by them.

Ample supplies are often readily obtained from the housewife's preserves, grocers' shops, and jam factories, while for the fleshy portion of their diet, any tender meat will be seized upon as readily as insects. Ham is a great favourite, and I have known wasps cut a hole clean through a thick ham in the course of a few hours. The fresh carcases of animals, such as rabbits or birds, which have been shot and overlooked, are soon cleared away, and I have seen wasps busy at a gamekeeper's gibbet removing all the flesh from a newly hung stoat.

A much richer store is available if there exist spirits brave enough to steal it—the concentrated essence of nectarous flowers, packed closely in the snowy combs of the beehive. This, however, is hidden away in a dark chamber, as difficult to enter as the home of the wasp itself, and a determined body of sentinels stands alert and ready at the portal, challenging every individual which attempts to enter. The wasp is soon drawn to the spot, however. It is too cunning to make a frontal attack on the citadel, but cautiously hovers about, inspecting it from every side. Not infrequently it succeeds in discovering some crevice at the rear which it can crawl through. Badly made hives or carelessly replaced coverings afford such opportunities, and at this time of year, beekeepers should be very careful to cover up their hives very securely after handling them. Finding no such back entrance, the wasp does not give up, but gradually approaches the doorway by oblique methods, keeping a wary eye open, for any signs of hostility on the part of the sentries. I have never seen a scouting wasp attempt to attack a sentry in the first instance, but sooner or later one of them will espy the invader and launch itself upon it, grappling with the enemy with resolute fury. In the majority of cases, the wasp succeeds in killing this first champion, and makes another attempt to approach the entrance. It may kill two or three bees before it is driven off. Sometimes it is the loser in the first combat, and if it persists long enough it will sooner or later be killed, owing to having used up all its ammunition. A well-defended hive is seldom very vulnerable to the attacks of wasps, though here and there one will manage to slip in and pilfer, especially in the cool of the evening, after the sentries have been withdrawn. It is noteworthy that wasps are better able to endure cold than bees, and are generally abroad much later in the day.

Should there be any real weakness in the defences, and one or two members of the scouting party succeed in entering, they will take away as much as they can carry and pass the news on to their comrades, who come in increasing numbers until the bees are completely overpowered, their stores removed, and their larvæ dragged out of their cradles to form food for those of the wasp.

Even if the wasp cannot succeed in getting inside a hive, its neighbourhood will still afford a happy and profitable hunting-ground. The more prosperous the colony the better. However late in the season it may be, there are always some members of the colony foraging successfully and bringing home more or less weighty supplies of honey. Some of these bees are so tired when they arrive that, instead of alighting on the entrance step and passing swiftly in, they pause to take breath on the ground before it. Such opportunities as this are eagerly seized by the waiting wasp, who pounces quickly down upon the luckless voyager, severs its body between thorax and abdomen, and bears away honey and meat in one vessel.

IV

The lack of a habit of storing, even in such a temporary manner as that of the bumble-bees, is a great handicap to the wasp, who is much more at the mercy of the weather than the bee. A few days' bad weather in summer, can be tided over in the beehive owing to the reserve supplies it contains, but even one day which is hopeless for foraging, causes much mischief in the vespiary. A week of rainy weather in July or August will reduce a wasp colony to the verge of starvation. At the best, it hinders the development of the nest, and causes a swift decline in population. For this reason, wasps are never abundant in changeable or wet seasons, but in long fine summers they thrive amazingly. The vespiary continues to grow, down to the end of September. By this time, the large females and a considerable number of males have appeared, and these are mated in due course. In a fine autumn the colonies linger on, plundering here and there, but to a large extent marking time by visiting the late blooming flowers, such as figwort, a waterside plant of which they are extremely fond. When sharp weather arrives, the fertilised females seek out hibernating quarters, choosing some covered place inaccessible to insect-eating birds. Such hibernating queens may often be found beneath the tiles of an outhouse, or under some old boards or sacks piled up. They will always be found lying in a singular attitude, the wings folded up round the head in most unnatural fashion.

The workers left in the nest now set about a singular task,

dragging out the remaining larvæ from their cells, and killing them, as though to spare them the agony of slow starvation, after which they also fall victims to the cold nights of autumn.

A populous colony of wasps may contain as many as thirty thousand individuals, and fifteen to twenty thousand cells is the usual capacity of a normal nest. In this great population, the workers are, of course, by far the most numerous, but the other sexes are more abundant than is usual in the beehive. The small unfertilised females, which lay eggs producing males only, are fairly numerous, but as they work in the construction of the nest, there is no wastage in their free production. Males are also fairly numerous. These are about twice the size of workers, and they are not indolent like the drones of the hive, for although they do not take any part in building the comb, or foraging, they act as scavengers, removing waste matter, dead bodies, etc., from the nest. The large females, generally known as queen-wasps, are equal in weight to six workers, and they are produced much more freely than in the case of bees, as many as seven hundred of the large cells they occupy as larvæ, having been counted in a single nest. The great risks they run during the rigours of winter and the early spring, when they have to start the nest single-handed, are sufficient warrant for their free production, for probably not more than one in a hundred succeeds in establishing a new colony.

The workers are not so much a race apart as in the beehive, for females take an equal part in the same kind of labours ; but when the nest is in full working order, workers are the only foragers, the males and females remaining in the nest till mating time arrives. Réaumur gives an interesting account of the general behaviour of the workers towards their fellows. On returning from a foraging expedition, he says, they reserve some of their spoils for the use of the larvæ and divide the remainder with great impartiality among the males and females. There is no compulsion on the part of the others, but a voluntary distribution amongst those which gather round the forager.

Another instance of co-operation is that when an object has to be moved which is too heavy for one wasp, two will share the burden. This is paralleled among the ants, but is rarely or never seen among bees.

Still more remarkable is the common practice of wasps of cutting off portions of any carcase they desire to remove. One writer relates an exceedingly interesting instance of this. He saw a wasp on the ground with a fly as large as itself. Being unable to lift it entire, it cut off the head and abdomen and started off with the central portion, but a light breeze was blowing, and this, acting upon the outspread wings of the fly, caused it to revolve and become unmanageable, whereupon the

wasp returned to earth, cut off the fly's wings and was thus able to carry the booty away.

The practice of cutting off the honey-laden abdomen of a bee, which I have witnessed a score of times, I have already remarked upon; but such an instance as the above may well cause the observer to inquire whether it is not a clear case of reasoning. I remember once seeing a large species of ant running along with the leg of a locust, which, although it was many times larger than itself, it seemed to manage with reasonable ease. To see what it would do, I caught hold of the thinner end of the leg and held it firmly. The ant tugged hard for a few moments and then, finding the leg immovable, ran round it several times, in order to see what the impediment was. Having satisfied itself that the leg could not be extricated, the insect went to the central joint, between the fleshy thigh and the shank, and, cutting through this, bore off the more valuable portion with very little delay.

V

So far as their general habits are concerned, the other species of *Vespa* agree very closely with that of the typical species, but there is considerable diversity, both in the situation and form of the nest. The hornet, *Vespa crabro*, generally builds in hollow trees, and the paper of which its nest is made is much coarser and rougher. It is often made of decaying wood or bark, which the common wasp never or very seldom uses, and the different colours of the barks give the nest a variegated appearance. *Vespa sylvestris* suspends its nest in the branches of a tree, making a very compact and smooth outer case, in which there is only one hole at the bottom.

The nearest relatives to the genus *Vespa*, are the species of *Polistes*, the typical one of which is *P. gallica*, a common insect in the South of Europe. In appearance, it differs little from *V. vulgaris*. Its colonies are very much smaller, about forty to sixty cells being the usual number found in the single comb, which is made in one circular piece about three inches across. It is attached to a branch of a low bush, is destitute of covering, and the cells are not vertical, but horizontal, with a slight upward tendency towards the mouth. Another of this genus, *P. nidulans*, of tropical habitat, constructs the most perfect cardboard imaginable. The outer case of this nest is somewhat bell-shaped and is affixed by a stalk at the upper end. The material is white, extremely tough and smooth, and quite impervious to rain. The cell combs inside this covering are disposed somewhat like those of the common wasp, but are rather concave. No columns are used

to support them, the sides being attached to the interior of the bell, while a single central hole affords access to each comb.

This form of nest is common to the genus *Polybia*, but the various species differ in little niceties of construction. *P. sedula*, for example, instead of making a hole in each comb, makes one through the side-wall between each comb, thus separating the nest into distinct non-communicating compartments. Some species save the necessity of building supporting pillars, by making each comb round a branch, which thus runs through the centre of the completed nest.

In all these cases, the nest is commenced at the top, a single comb being built and the bell-like covering extended downwards as each successive comb is added.

NOTES

Payment by Results in Medical Research.

We were glad to see by a press message from Montreal dated June 27 last that the Canadian Government are giving an annuity of $7,500 to Dr. Banting, of Toronto, for his discovery regarding the insulin treatment of diabetes, which already promises to be very useful in that distressing and common malady. We hope that the money is a personal reward to him for his brilliant work and not a mere subsidy for laboratory expenses. If so, the virile people of our great Dominion have again asserted the honourable principle of payment for medical discoveries which was first admitted by the British Parliament in 1802 in the case of Edward Jenner, the discoverer of vaccination against smallpox; which has always been maintained in these pages; but which was recently and unwisely repudiated by our politicians (Science Progress, 1915, vol. x, p. 315; 1920, vol. xiv, p. 635; 1920, vol. xv, pp. 113, 285; and 1921, vol. xvi, p. 286).

Advances in medical and sanitary science and practice may be classed in three groups, namely: (1) Those that are made by persons specially employed and paid for investigation; (2) those which enhance remunerative private practice; and (3) those which bring no pecuniary reward to the men who achieve them. Good work is doubtless done under all these headings; but the greatest medical discoveries, such as those of Harvey, Jenner, Pasteur, Küchenmeister, Koch, Laveran, and Manson, belong to the third class, and were made by private enthusiasts who followed some clue which appeared strong to them (but which would certainly have been rejected by committees of management); and who, without subsidies and often at considerable cost to themselves, revolutionised our medical knowledge. It is for this class that payment by results is especially demanded. Thus the studies of Harvey and Jenner ruined their medical practice, and the latter was justly rewarded by Parliament in view of this fact. Koch was a country doctor when he discovered the bacteria of tubercle and anthrax, but was wisely compensated by the German Government with a large payment and an Institute. On the other hand, Harvey and Reed (who found how yellow-fever is carried) were allowed to die in comparative poverty.

It is only fair that professional men who give up their chances of lucrative medical practice in order to make recondite investigations for the public good should receive some compensation when they succeed. But, more than this, it is to the public interest that as many men as possible should be encouraged to follow their example. Not only compensation, but rewards should be offered. Up to the present everyone (at least in this country) knows that medical research is in most cases merely an *ignis fatuus* which leads into the bog of poverty. Out of our 30,000 doctors, how many are trying to follow in the footsteps of Harvey and Jenner and are seriously setting themselves to solve any one of the intricate medical problems which still confront us? They cannot afford to do so. Many attempt, and a few succeed in making, improvements, sometimes considerable ones, in clinical practice (class 2), or work at routine subjects for small salaries in laboratories (class 1). But we still continue to die of cancer, tubercle, and pneumonia; thousands of our children perish of measles and whooping-cough; and we are still racked by rheumatism and a hundred obscure complaints. Why? The answer is that no one is going to spend his lifetime studying such difficult problems, with a thousand-to-one chance of failure, when he knows that even in the remote event of success he will get nothing for his pains? Even such honours as he may obtain will be less than those which he can easily achieve by an assiduous cultivation of the bedside manner or the arts of self-service! Unless the public take to paying honourably for medical or sanitary benefits already received they will, we fear, continue to suffer miserably from many diseases which would probably be easily preventable or curable if only we knew how to prevent or cure them; the best men will not help them.

Apparently this country is now spending at least £200,000 a year on subsidised medical researches of class 1, in the forms of government grants, university grants, scholarships, and institutional and commercial investigations; and the results are often wrong, incomplete, or useless. On the other hand, the country offers or gives nothing at all for approved discoveries, even of the first importance, already made by private persons without any public expense at all! Surely there is some fundamental economic error in this kind of bureaucratic administration.

The truth is that in this country the management of medical matters, including research, is almost entirely in the hands of persons who have themselves added little or nothing to our knowledge of disease, but who yet presume to control the investigations of others. Great vested interests, and indeed monopolies, in "research" have been created and large sums

are being expended upon it ; but we think that the time has come to reconsider the principles under which the money is now being used. Are they quite sound ?

A Scientific Romance.

Although the English-speaking nations have produced many men of great intellectual capacity, perhaps, indeed, the leading men of this class, yet we really cannot say that the mass of the people are at all intellectually inclined. Their chief interests appear to be money-making and game-playing, both of which occupy most of the thoughts of the masses. One has only to open the papers to understand how very little mere science, or indeed mere art, disturbs their more pleasant engagements. Occasionally, however, the sun of science does succeed in gleaming through the more sordid clouds of baser preoccupation, and sometimes we even reach the astounding phenomenon of a scientific novel. Many of the romances of H. G. Wells have probably done more to stimulate interest in science than all the publications of our learned societies—though we fear that Mr. Wells himself has received very little thanks for his services in this line to our goddess. Now we are delighted to receive a romance called *Atoms*, by Messers. D. C. Wignall and G. D. Knox (London: Mills & Boon). Here the public will find some of the wholesome powder of physics combined with the jam of a stirring tale. Apart from the inevitable love-story, the great man of the book is Mr. John Alexander Grant, who discovered a feasible scheme for " transmitting power wirelessly " and who is now attacking the problem of obtaining practical results from atomic energy. Several great financiers wish to get hold of him in order to " corner " all the world's production of energy. Needless to say, one of these financiers is a villain, while the other, we gather from internal evidence, may easily become so. But the latter employs Grant and gives him a really wonderful laboratory, the description of which will make the mouths of all physicists and men of science feel as if they had been taking pilocarpine. The scene is laid in Paris. The wicked financier wishes to suppress the other one and attempts to assassinate Grant. Fortunately this gentleman (who, of course, has a beautiful daughter) escapes and discovers (all within two or three months) how to extract out of the jealous pocket of nature a new kind of super-radium which places atomic energy upon the altar of humanity. The machinations and counter-machinations described nearly result in an awful tragedy which would have destroyed the whole of Paris, not to say the world, but for—— We wish a large sale for this bold book, and recommend it particularly to the attention of science masters, who will find it of assistance in their teaching.

Notes and News.

The honours lists published during the past quarter included the following awards to scientific men :—*Baronets :* Sir A. A. Bowlby, President of the Royal College of Surgeons, and Sir Thomas J. Horder, M.D. *Knights :* Dr. G. F. Blacker, Dean of University College Hospital Medical School ; Prof. W. M. Flinders Petrie, Edwards Professor of Egyptology, University College, London ; Mr. G. H. Knibbs, Director of the Bureau of Science and Industry, Australia ; Dr. H. W. G. Mackenzie, Senior Censor, Royal College of Physicians ; and Prof. W. J. R. Simpson, Professor of Hygiene, King's College, London. *C.I.E.:* Mr. J. Evershed, retiring Director of the Kodaikanal and Madras Observatories. *M.B.E.:* Mr. R. Ward, Superintendent of the Botanic Gardens, British Guiana.

The Albert Medal of the Royal Society of Arts has, this year, been awarded in duplicate to Sir David Bruce and Sir Ronald Ross.

Dr. F. A. Bather, of the British Museum, and Prof. F. O. Bower, of Glasgow, have been elected honorary foreign members of the Royal Danish Academy.

The Helmholtz gold medal, awarded once every ten years by an international committee for the most significant research in the domain of optics, has been given to Prof. K. von Hess, of Munich, for his investigations on colour vision.

During his visit to England last spring Prof. H. A. Lorentz received the Doctor of Science degree from the University of Cambridge.

Among the names of the scientific workers whose death has been announced during the last quarter were the following: Prof. J. Cox, physicist, of McGill University, Montreal; Dr. H. Goldschmidt, technical chemist, and inventor of the thermit process for welding steel; Prof. G. L. Goodale, botanist, of Harvard; Sir H. H. Howorth, a trustee of the British Museum; Prof. Immelmann, general secretary of the German Röntgen Society; General E. A. Lenfant (African explorer); Dr. A. Looss, helminthologist; Prof. C. Niven, physicist, of Aberdeen; Captain C. H. Ryder, Director of the Danish Meteorological Service; Mr. M. de C. S. Salter, Superintendent of the British Rainfall Association.

Dr. A. Russell has been elected President of the Institution of Electrical Engineers; Sir Oliver Lodge, President of the Röntgen Society; Dr. A. B. Rendle of the Linnean Society, and Dr. E. F. Armstrong of the Society of Chemical Industry.

The last few months have been notable for a number of retirements of men occupying prominent positions in the world of science. Among them are Dr. G. E. Hale, Mr. J. Evershed, and Dr. S. W. Stratton. Dr. Hale retires on account of his health, and is succeeded in his directorship of Mount Wilson Observatory by Dr. W. S. Adams. He will, however, still control the general policy of the Observatory. Dr. T. Royds succeeds Mr. Evershed at Kodaikanal and Madras, and Dr. G. K. Burgess is now in charge of the Bureau of Standards, Washington.

Prof. A. Smithells, who recently vacated the chair of Chemistry in Leeds University, has been appointed director of Salter's Institute of Industrial Chemistry.

Prof. Sir W. Herdman has given £20,000 to Liverpool University in order to provide new buildings for the geology department and as a memorial to the late Lady Herdman. Edinburgh and Sheffield Universities have each received anonymous donations of £20,000. In the former case the gift is intended to serve as the nucleus of a fund for a new department of zoology, and at Sheffield to provide one undergraduate scholarship in pure science and a number of post-graduate scholarships for research in metallurgy.

The Times of June 22, 1923, publishes a public appeal signed by Mr. Asquith, Lords Dunraven, Hardinge, Lansdowne, and Leverhulme, and by Sir Anthony Bowlby, J. Bordet, Dr. Hugh S. Cumming, Sir Arbuthnot Lane, Sir Humphrey Davy Rolleston, Dr. Philippo Rho, Sir West Ridgeway, Dr. P. P. E. Roux, Sir Charles Sherrington, Professor Richard Strong, Dr. W. H. Welch, Sir Alfred Yarrow, and other gentlemen, for a "Ross Institute" to commemorate the twenty-fifth anniversary of the discovery of the mosquito-transmission of malaria. The President is to be Viscount Leverhulme, and the Honorary Treasurer is Lord Willoughby de Broke, address 56 Queen Anne Street, W.1.

We are glad to report that our valued contributor, Mr. H. Spencer Jones, of the Royal Observatory, Greenwich, has been appointed Astronomer Royal at the Cape.

The Council of the Royal Society has decided to use the greater part of Sir Alfred Yarrow's gift of £100,000 to found a number of professorships for

men of established reputation as research workers, who will be expected to devote the whole of their time to research. In this way the Society provides for research as a definite profession, freed from the tiresome interruptions which inevitably handicap the conscientious college professor.

The Senior Studentships given by the Commissioners for the Exhibition of 1851 have, this year, been awarded as follows: Dr. W. Davies, chemist, University of Oxford; Dr. L. C. Jackson, 1851 science research scholar in Physics, University of Leyden; Mr. J. H. Quastel, biochemist, University of Cambridge; Mr. D. Stockdale, metallurgist, University of Cambridge; Mr. H. Williams, geologist, University of Liverpool.

The Ramsay Memorial Fellowship Trustees announce the following elections and re-elections to Fellowships for the session 1923–4: British Fellowships to Mr. S. Coffey and Dr. R. W. Lunt, for research at University College, London, and to A. F. Titley, of Bristol and Oxford; Glasgow Fellowships to Mr. T. S. Stevens and Mr. J. A. Mair. All these Fellowships are of the value of £300. Norwegian Fellowship of 5,400 kroner to Mr. Gunnar Weidsmann to work under Prof. Gowland Hopkins at Cambridge; Danish Fellowship, £300, to Mr. K. Højandahl, who will continue to work at Liverpool; Netherlands Fellowship, £300, to Mr. J. Kalff, of Amsterdam; French Fellowship, £100 plus 14,000 francs, to Dr. H. Weiss, who will work under Sir. W. Bragg at the Davy Faraday Laboratory. Appointments to the Canadian, Greek, Italian, and Swedish Fellowships had not been announced at the time of writing.

The erection of a Solar Observatory in the Australian continent has been under consideration for some years. The matter is now to be proceeded with, and a site at Mount Strombo, near Canberra, the federal capital, has been selected. A director is to be appointed as soon as possible in order that he may visit other Observatories, especially Mount Wilson, before taking up the organisation at Canberra.

The preliminary arrangements in connection with the regular publication of the *Journal of Scientific Instruments* have now been made by the Institute of Physics in co-operation with the National Physical Laboratory. The special attention of those workers who have new designs for instruments is called to the fact that the *Journal* is to serve as a medium of publication of detailed descriptions and critical surveys of the behaviour of such instruments. Original papers or laboratory and workshop notes dealing with the practical or theoretical aspects of scientific instruments should be sent to the Editor, Dr. John S. Anderson, The National Physical Laboratory, Teddington, Middlesex.

Sir George Beilby has resigned from his voluntary post as Director of Fuel Research and Chairman of the Fuel Research Board after nearly seven years' service. He has been succeeded by Dr. C. H. Lander, who will act as Director of Research, and by Sir Richard Threlfall, who becomes Chairman of the Board. Sir George Beilby retains his membership of the Advisory Council of the Department of Scientific and Industrial Research and has consented to act as Honorary Adviser to the Board. One of the main functions of the Board is a survey and classification of the coal seams in the various mining districts by means of chemical and physical tests in the laboratory, supplemented where desirable by large-scale tests at H.M. Fuel Research Station, East Greenwich, or elsewhere. The Board considers that the best way to carry out this work is by means of local committees, the personnel of which would include colliery owners, managers, representatives of the Fuel Research Board and of the Geological Survey of Great Britain, as well as of outside scientific interests. Each committee would be charged with the duty of superintending the work of the survey in a coal-mining area; and in this way the survey would become from the commencement of practical value, since local knowledge and experience would be made

available, and the selection of seams would be decided by those most likely to estimate correctly the relative importance of the problems to be solved. The seams selected would undergo physical and chemical examination by the local experts, after which a final selection would be made of those likely to justify experiments on a practical scale, to test their suitability for particular uses or methods of treatment. The first of these committees has now been actively at work in the Lancashire and Cheshire area for nearly eighteen months, and the Board has recently appointed a committee to deal with the survey in the South Yorkshire area.

The British Non-Ferrous Metals Research Association has undertaken an extensive series of investigations on Die-casting Alloys which will spread over at least three years and entail an expenditure exceeding £10,000. Various types of alloys are used in die-casting, and, recognising the wide field, the proposed research has been divided into three sections: (1) Brass and Bronze Alloys; (2) Aluminium Alloys; (3) Low Melting-point Alloys (Zinc, Tin, Lead, etc.). These three branches, with suitable arrangements for co-operation, will be dealt with in separate institutions, under the supervision of recognised leaders of metallurgical research. Sooner or later, almost every branch of industry in which metal parts of any complicated shape are required, turns to die-castings as the ideal for economical production. The sphere of application will extend considerably if the research work contemplated succeeds in improving the quality and assuring the reliability of the alloys used.

The National Research Council has recently issued a Colloid Bibliography in mimeographed form. The author, Dr. Harry N. Holmes, of Oberlin College, Chairman of the National Research Council Committee on the Chemistry of Colloids, intends this edition to be preliminary to a more comprehensive one. Yet it is a book of 135 pages containing 1,800 references on 106 topics. All the references are classified and many are accompanied by brief comment as an aid in deciding on their relative importance. This book may be purchased from the Washington office at $1.00. The theoretical and industrial importance of colloid chemistry is now admitted without question. In fact it links together the sciences of chemistry, physics, zoology, botany, geology, medicine, agriculture, and even astronomy whenever that science deals with comets' tails. Few are the industries that do not have colloid problems to solve. The human body is colloidal in composition, the digestive processes are governed by colloidal rules of action, and the bacteria of disease are of a colloidal degree of dispersion. But for the colloidal adsorption of calcium phosphate from the blood-stream by the bony cartilage man would be a mere spineless jelly-fish.

The Publicity Service of the British Science Guild (6 John Street, Adelphi, London, W.C.2) has issued a most interesting pamphlet dealing with helium gas and its uses, written by Prof. J. C. McLennan. A survey of all the natural gases within the Empire showed that those from Ontario and Alberta, Canada, were richest in helium, though the proportion was relatively low, being about $\frac{1}{3}$ per cent. of the natural gases. The supply from sources in Great Britain is almost negligible, the natural gas at Heathfield, Sussex, having a helium content of only $\frac{1}{8}$ per cent., and that from the King Spring, Bath, of $\frac{1}{6}$ per cent. No natural gas within the Empire has been found to contain as much as $\frac{1}{2}$ per cent. of helium, whereas in the Western States of America, especially in Texas, natural gases exist which contain from 1 to 2 per cent. of helium, and some springs in France have as much as 5 per cent.

We have received from the University of Chicago Press a catalogue of the very comprehensive collection of astronomical photographs which are available for sale as lantern slides, prints, or transparencies. The list contains well over 500 titles and includes everything likely to be required for ordinary lecture purposes. The majority of the photographs were taken at the Yerkes

Observatory, and all such photographs which have appeared in the *Astrophysical Journal* can be supplied. The lantern slides are all 4 × 3¼ inches in size and cost 75 cents.

A full account of the world's production and resources of Mercury Ores has now been added to the Imperial Institute Monographs on Mineral Resources (John Murray, price 5*s.* net). Compiled by Mr. Edward Halse, it maintains the high standard set up by the other volumes in this series. We note from it that the contract by which the Rothschilds had the exclusive rights for the sale of the mercury produced at Almaden expired in December 1921. The very considerable fluctuations in the market price which have occurred this year are an indication of the struggle which has taken place for the control of the market. The average price of a bottle of mercury (75 lb.) in 1913 was £7 9*s.* 2*d.*; in 1917 it rose to £22 4*s.* 3*d.*; in 1918 it was quoted as "nominal" only, and in 1922 the average was £11 5*s.* This summer it could be bought for laboratory use at a few shillings over £10.

It is stated in the Supplement to *Science* (June 1, p. xii) that reverberation and high-pitched noises in offices can be largely eliminated by using ceilings of felt covered with oilcloth full of small holes. The felt is supposed to absorb sounds of short wave-length, and the perforated oilcloth to provide a covering which obviates the reflection which would occur at an unbroken painted cloth surface.

Scott's ship, the *Discovery*, has been purchased by the Crown Agents for the Colonies, on behalf of the Government of the Falkland Islands, to be employed for research in the interests of the whaling industry. It is desired to determine the geographical limits of the whales, to trace their migrations, and to estimate their numbers and rate of reproduction in order that the industry may be controlled so as to prevent further depletion of stock. At the same time it is intended to carry out oceanographic, magnetic, and meteorological observations. The *Discovery* is being refitted and will hardly be ready until next year; meanwhile a suitable director for the expedition has to be found. It is anticipated that the first expedition will be away from England for over two years.

The third Asiatic expedition of the American Museum of Natural History, which started in the spring of 1921 under the leadership of Roy C. Andrews, had for its objective the exploration of Central Mongolia, a region including the most arid section of the Gobi Desert. As indicated in *The Times* (August 2, 1923), results of the first importance have been obtained. More detailed accounts of these results may be found in *Science* (June 29) and in the pamphlet *The American Museum and Citizenship*, by Henry Fairfield Osborn, abstracted from the Annual Report of the American Museum of Natural History for the year 1922. A cable from Andrews at Peking, dated September 25, 1922, epitomises the results for that year: "... Mongolia expedition discovers vast fossil fields, rich Cretaceous, Tertiary deposits. Skull *Baluchitherium*. Complete skeletons small dinosaurs. Skulls rhinoceros. . . . Two thousand mammals. Mapped large area. Extremely important geological discoveries."

The geological discoveries were as follows: A great unconformity separates the folded sediments of Jurassic and earlier time from all of the desert basin sediments. The marine sediments in this district cease after the Jurassic period; above it the sediments are entirely epi-continental. Above the unconformity ten new formations were found more or less distinguished from each other by their vertebrate remains. Four of these are Cretaceous, two at the confines of Eocene and Oligocene, three in Miocene, and one in the Pliocene. Dr. Osborn claims that this Cretaceous–Tertiary continent constituted the homeland of the chief orders of reptiles and mammals, from which they radiated to Western Europe and North America. The ex-

pedition is visiting the same region this year, and further important discoveries are anticipated.

The first annual report of the governing body of the West Indian Agricultural College, St. Augustine, Trinidad, shows that a satisfactory start has been made and that the wide interest and practical sympathy which its foundation has evoked justify the confidence which is expressed as to its future progress and prosperity. As a result of the efforts of the Principal, Sir Francis Watts, and his staff the College opened last October with eleven undergraduate and four post-graduate students. An Imperial grant of £5,000 was received during the year on account of a promised contribution of £15,000 made on the condition that functions of the Imperial Department of Agriculture were maintained by the institution. The College has acquired an area of eighty-four acres for its work, has four staff residences and a disused hospital which has been adapted as a provisional College building. The governing body has now under consideration the desirability of erecting permanent laboratories and a model sugar factory. The Building Fund in December 1922 had an unexpended balance of £19,460, and various firms had promised factory equipment to the value of upwards of £20,000.

Special Report No. 13 of the Food Investigation Board is entitled, *Studies in Sweetened and Unsweetened (Evaporated) Condensed Milk*, and contains the results of a large number of experiments carried out by W. G. Savage and R. F. Hunwicke (H.M. Stationery Office, Kingsway; price 4*s.* 1½*d.*, post free). The conclusions arrived at are chiefly of interest to manufacturers and public analysts. There are, however, a few points of general interest. The most important relates to the great increase in the bacterial content which occurs when the tin of milk is opened. In the absence of oxygen, condensed milk is a comparatively unsuitable bacterial medium, and the tendency is for a decrease rather than an increase in the number of bacteria present; but once the tin is opened the bacteria multiply with great rapidity, and it is concluded that, if condensed milk be used for infant feeding at all, it should be put up in tins containing not more than one day's supply. Darkening of the milk (through a light yellow to buff and finally to a dark brown) is an indication of age, and no relationship was observed between bacterial content and coloration. The greater the temperature of the milk in store, the more rapidly does it darken, the change being probably due to caramelisation of some of the sugar.

The Smithsonian Institution has issued a pamphlet on *Manufactured Gas in the Home*, by Samuel S. Wyer (Bulletin 102, part 8; Washington: the Government Printing Office, 10 c.), containing information which should be read by every gas-user, and it is to be regretted that a similar booklet, with the slight modifications necessitated by the change of country, is not available for wide circulation over here. While emphasising the great advantages which follow from the use of gas produced in by-product coke ovens, it points out clearly to the user how his burning appliances should be designed, adjusted, and used so as to give efficient and safe service. A special warning is given against the use of the so-called "odourless," "smoke-consuming," and "chimneyless" gas heaters. "These are always dangerous and a positive menace to health." "Much depression and lassitude of spirit, lower vitality, and hence less resisting power to the ever-present disease germs, may be traced to gas fumes from flueless gas-heating stoves." "Furthermore, a flueless stove properly adjusted at 9 o'clock in the evening, when the person goes to bed, may become a carbon-monoxide generator several hours later, due to deflection of the flame or small change in pressure, when the person is asleep." The warnings could hardly be more emphatic! One likely cause of carbon-monoxide production is said to be the radiant gas fire when it is adjusted so that the radiating elements glow for more than three-quarters of the distance from the bottom to the top. The possibility of replacing

gas heaters and gas cookers by electric appliances is discussed, and from experiments carried out at the University of Washington it is found that 6 lb. of coal would be required for electric cooking to 1 lb. for gas cooking. Hence electric heating must be regarded as a wasteful luxury.

According to correspondence in the medical press there is evidence of much surprise and some disconcertancy among the doctors regarding the current outbreak of smallpox, chiefly in Gloucestershire. The disease appears to be of such an extraordinarily mild type that some observers think that it must be merely varicella (chickenpox)—and the difficulty of diagnosis between varicella and variola (smallpox) is notoriously great, except in the presence of well-defined epidemics of either. Little prominence, however, seems to have been given to the idea that the disease in England may be merely what is called Alastrim in the tropics, a malady which lately prevaled in Antigua. Some account of this West Indian epidemic will be found in the *Journal of Tropical Medicine*, June 15, and a short but excellent description of Alastrim is contained in Drs. Castellani and Chalmers's great *Manual of Tropical Medicine*, last edition, 1919, page 1491, with a plate showing the remarkable resemblance of Alastrim to smallpox. The characteristic of Alastrim is that it is extremely mild (mortality only 1–2 per cent.). Cowpox vaccination protects against Alastrim, but it also succeeds immediately after an attack of that disease. We should say, however, that the whole subject requires much more careful investigation. Is it not surprising that after all these years so little addition has been made to the great work of Jenner on smallpox, which was completed a century and a quarter ago? We have not certainly found the germ of smallpox, and consequently all these important problems remain in the air. We do not know exactly how the contagion is conveyed, and we have made no attempts to discover a cure. Nothing better shows the incompetence consistently exhibited in the world's counsels concerning medical matters. In spite of the thousands of doctors and hundreds of paid investigators the most important medical problems still remain unsolved and often not even attacked.

The same remarks may be made regarding cancer. The Ministry of Health has recently issued a memorandum on cancer prepared by a departmental committee. It is a disturbing document. The cancer rate continues to rise by annual increments which really cannot be explained by improved diagnosis or by any statistical considerations. The death-rate in England and Wales has gradually increased from 0·32 per 1,000 in 1851–60 to 1·12 per 1,000 in 1911–20, and 1·21 per 1,000 in 1921. In other words, nearly one out of every ten persons now living will probably die of this most painful and terrible malady—a pleasant prospect to look forward to. A brave attempt is being made by a number of gentlemen to start a "British Empire Cancer Campaign," and we wish every success to it—they are asking for a million pounds from the public. At the same time, many subsidised cancer researches are now being carried out in connection with a number of hospitals and other institutions, and there is already an Imperial Cancer Research Fund. We wish prosperity to all of them; but as some of them have been in existence for many years the public is beginning to feel rather despondent on the matter and is inquiring whether something else cannot be done to stimulate quicker progress.

We congratulate the War Office for having appointed a distinguished man of science, Sir William B. Leishman, K.C.M.G., F.R.S., to be Director-General of its efficient Army Medical Service.

CORRESPONDENCE

To the Editor of SCIENCE PROGRESS

ATOMIC FORM

I—FROM EDWARD E. PRICE

DEAR SIR,—With reference to the review of this book published in the April number of SCIENCE PROGRESS, will you allow me to point out that some work has been done on the construction of space-lattices consisting of carbonoid aggregates which has not yet been published. It has been found that by simple arrangements of carbonoids in three dimensions, two different forms of strictly rectangular structures can be built up, both corresponding to the tetragonal system, and by a further and different arrangement a third structure analogous to a tetrahedral space-lattice can be formed. Your reviewer was naturally unaware of these results.

Diamonds are obtainable in a variety of forms, including not only the octahedron and cube, but also tetrahedral and tabular crystals are obtainable. A careful study of these shows that if the space-lattice is arranged on the cubic system a tetrahedral form exists (as described) which is part of either the octahedron or the cube, the form being dependent upon the method of arrangement adopted.

In the octahedron the bases of the tetrahedra are on the outside and form the eight faces of the octahedron. In the cube the outer surfaces are those of the three right-angled faces surrounding the apex of the tetrahedron. The latter may therefore be correctly described as the simplest form of the carbon crystal.

May I also point out that with regard to crystals of benzene, we have to deal with six hydrogen atoms which may be expected to exercise some influence on the crystal structure of that compound.

I am, sir,
Yours faithfully,
ED. E. PRICE.

May 7, 1923,
THE HAVEN, DORMAN'S PARK,
SURREY.

II—FROM W. A. ASTBURY, B.A.

DEAR SIR,—The reviewer must confess that he was unaware of the unpublished work on the construction of space-lattices from "carbonoid aggregates," but that does not alter the fact that the structure of diamond has been known for quite a long time (W. H. and W. L. Bragg, *Proc. Roy. Soc. A.*, Vol. 89, p. 277). And whatever discussions may have arisen with regard to the details of more complex structures, the Bragg analysis of diamond has been accepted without hesitation by chemists, physicists, and crystallographers throughout the world. The structure of diamond is simply this: it consists of two interpenetrating face-centred cubic lattices of carbon atoms arranged in such a way that each atom lies at the centre of gravity of four others.

In Shearer's paper on "The Relation between Molecular and Crystal Symmetry" (*Proc. Phys. Soc.*, Feb., 1923) it is shown how the carbon atom must possess at least one threefold axis of symmetry; but more recently Sir Wm. Bragg has concluded that it is probably more highly symmetrical still. The unit cube contains eight atoms, and if these correspond to eight interpenetrating differently orientated simple cubic lattices, the symmetry of the carbon atom in diamond is at least three. But X-rays reveal no more than *two* different orientations of carbon atoms, *i.e.* there are really only two Bravais face-centred cubic lattices, and the symmetry of the atom is twenty-four, that of a *regular* tetrahedron (*v.* Sir Wm. Bragg's letter to *Nature*, April 21–3, where it is shown that the structures of diamond and basic beryllium acetate $Be_4O(Ac)6$ are completely analogous). This recent development makes the case for the "carbonoid" rather worse than when the review was written. But even in the less symmetrical form of carbon, graphite, one-trigonal axis is still maintained by the carbon atom. There is a direct connection between the structures of diamond and graphite, and the aliphatic and aromatic divisions of organic chemistry.

It is also necessary to repeat that the tetrahedron, whose base is an equilateral triangle and other sides right-angled triangles, is *not* the simplest form of diamond. It is merely the corner chipped off the cube (in accordance with the perfect octahedral cleavage), and as such is a combination of *two* forms, the cube {100} and the octahedron {111} (or regular tetrahedron {111}.) Any elementary textbook on crystallography will elucidate this point. With regard to benzene, see Shearer's paper above, or Sir Wm. Bragg's lecture (*Trans. Chem. Soc.*, vol. 121, 1922).

May 22, 1923. W. T. A.

THE EVOLUTION OF THE CATERPILLAR

From C. C. Ghosh, B.A., F.E.S., Entomologist, Agricultural Department, Mandalay, Burma

Dear Sir,—In this interesting article, by Herbert Mace, in order to explain how a leaf-rolling caterpillar brings together the two edges or two opposite parts of a leaf, a footnote is added (Science Progress, April 1923, p. 624) that "it is probably natural shrinkage of the silk, as it dries on exposure, which draws the parts together." This is not the real fact, as will appear from observing a caterpillar doing the work. The silk is applied to two opposite points and the caterpillar places its legs on the silken thread and presses it down, thus drawing the two points closer. At the same time more silk is applied and similarly pulled down. Therefore the two opposite points of the leaf are brought quickly together. If the natural shrinkage of the silk had to be depended upon the work would have taken probably a day, while it is actually performed in some cases in a minute or two.

May 17, 1923, C. C. Ghosh.
Mandalay, Burma.

DR. KAMMERER IN CAMBRIDGE

From Prof. E. W. MacBride, F.R.S.

Dear Sir,—Perhaps you will allow me to make some comments on Mr. Thacker's account of Dr. Kammerer's Cambridge lecture which appears in your last issue.

Mr. Thacker says that the phenomena described by Dr. Kammerer form too narrow a basis on which to found the far-reaching generalisation that acquired characters are inheritable. Now the idea that the effects of habit are inheritable is one which was forced on me by a wide survey of the facts

of comparative embryology before ever I had heard of Kammerer, and to this same conclusion the best systematic zoologist and the best palæontologist amongst my acquaintances have likewise been driven by the facts which have formed the subjects of their lives' study. What was needed to complete conviction was the experimental demonstration that, in one or two particular instances, the effects of habit were actually inherited, and it is this proof that Dr. Kammerer has supplied, and the working out of it occupied a period of thirteen years.

Does Mr. Thacker suggest that before accepting the possibility of the inheritance of acquired characters he must wait till a similar proof has been brought in the case of a large number of different species? No ordinary span of human life would suffice for such a demonstration, and in no other science than biology would such a requirement be made. If a law is deduced from a general survey of phenomena and its truth is experimentally shown in one or two critical cases—then the law is regarded as proved. The only reason why this is not so in biology is the unfortunate fact that in the minds of many biologists Mendelism has ceased to be a descriptive theory, but has become a dogma—nay, even a religion.

But Mr. Thacker goes on to make specific objections. He complains that Dr. Kammerer did not state what was the normal range of variation in the colour pattern of *Salamandra maculosa* or the number of specimens which he used for his various experiments.

Now Dr. Kammerer stated that full details were to be found in his complete papers, which are well-known parts of the literature in that department of heredity, and which it is evident that Mr. Thacker has not troubled to read. But in his lecture Dr. Kammerer did state certain things which should have been decisive for Mr. Thacker. He stated that in trying the effect of a yellow background on *Salamandra*, he always started with an almost black specimen and that he invariably got the same result in all the salamanders which survived to maturity. This result—in two generations—was the production of a *Salamandra taeniata*, in which the yellow colour was arrayed in two broad bands on the back. This variety is not to be found amongst the wild Salamanders in the neighbourhood of Vienna, all of which belong to the sub-race *S. typica*.

Then Mr. Thacker complains that salamanders belong to a group of animals which are apt to behave very differently in different circumstances—as witness *Amblystoma*. This objection is as rational as would be the objection of a conservative astronomer to modern efforts to determine the distances of fixed stars on the ground that experimenters had begun their studies with stars with a determinable parallax! Undoubtedly Amphibia generally, and salamanders and newts in particular, are more plastic than many other animals, and therefore the results of changed conditions on them can be demonstrated in a measurable time; but even when *in one generation* they change their structure in response to new habits yet the children start where the parents left off, and the demonstration of the fact that the exposure of the parents to a particular environment produces an appreciable effect on their offspring, is the crux of the proof of the inheritance of acquired characters. Finally, Mr. Thacker objects to Kammerer's dilemma that "If what changes cannot be hereditary and if what is hereditary cannot change, we can only predict the immutability of species." Mr. Thacker says that followers of Weismann only say that a change, to be hereditary, must be a change in the germ-plasm, and that Kammerer's dilemma is a truism. Mr. Thacker's statement is indeed a meaningless truism. What, it may be asked, is meant by the question-begging phrase the germ-plasm? If Mr. Thacker means the germ-cells, then all the trend of modern cytological research is to show that there is no essential difference in constitution between germ-cells and somatic cells, and does Mr. Thacker imagine that Kammerer

does not know that if a change appears in the offspring, the germ-cells from which they developed must have been altered ?

No ; the true alternative is whether we are to regard the alterations in the germ-cells as entirely uncaused or due to " chance," or whether they are the results of changes produced by the influence of the altered soma. Which is the more reasonable supposition I leave to the judgment of your readers.

E. W. MacBride.

Imperial College of Science and Technology,
July 4, 1923.

THE DARWINIAN THEORY

I—From R. A. Fisher, M.A.

Sir,—In his article on " The Present Position of the Darwinian Theory," Prof. MacBride puts forward a case for the " inheritance of acquired characters," and appears to regard the acceptance of the reality of such inheritance as necessary for a belief in the gradual transformation of species. The gap in his argument is best displayed in the following passage (p. 82) :

" We are driven, I think, to the conclusion that there is no evidence of the existence of that variability to an indefinite extent in all directions which Darwin postulated. In a word, the minute differences dividing members of one litter from one another are not inheritable. It is usual nowadays to attribute such differences to accidental variations in nutrition either prenatal or postnatal, and they are termed Fluctuations. If we reject both the inherited effects of habit and indefinite variability, there remains only a third alternative to fall back on, *viz.* the strongly marked variations formerly denominated sports or monstrosities, which sporadically occur not only amongst our domesticated breeds, but also amongst wild species, and which are known to be strongly inherited."

The process of exhaustion in the last sentence is so far from being complete that it omits the almost universal form of variability, which may be exemplified by human stature. The differences in stature dividing brothers from one another are not, in ordinary language, " sports or monstrosities " ; they can scarcely be attributed to the inherited effects of habit ; and they are known to be strongly inherited. They represent, I fancy, very much the type of " indefinite variability " which Darwin had in mind, and if we speak of them as fluctuations (as opposed to sports) we must recognise that fluctuations are of two kinds, those due to heredity and those due to environment. If, on the other hand, we wish to designate all hereditary changes by a single word, *Mutations*, for example, we must remember that this term (which Prof. MacBride uses as synonymous with " sports or monstrosities ") is applicable equally to the hereditary basis of fluctuating variation.

The pure-line experiments to which Prof. MacBride refers show that such mutations are rare ; their rarity is of importance in throwing light upon the question of whether the factors underlying hereditary fluctuating variation are inherited on the Mendelian scheme, as so many " sports " are known to be. Mendelian inheritance of fluctuations implies that a large number of factors are present, and I have recently shown (*Proc. Roy. Soc. Edin.*, vol. lii, pp. 321–41) that with Mendelian inheritance mutations must be very rare, else the variability of the species would continually increase without limit. Under blending inheritance continual new mutations would be necessary in every generation in order to maintain the variability ; whereas with Mendelian inheritance the new mutations have only to be sufficiently numerous to counterbalance the occasional extinction of genes, which occurs principally owing to adverse selection. The rarity of mutations in pure-line experiments is thus favourable to the view that hereditary fluctuating variation is due to numerous Mendelian factors.

To this evidence of the Mendelian inheritance of fluctuating variation may be added that of the statistical study of human measurements. A measurement dependent on numerous cumulative factors should show approximately normal distribution, and the simultaneous distribution of the measurements of two relatives should show an approximately normal surface with linear regression; these expectations have been strikingly realised. Going more into detail, it is invariably observed that the fraternal correlation is slightly larger than the parental correlation, and this flows as a necessary consequence from the Mendelian phenomenon of *Dominance*. Finally, from the correlations between husband and wife, combined with the parental and fraternal correlations, it is possible to calculate a quantity termed the "dominance ratio." The available figures for three human measurements agree within the limits of random sampling, and give a mean value ·32 ± ·03; this also agrees closely with expectation, for, as I have recently shown (*ibid.*), the theoretical value for the dominance ratio for a population *subject to selection* is exactly one-third.

Those who are prepared to believe that such a series of verifications is due to chance will scarcely be convinced by the actual isolation and identification of the factors concerned; this has, however, been achieved in *Drosophila* in the case of the fluctuating variability of the abnormal condition known as "beaded wings." The rapidity with which a character dependent upon cumulative factors may be modified by selection is shown also by the hooded pattern in rats, in which selected lines have been modified in both directions so as to consist of almost all-black and all-white individuals, and in which back-crosses have verified the hypotheses of cumulative factors.

These examples show that the selectionist, who (*pace* Prof. MacBride) may also be called a Darwinian, has no need to postulate the "inheritance of acquired characters" in explaining the gradual and progressive modification of characters showing fluctuating variability. If Dr. Kammerer and Prof. MacBride can demonstrate the inheritance of environmental responses, so much the better; but such a demonstration would be far from displacing the natural selection of fluctuating characters as the predominant cause of evolutionary change.

R. A. Fisher.

Rothamsted Experimental Station.
July 20, 1923.

II—From Prof. E. W. MacBride, F.R.S.

Dear Sir,—Mr. R. A. Fisher has been kind enough to send me a typewritten copy of a letter which he has addressed to you criticising my article on the "Present Position of Darwinism," which was published in the last number of Science Progress. The point of Mr. Fisher's criticism is that, in analysing the different types of variation, I had omitted "the almost universal form of variability, exemplified by human stature." This, it seems to me, is a very rash accusation to be levelled by a non-biological mathematician against a biologist. Mr. Fisher goes on to make some remarkable statements. The fact that measurements of human stature can be arranged on a Galtonian curve of error is held to prove that they are due to the occurrence of a large number of small inheritable "mutations." Mr. Fisher also states that differences of stature between brothers are known to be strongly inherited.

Now Mr. Fisher's objections are characteristic of the loose kind of statistical juggling that passed for research in heredity before the insistence on exact experimental methods. How, for instance, does Mr. Fisher know, or how could anyone know, that differences in stature between brothers are inherited? This could only be done if the brothers married women of exactly the same genetic constitution, and how could this sameness be

ascertained? One has only to state the position clearly in order that everyone can see the absurdity of it. The fact is, that knowledge of the principles of heredity can only be obtained by careful experiments conducted under standardised conditions. It was tacitly assumed by Darwin that differences between members of the same family, brood, and litter were inheritable. The pure-line experiments were devised in order to test this assumption; and the answer that they have uniformly given is that the assumption is incorrect. To attempt to maintain, on the ground of the mathematical manipulation of human statistics, that small "mutations" and excess and defect do nevertheless occur in sufficient numbers to account for the whole process of evolution is thoroughly unscientific. We are not allowed to make experiments with our fellow human beings. Therefore the laws of heredity can only be ascertained by dealing with the lower animals and plants.

I do not for a moment deny that there are inheritable differences in stature amongst the population of these islands; but their explanation is very simple. Our people are a mixture of three different races; the Nordic, the Alpine, and the Mediterranean. Each race has its own normal stature, which is a racial characteristic. These races have been intercrossing in the most complicated fashion for the last 2,000 years; probably the majority of individuals are blends in different proportions of all three. Is it any wonder, therefore, that these various inheritable tendencies in stature exist?

There is every reason to believe that the racial characters have been slowly acquired during a struggle extending over many thousands of years with the forces of Nature in the region in which each race was evolved, and that the characters are examples of functional evolution or the "inheritance of environmental response."

I do not wish to trespass upon your space to discuss the theory of "multiple factors," or Morgan's supposed verification of them in the case of "beaded wing." I have already expressed my opinion that one and all of Morgan's mutations are pathological in character and have no analogy with the differences between natural races and species. No one denies for a moment that these pathological variants mendelise beautifully when crossed with the type, but I strongly suspect that the phrase "multiple factors" could advantageously be replaced by "differing degrees of damage."

Mr. Fisher states that the "selectionist" has no need to fall back on the inheritance of acquired characters in order to explain evolution. Selection by itself creates nothing, it only enables the "fittest," when here, to survive. I am as strong a believer in natural selection as Mr. Fisher, but the fittest are, in my opinion, those individuals which react most strongly and vigorously to the environment. In Mr. Fisher's view they owe their genesis to "chance."

Finally, Mr. Fisher invites Dr. Kammerer and myself to demonstrate the inheritance of environmental response "if we can." I am sure that neither Dr. Kammerer nor I nor any other well-advised biologist would attempt such a task unless: (1) Mr. Fisher acquired sufficient biological knowledge to appreciate our evidence; (2) made himself acquainted with the details of that evidence.

To what has been the dominant factor in evolution I prefer to Mr. Fisher's opinion the views of those of my friends amongst palæontologists and systematic zoologists who are best acquainted with the facts, and they are tending more and more to view the Lamarckian solution with favour. Mathematicians can render invaluable help to biology, as they can to other sciences, when they deal with data which have been sorted and prepared for them by the specialists in that science; but when they attempt to draw conclusions from unanalysed data in subjects which are unfamiliar to them, their efforts only result in obscurity and error.

E. W. MacBride.

August 26, 1923.

ESSAYS

THE PROPAGATION OF SOUNDS FROM EXPLOSIONS (J. P. Andrews, B.Sc.).

During the past few years, the curious problems connected with sounds from great explosions have engaged the active attention of meteorologists and of physicists both in this country and on the Continent. The chief interest centres round the anomaly of propagation, according to which, not only does the intensity of the sound fall off as the distance from the source increases, but it is renewed with unexpected vigour at much greater distances, although in the intervening area no sound at all can be heard. Thus we have an Inner or First Area of Audibility very roughly surrounding the source, the dimensions of the area being different for different explosions. This is encircled in most cases by a very irregular belt where sounds heard are few or none—the " Zone of Silence "—whilst farther afield yet are found areas where sounds are again heard quite unmistakably. This last is known as the Second or Outer Area of Audibility, and it has been with a view to explaining the manner in which the sound reaches this area without traversing the lowest regions of the atmosphere that most thought has been expended.

The oldest theory was founded on considerations of wind and temperature, and has for that reason been called the meteorological theory. In principle it was as follows: if a sound wave, in travelling upwards in a slanting direction, is overtaken by a wind whose component of horizontal velocity increases with the height above the earth's surface, then the sound wave will be swung round, since its more elevated portions are travelling the quickest. Under these circumstances, the wave should eventually return to earth without having been detected in the area close to the source. On the other hand, if the direction of the wind is opposed to that of the sound wave, with a similar velocity-gradient, the sound-wave should be deflected from the earth. The simplicity of this outline would never apply to an actual case, but certain consequences may clearly be drawn. For example, it would not be expected that the various areas would be symmetrically disposed round the source, whilst the summits of the " sound-rays " will rarely reach a greater altitude than 10 km. Up to the outbreak of war there were, however, few observations by which the theory might be tested. The observations of Fujiwhara in Japan, while affording justification for the hypothesis, scarcely confirmed it.

Whilst the matter was in this nebulous state, von dem Borne in 1910 threw out the bold suggestion that the sounds of the outer area were due to waves returning from much greater altitudes, of the order 100 km. in fact. He considered that the quiescence of the atmosphere above 10 km. would allow the heavier gases to sink to the bottom, leaving an upper atmosphere composed mainly of hydrogen and helium. At 100 km. from the earth's surface he estimated the amount of hydrogen to be 99. It follows, for reasons used before, that any sound wave penetrating to such heights will be swung round toward the earth. In distinction from the older case, we observe that the sound areas should be symmetrical with respect to the source, except as they are modified by winds near the surface. Von dem Borne calculated

that a fairly "sharp" inner edge should be observed on the Outer Area, at a distance never less than 114 km.

On account, perhaps, of the ingenuity of the theory, it attracted attention without carrying conviction, until in 1915 it found an advocate in Van Everdingen, the Dutch meteorologist, who in that year published a careful analysis of eight series of explosions whose audibilities had been observed in Holland. The evidence was in some cases decidedly in favour of the new or "physical" theory, as in one instance where the inner edge of the second area formed an almost perfect arc of a circle whose completion appeared to be in the belligerent area, or when it was pointed out that there actually was a minimum distance from the source within which the outer area seldom ventured. Unfortunately this distance was nearer 160 km. than 114 km.; but van Everdingen, on more accurate recalculation, gave very plausible reasons why this should be reckoned as an inaccuracy of van der Borne's work rather than a blemish of the theory. It cannot be said, however, that the weight of observation was on the side of the physical theory, for seven out of the eight series showed a disconcerting lack of symmetrical distribution, although, again, it seemed certain that winds had no consistent effect in producing these distortions.

Renewed interest stirred up criticism, and objections were soon brought forward. Schmidt, for example, remarked that at 100 km. the air is so attenuated as to amount to a vacuum, and it was inconceivable that sound should be propagated across a vacuum and return to earth with sufficient intensity to rattle windows. Yet it appears, as was pointed out by Schrödinger, that there are ways in which this might occur. Indeed, it had already been shown that the upper atmosphere would converge the sound waves in a manner analogous to that of a convex lens.

The theory has encountered much more serious setbacks since then, from more recent observations which, as often as not, are not explicable on either theory. For instance, M. A. Perot reports that bombardments at the front were heard at Meudon only when the wind blew from Meudon to the battlefield, and never when the wind was in the contrary direction. Meteorological influence is apparent, but in an unexpected form. Again, bombardments were only audible on the Allies' side of the line in summer (it is reported) at great distances, whereas the Germans and Belgians heard them only in winter. It begins to appear, too, that whether highly arched rays are to be considered or not, there are certainly some sounds which reach the second area of audibility by a low route. This was noted in the time of the great explosion at Silvertown in 1917 (on which occasion there also appears to have been evidence that air "waves" of inaudible frequency passed through the Zone of Silence). The results for the British Isles of the observations on the audibility of the Oldebroek explosion of October last confirm this. The observed velocities have been grouped, and it would seem that the sounds travelled in some cases by briefer routes than in others. Now van Everdingen has calculated that the higher route should require a few minutes longer than the low route, and it may be that both routes are possible. It would be rash, however, to regard the recent observations as conclusive.

Unexpected difficulties have arisen with an attempt at more precise observation. Major Tucker's observations indicate that earth tremors are capable of travelling from the explosion and disturbing an accurate recording instrument, to the confusion of the results. Nevertheless, experimental explosions such as that at Oldebroek cannot fail to give a clearer insight into the real nature of this baffling problem, whose intricacies seem to have increased with every attempt to unravel them.

INAUDIBLE AIR-WAVES (Charles Davison, Sc.D.).

INAUDIBLE air-waves are manifested chiefly by the shaking of windows, the disturbance of pheasants, and, in a few cases, by the records of barographs. Close to the origin, windows rattle while the sound of an explosion is heard; at distances of several or many miles, the two noises are separated by a second or more, showing that the sound-waves and inaudible air-waves travel with different velocities or by different routes. The disturbance of pheasants has been attributed to keener powers of hearing low sounds than are possessed by human beings. That this is not the case, however, is clear, (i) because men hear the sound of an earthquake far beyond the limits of pheasant-disturbance,[1] and (ii) because pheasants are sometimes not affected until after the sound has died away. It is more probably due to the quivering of the branches on which they are resting produced by the passage of the inaudible air-waves.

AIR-WAVES FROM GUN-FIRING

The sudden disturbance of pheasants is often caused by distant gun-firing. For instance, in May 1912, a trial firing of the heavy guns of H.M.S. *Orion* was carried out off Weymouth. The reports were heard at Hambledon, near Godalming (91 miles). "At each discharge," an observer there writes, "there was . . . a prolonged dull vibration, hardly audible except by the noise of the secondary vibration induced in doors and windows, but," he adds, "the pheasants were crowing all around." Pheasants were also disturbed by the same gun-trials at Drayton St. Leonards, near Oxford (95 miles).

On July 18, 1900, at about 10 p.m., a sham fight took place between two portions of the French fleet assembled at Cherbourg. The number of vessels engaged was 43, including 13 of the largest battleships then built. It is estimated that the number of charges fired was more than 24,000. The night was very still, there was hardly a breath of wind and the sea was calm—conditions that, shortly after sunset, favoured the transmission of sound-waves in the lowest atmospheric layer. The sound was heard along the south coast of England from Torquay (101 miles) on the west to Brighton (104 miles) and Henfield (107 miles) on the east, and in only two cases more than a mile or two from the coast. At Lancing (100 miles) a curious throbbing sensation in the air was noticed, and an observer at Brighton remarked that he heard or felt the sound. At most, if not all, the places of observation, windows were shaken, and this also happened at more distant places at which no sound was heard—at Plymouth (123 miles) and Menheniot, near Liskeard (136 miles).[2]

Action of Dogger Bank: January 24, 1915.—On this day there was a running fight between British and German cruisers. The enemy ships were first sighted at 7 a.m. Soon afterwards they turned and made for their home ports, closely followed by the British vessels. The first shots were fired at about 9 a.m. Half an hour later, the slow-going *Blücher* fell behind, and, becoming a target for each of our cruisers as it passed, sank about noon. At 7.30 a.m. the position of our ships was lat. 55° 10′ N., long. 3° 32′ E.; at 11.30 a.m. it was lat. 54° 21′ N., long. 5° 4′ E.[3] At 9.30 a.m. it would be about lat. 54° 48′ N., long. 4° 20′ E., or about 232 miles east of Sunderland. The distances given below are measured from this

[1] During the Hereford earthquake of 1896, pheasants crowed at a distance of 111 miles to the north-west of the origin; the sound was heard by man to a distance of 170 miles in the same direction. During the Doncaster earthquake of 1905, the farthest place at which pheasants were affected is 38 miles from the origin; the sound was heard to an average distance of 62 miles.

[2] *Nature*, vol. lxii, 1900, pp. 378–9.

[3] Jellicoe, *The Grand Fleet* 1914–16, pp. 188–99.

point, but it should be remembered that the position was constantly varying, and that the British and German lines were separated by several miles.

Long before the news of the battle reached this country, it was clear, from the behaviour of pheasants, that heavy firing was taking place off our eastern coasts. From the north of England, Canon Rawnsley collected many records, and several interesting letters also appeared in *The Times*. From the southern counties there comes only one record of a disturbance that may have been connected with the North Sea firing. At Merton, near Thetford (216 miles), " the pheasants from 8 to 10.45 shrieked themselves hoarse with terror," and " even the smaller birds on this occasion were terrified." It should be remembered, however, that there was at this period frequent firing in the North Sea of which the newspaper Press contained no account. Moreover, it was not until about 9 a.m. that the firing from the cruisers became general, and the sound-waves would require about seventeen minutes to travel 216 miles.

At only one place in this country was the sound of the firing actually heard. In Gunby Woods, near Burgh-le-Marsh in Lincolnshire (208 miles), the crowing of pheasants was noticed, and the guns, it is said, were heard simultaneously. At 9.45 a.m., pheasants in the Mulgrave Woods, near Whitby (207 miles), were excited. In other parts of Yorkshire, the restlessness and crowing of pheasants were noticed at West Ayton and Langdale End (200 miles), Market Weighton (216 miles), York (221 miles), Byland Abbey [1] (236 miles), and Brompton and Ripley [1] (248 miles); and, in the north of Lincolnshire, at Saxby (221 miles). Farther to the west, similar observations were made at Skirwith Abbey and Lowther, near Penrith (282 and 288 miles), Barrock Park, near Carlisle (294 miles), Runcorn (311 miles), and Clifton Moor, near Workington (320 miles). At Ripley the sound of the guns was inaudible and was represented by a curious " soughing " in the ears.

Air-waves from Explosions

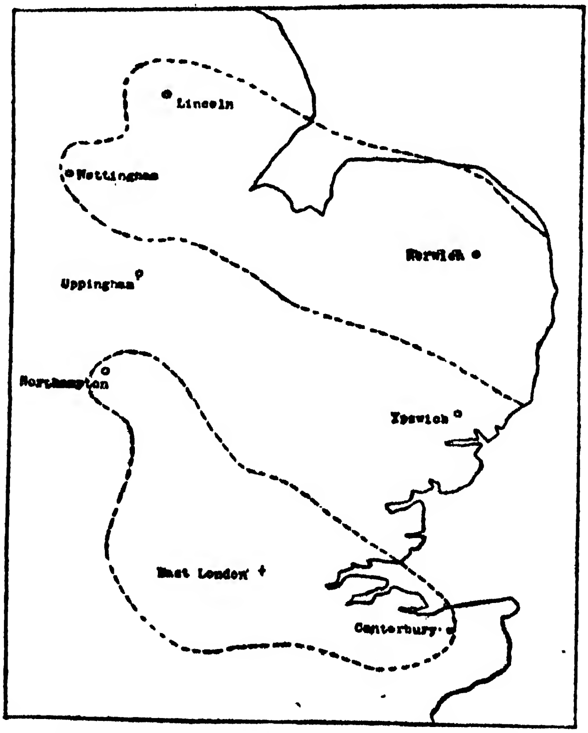
Explosions of Zeppelin Bombs.—During the war, the counties along the east coast were frequently visited by enemy airships. These counties contain many pheasant-covers, the birds in which were alarmed in the usual manner; they flew restlessly from trees and the cock-birds crowed loudly. The excitement among them occurred when the explosions of the bombs were quite inaudible to human ears, and even when the bombs were dropped at a distance of 80 miles. When the explosions were just heard, it was noticed that the pheasant-crowing usually began a second or more before the arrival of the reports. At small distances, however, the sound was heard before the arrival of the inaudible waves. For instance, during the Midlands air-raid of January 31, 1916, the Zeppelins passed from three to seven miles west of Birmingham. The flashes of the bombs were visible as great columns of light, and the sounds were followed, from about half a second to a second later, by a brief and sharp rattling of the windows.

East London Explosion: January 19, 1917.—The areas over which the sound of this explosion was heard are shown in the accompanying map. The inner sound-area extends east-south-east to near Canterbury (48 miles), and north-west to near Wellingborough (66½ miles); towards the north-east its boundary passes at 20 miles, and towards the south at 19 miles, from the origin; the total area included is 3,390 square miles. The outer area contains 5,000 square miles. The width of the silent zone varies from 28 miles at the western end to 48 miles at the eastern. The distance of the outer margin of the zone from the origin ranges from 65 to 95 miles.

In the inner sound-area, at Turnham Green (13½ miles), a lady, who was out-of-doors, held a powder-box open in her hand. Though the air was still,

[1] These places are given as Wyland Abbey and Ripleth in *The Times* account.

a little puff of powder rose from the box; almost immediately afterwards the report was heard. Blinds and curtains were driven inwards, several inches as a rule, at New Southgate (7 miles), Enfield (11½ miles), Wimbledon (13 miles), North Cheam (14 miles), Twickenham (17½ miles), and Sunninghill (21½ miles). Observers in the open air noticed the swishing of tree-tops at Windsor (28 miles) and Ascot (31½ miles), both places being near the boundary of the inner



Sound-areas of East London explosion, January 19, 1917.

sound-area. That the tops of the trees were affected may be due to the upward tilting of the air-waves as they prepared for their journey over the silent zone. Windows suddenly rattled with some violence, and then, after a few seconds, returned to rest. The window-rattling occurred after the arrival of the sound-waves in London, at Barnes (12 miles) and East Acton (13 miles), and before their arrival at Luton (32½ miles). Doors were shaken before the sound was heard at Harpenden (28 miles). At Upper Norwood (11½ miles), a window was blown outwards before the roar of the explosion came. Pheasants were disturbed immediately after the sound was heard at Mountnessing, near Brentwood (18 miles), Stevenage (30 miles), and Bradenham, near High Wycombe (37½ miles); and before at Woodhall Park, near Hertford (24 miles).

In the outer sound-area, windows were shaken before the sound-waves arrived at Great Ponton (99½ miles) and Hatton Holgate (115 miles). Pheasants are said to have been disturbed before the reports were heard at nineteen places, and after at two. Both of these places, however, are close to others at which pheasant-crowing preceded the reports.

Some of the most interesting observations of the inaudible air-waves are those which were made between and beyond the two sound-areas. In the silent zone, windows were shaken at about the time when the sound might have been heard at eight places, four of which are in the narrow and lofty district at the western end of the zone; while pheasants screeched at eight places, five of which are at the western end. Such observations were, however, much less frequent in the silent zone than in the two sound-areas. Beyond the inner sound-area, pheasants showed the usual signs of alarm at Swyncombe, near Henley-on-Thames (45 miles), Bradfield (51 miles), and West Woodhay, near Newbury (63 miles); possibly also at Nursling, near Southampton (77 miles), and Alderbury, near Salisbury (84 miles). Beyond the northern boundary of the outer sound-area, they are said to have screamed near Beverley (165 miles), Malton (180 miles), and Thirsk (200 miles).[1]

Krakatoa Explosion: August 27, 1883.—The most remarkable record of inaudible air-waves is that of the great explosion of Krakatoa. At some stations the barograms showed traces of seven passages, the first, third, fifth, and seventh being along the minor arc and afterwards round the world; the second, fourth, and sixth along the major arc, through the antipodes of Krakatoa, and afterwards round the world. The air-waves were thus traceable after more than three complete traverses of the earth. The mean velocities along the minor arc and its continuation was respectively 1,037, 1,002, 999, and 990 ft. per second; those along the major arc and its continuation were respectively 1,061, 1,049, and 1,041 ft. per second. Since at 0° F. the velocity of sound is 1,055 ft. per second, at 50° F. 1,145 ft. per second, and at 80° F. 1,177 ft. per second, it follows that the velocity of the air-wave in either direction was slightly less than that of sound at ordinary temperatures. As Lieutenant-General Strachey remarks, "its mode of propagation by an aerial oscillation, of comparatively short duration, was also closely analogous to that of sound."[2]

Conclusions

(i) In every case, the inaudible air-waves are manifest at much greater distances than the sound-waves are heard—the distances being 107 and 136 miles for the Cherbourg review, 128 and possibly 200 miles for the East London explosion, and 208 and 320 miles for the Dogger Bank action. The figures for the Krakatoa eruption—2,968 and nearly 85,000 miles—are not comparable with the above, owing to the convergence of the inaudible waves towards the antipodes.

(ii) At ordinary temperatures, the actual velocity of sound is somewhat greater than that of the inaudible waves.

(iii) When a silent zone is manifested, the inaudible waves cross the zone at a less elevation than the sound-waves, though near enough to the ground to produce occasional disturbance of windows and pheasants. They ultimately cease to be perceptible owing mainly to loss of strength. The boundaries of the sound-areas, on the other hand, are determined rather by the paths of the sound-rays than by the weakening intensity of their vibrations. While both sound-waves and inaudible air-waves are travelling near the ground within the inner sound-area, the sound-waves slightly outrun the inaudible waves, but, even before they reach its boundary, the sound-waves begin to fall behind, as they rise into higher and colder air. In the outer sound-area, it is probable that the sound of the explosion is always heard after the disturbance of windows, etc., but the interval between the two phenomena is so brief as to suggest that the sound-waves, while following their somewhat loftier course over this silent zone, do not attain to any considerable height.

[1] *Edin. Roy. Soc. Proc.*, vol. xxxviii, 1918, pp. 115–29.

[2] *The Eruption of Krakatoa and Subsequent Phenomena*, edited by G. J. Symons, 1888, pp. 57–78.

ESSAY-REVIEW

LIFE AS MEMORY. By the Rev. A. A. Luce, M.C., D.D., F.T.C.D., being a review of: **La Memoria Biologica.** By Eugenio Rignano. [Pp. 249.] (Bologna: Nicola Zanichelli. Price L. 17.50.)

This is an ambitious book. It surveys most sublunary matters and some others. It strides from chromosomes to the Categorical Imperative, from nuclear division to the problem of immortality, from blastomeres to Einstein's theory of relativity. For its general design we have nothing but praise. There is room for more books of this type. It is the work of a synthetic mind. Rignano combines the philosopher's breadth of vision with the experimentalist's attention to detail. He might not take it as a compliment, but we must say it—he is a metaphysician in spite of himself. In this book he advances a metaphysic of life. He discusses many subjects, but he has only one theme. He believes that all the essential elements of human experience are explained by conceiving life as memory—a conception for which he claims experimental evidence, but which is, and must remain, at bottom, metaphysical.

The book is primarily for biologists. It is likely to make a wider appeal. Psychologists, particularly those of the behavioristic school, will find in it much to their purpose; while those metaphysicians who, thanks to the influence of Wundt, Driesch, and Bergson, include in their studies the theory of the organism, will react to the challenge of Rignano's hypothesis. The conception of life as memory is not new. It was introduced to the notice of the biological world by Hering and Semon, to whom Rignano acknowledges his indebtedness. Bergson has familiarised philosophers with the notion. Still, Rignano is to some extent a pioneer. He has made an heroic attempt to reconcile conservative biologists to Semon's thesis by giving it the appearance of an experimental basis, and by eliminating its animistic and psychic aspects. In this attempt he is not likely to succeed; for mindless memory is not an attractive concept, and, it would seem, *la memoria biologica* is simply memory with the remembering left out.

Even if it fails in its main purpose, Rignano's study is useful and stimulating. Its message is—" Leave the shoals and shallows and return to the main stream; biology must not be diverted from the discussion of first principles by the attractions of departmental studies." The interest of the theme is somewhat impaired by faulty expression. Rignano has the requisite knowledge and penetration; but his mode of expression only suits a research record, and is quite unequal to a work of this scope. Repetition is his worst vice. He credits with memory all living things except his readers. For their benefit he repeats and recapitulates *ad nauseam*. Apparently he is resolved that his literary style shall exemplify one of the main biogenetic laws. For the ontogeny of his paragraphs is very often a résumé of the phylogeny of his chapters.

Prefacing his work with a powerful plea for collaboration between theorist and experimentalist, Rignano proceeds to give a lucid sketch of the evolution of evolutionary theory. He indicates his own position in the sequence of biological theorists, and makes it clear that the problem of acquired characters

is the starting-point of his own investigations. The transmissibility of some acquired characters seems at first sight the very datum of progress. How otherwise does evolution advance? How otherwise do species arise? How otherwise in the process of nuclear division does differentiation of structure arise? How otherwise can there be any truth in the axiom of transformism that heterogeneity comes out of homogeneity? Yet in spite of the *prima facie* evidence for the transmissibility, the opinion of biologists has been and is divided on the question. Lamarck taught it explicitly. Darwin, while not denying it, obscured it by laying almost exclusive stress upon the selective factor. Galton shook Darwin's position by focusing attention upon "particulate inheritance," and by experimental disproof of Darwin's theory of the circulation of gemmules. Thus he prepared the way for Weismann's theory of the continuity of the germ plasm, *i.e.* of the insulation of the germ from the soma, with its corollary of the absolute non-transmissibility of acquired characters. The neo-Darwinians and Weismannians seemed to have won the day. But further experiments, particularly those of Mendel, broke Weismann's spell and proved that natural selection cannot support the weight of the transformist theory. The continuity of the germ plasm is beginning to seem an unnecessary and unnatural supposition, and the abandonment of Lamarck's theory of the transmissibility of acquired characters is, in Rignano's opinion, a collective scientific aberration.

Those biologists who agree with Rignano on this point are at once confronted with a grave problem. What is the mechanism of the transmission of acquired characters? How can we conceive the functioning of germinal elements, at once isolated from the somatic and yet determinant of the somatic, relatively independent of the environment to preserve the type, yet sensitive to the action of the environment to acquire the differentia? The first part of Rignano's book is an attempt to answer this group of questions. He presents an elaborate theory of the process of organic development which he styles the theory of centro-epigenesis. He claims that it answers the chief difficulties attending the doctrine of the transmissibility of acquired characters, and that it leads on to the view of memory as the fundamental property of life. As the doctrine of centro-epigenesis is Rignano's most tangible contribution to biological theory, we must discuss it at some length.

Centro-epigenesis

The student of organic development is confronted, says Rignano, by three dilemmas, and the only way of escape between the horns of these dilemmas is that provided by the theory of centro-epigenesis.

The first dilemma is "preformism or epigenesis." According to preformism, nutrition is only an accessory factor in development. Theoretically, any portion of the embryo, detached from the organism, is able to continue its development. The facts which disprove preformism are those of regeneration. If the specific determinant of the leg, for instance, is present as such in the earliest embryonic stage, whence is drawn the material for the regenerate member? On the other hand, development solely by way of epigenesis is equally inadmissible. The experiments of Roux and Born in isolating portions of the embryo show conclusively that those portions contain within themselves all the elements necessary to determine their own development. Rignano's solution of this dilemma introduces the first element of the centro-epigenetic theory. All the apparently contradictory facts are reconciled, he says, by supposing that the plasmic action which determines growth starts from a definite part of the organism, which he names the central zone of development. If any portion of this zone is contained in the embryonic fragment, it renders that fragment capable of developing on its own account. He suggests that in the vertebrates this central zone may be composed of part of the spinal medulla.

The second dilemma concerns the constitution of the germ plasm. Is it composed of preformistic germs or of non-representative substances? The common fact of "particulate inheritance" seems to decide definitely in favour of preformistic germs. The ordinary phenomena of heredity, as well as the more striking phenomena of atavism and hybridisation, show that the smallest and most trivial characters can pass independently down the generations. It seems axiomatic that infinitesimal determinants of the several characters are present in the germ plasm. But, on the other hand, the theory of non-representative substances seems equally cogent. For the preformistic germs, if they exist, must be connected in the germ plasm according to a rigid system. Yet any such rigid structure of germs is ruled out by the facts of atavism. The occasional occurrence of the zebra-striping in horses, for instance, cannot be due to the random presence in the germ plasm of germs capable of producing black and white cells. It is inconceivable that the germ plasm could, through all its phases of increased size, multiplication, division, etc., preserve intact its supposed rigid structure.

As a solution of the second dilemma Rignano propounds the hypothesis that the germ plasm is constituted of elementary accumulators of vital energy. These accumulators are no doubt analogous to electrical accumulators. There is this essential difference. The discharge of electrical energy is general. The discharge of nervous or vital energy is specific. Consequently the activation of the fertilised ovum and of the other nuclei which go to form the central zone of development will take place by successive discharges of nervous energy in a given order from the first segmentation of the ovum to the adult state. Thus these accumulators, by being determinants, are sufficiently like preformistic germs to explain "particulate inheritance," and by allowing progressive transition from stage to stage of the ontogeny, they escape the objection to which preformistic germs are liable on the ground of rigid structure.

The third dilemma concerns the nature of cell division. When nuclear division takes place, are the daughter nuclei qualitatively identical with the parent nucleus, or does the specification of somatic function occur at this stage? Rignano maintains that experimental evidence supports both nuclear somatisation and the qualitative equality of nuclear division, and that again recourse must be had to an intermediate hypothesis.

Rignano is too brief here, and the lines of his hypothesis are not drawn with precision. The essence of it seems to be to admit the qualitative equality of nuclear division in the earliest stages of the ontogeny, but to deny it in the later stages. The nucleus of the fertilised ovum divides to form two nuclei, identical in quality with itself, containing all the germinal elements. Afterwards, as the development proceeds, somatic elements are added to the germinal element. The somatic elements are influenced by their relations to the other nuclei, and consequently acquire specific function. The somatic elements, which retain, like the germinal, the character of accumulators previously described, tend everywhere, except in the central zone of development, to substitute themselves for the germinals. In the lower animal organisms and in many vegetables the process of substitution would never be complete; so that beside the somatic elements in full functional activity, the germinals would continue to exist in an inactive condition. But this inhibition of their action is not permanent, and when the functional activity of the somatics is arrested by any cause, the germinals would again enter into action.

We thus see that the three momenta of the theory of centro-epigenesis are:

(1) That plasmic action radiates from a special zone of the organism, called "the central zone of development."

(2) That the germ plasm or nucleus of the fertilised ovum is composed of elementary accumulators of vital energy.

(3) That the germinal elements transmit themselves integrally from nucleus to nucleus; but that a true and proper nuclear somatisation takes place subsequently. For somatic elements, like the germinals in being accumulators, unlike them in being determined *ab extra*, attach themselves to the germinals and tend to displace them everywhere except in the central zone.

The three parts of the hypothesis are welded together by the supposition of a continuous circulation throughout the organism of vital or nervous energy along the protoplasmic filaments connecting the cells—a supposition which, in Rignano's opinion, has been verified by Pfeffer's experiments upon the trophic action of the nervous current.

The crux of the hypothesis is the formation of the central zone. Why should some nuclei take the lead, elevate themselves into a controlling position, and depreciate the remainder into a subordinate, differentiated, and somatic condition? On this point we find Rignano unsatisfactory. He speaks of some nuclei "fortuitously possessing better nutrition." It is certainly a weakness in the hypothesis that it has to invoke chance in such an essential matter. Further, the critic must ask: "Does the central-zone theory really explain what it sets out to explain? Does it not simply sweep the dust into the corner and declare the room clean? Does it do more than shift the *venue* of the problem from the organism as a whole to a hypothetical portion of it?" For instance let us consider the two definite merits claimed by Rignano for his theory. First that it renders intelligible the mechanism of the transmission of acquired characters. Owing to the nervous circulation, he says, new specific elements may deposit themselves in the nuclei, and those that reach the nuclei of the central zone will be preserved and will reappear in successive generations as acquired characters. Is this more than to say that the organism transmits acquired characters, because the central zone does so? Again, we are told that the theory explains the remarkable fact of the reappearance of the past history of the species in the development of the individual. For the ontogeny is the register of the process of the successive disturbances of the dynamic equilibrium, due to the activation of the germinal elements accumulated in the central zone. This seems like saying that the ontogeny of the organism recapitulates its phylogeny because the ontogeny of the central zone does so. Rignano himself says that the future of his theory of centro-epigenesis depends upon the results of a further study of the mysterious process of synapsis.

The reader of Rignano's book, after wrestling with this elaborate theory which occupies the first half, naturally asks, "What has this theory to do with the professed subject of the book, namely the mnemonic basis of life?" Here we touch the radical defect of the whole argument. The connection between centro-epigenesis and memory is of the flimsiest. Centro-epigenesis may be true. It may be true that memory is at the base of life. But the truth or falsity of the one theory has, we believe, no necessary connection with the truth or falsity of the other. Materialist and mnemonist, each is at liberty to accept or reject centro-epigenesis. Apparently our author has not made the connection between the two parts of the theory in his own mind. The reason is obvious. He hesitates between two interpretations of the word "memory." He is trying to ride two horses at the same time. Memory is physical when he is explaining growth, psychic when he is explaining thought. But we must hear Rignano himself. He claims that centro-epigenesis is a mnemonic theory because to regard the nuclei as accumulators is to regard them as memorising. They take in new elements, *i.e.* they learn new habits. They discharge, *i.e.* they recall. He points out, with a great deal of truth, that cellular specialisation, the habit-response to novel stimuli, reproduction, regeneration, the recapitulation of the phylogeny in the ontogeny, the transmissibility of acquired characters, are phenomena closely analogous to

memory. How far he is from interpreting "memory" in the natural sense is seen by his demand, as against Semon, that memory shall be localised. Now memory may be, and no doubt is, normally attached to space: it cannot be localised, *i.e.* made completely coterminous with a space-quantum. Yet Rignano "localises" the phylogenetic memory in the central zone. What sense is there in saying that the memory of the stages of reptile, fish, bird, etc., is *located* in a cubic inch of matter near Jones's spinal column?

Rignano in fact returns almost openly to the ordinary physical explanation of vital process. After basing centro-epigenesis upon "memory," he proceeds to base "memory" upon energy. He argues, for instance, that a colour sensation causes a specific nervous current which is stored in the nuclear accumulator. Then the "memory" of that sensation is the reactivation in an inverse direction of the original nervous current. Memory, in fact, in this section of Rignano's argument, is simply the alternation of current of charge and current of discharge. He lays down, it is true, that nervous energy is *sui generis*, and he takes "nervions" instead of electrons for the unit. But when he applies his bio-energetic theory, he treats nervous energy as if it were physical energy of an electrical type, and he ascribes a molecular structure to the specific nervous current which charges and discharges the accumulators.

The remainder of the book is in the nature of a compilation. The argument is not continuous. Rignano gives a sketch of Semon's theory of life, and devotes a chapter to showing that Francis Darwin should be classed as a mnemonist. True to the scientific principle of giving a fair hearing to all sides, he includes a long trenchant criticism of his own views from the pen of Bottazzi, an eminent biochemist. Besides this subsidiary matter, he deals with two subjects essential to the primary design of his work. These are finalism and bio-psychology. We can here only give a brief account of his treatment of these subjects. The biological theorist cannot escape the question of finalism. He must explain or explain away organic purpose. He must, that is, show that finalism is only a concept in the human mind arbitrarily imposed upon non-teleological process, or, if he admits objective purpose, he must relate it to his broad theory of life.

Here again Rignano's hesitation between two standpoints is fatal. He is unconsciously trying to run with the hare of mechanism and hunt with the hounds of finalism. He rejects Pauly's theory of the autoplasmation of the organism on the grounds that it is animistic, but he contends that no mechanistic explanation of the purposive action of the organism is possible. He sees that to call purpose "adaptation" is not to explain it, and that throughout the whole field of life we find not merely the adaptation of the organism to the external circumstances of the moment, but anticipatory adaptations, that is adaptations to conditions which as yet are non-existent. These anticipated ends are accounted for by Rignano on the supposition that each active state of dynamic equilibrium leaves a trace of itself in the accumulator. Thus system A, which has passed away except for this trace, may be restored, *i.e.* released from the accumulator, by the repetition of a portion of the external conditions that originated system A. If this vicarious action of the part for the whole is a fact in the organic world, if, that is, part of the cause can deputise for the whole, it certainly explains many of the phenomena of ontogeny and strengthens the argument for the mnemonic basis of life. For such vicarious action is a very close parallel to the memory cue. The power of a single word or other sensation to evoke from the subconscious mind a long train of thought is a fact of ordinary observation.

Still Rignano refuses to adopt the animistic position to which the conception of the end as "memory cue" naturally leads. He takes refuge in the neutral term "tendency." Now "tendency" is, strictly speaking, a teleological word. It means an unconscious end or an end not fully thought

out. Energy has no tendencies. Yet for Rignano the behaviour of organisms and the discharge of energy are both alike "tendencies." For instance, he lays down that hunger is the tendency to restore the normal nutritive environment. He means more than that the satisfaction of hunger does restore the nutritive equilibrium. How much more? Is he prepared to attribute the psychic property of appetition, *i.e.* action to realise an end, to the organism as a whole and in a lesser degree to its parts? If so, he has left his theory of energy-accumulators far behind.

When he deals with the question of the *vera causa* of organic change, he departs still farther from the mnemonic basis of life and empties the word "tendency" of all meaning. He does so mainly by the surreptitious introduction of equivocal concepts, such as "force" and "gravitate." Life, he says, gravitates towards an end. He regards the efficient cause as a *vis a tergo*, namely accumulated potential energy; the final cause as a *vis a fronte*, namely the suggestion of the memory cue. Now if a psychic or semi-psychic nature be attributed to the organism, there is at least sense in speaking of the final cause as a *vis a fronte*. It is as intelligible that the end "vision" should induce the organism to develop the mechanism of vision, as that ambition should induce a young man to carve out a career. But explain life on the basis of physics or energetics, and there is no longer any meaning in the term *vis a fronte*. Regarded as a force that pulls or as a point of attraction, it is purely an imaginary concept. It is nothing because it does nothing. Rignano wants to have it both ways. He tries to keep finalism and banish animism. He fails to see that final causes and anticipated ends must accompany animism into banishment. Experimental biology has pronounced the sentence of exile, and she will and must stand over it.

Rignano's bio-psychology, which rounds off his theory of life, is a *tour de force*. It is interesting as an indication of what may yet be accomplished in this new field; but as the former part of the book has failed to make the transition from the mechanism of the emotions to the emotions themselves, psychologists will hardly find the latter part convincing. Rignano's fundamental principle is that thought and will in all their modes derive from the emotions, or "affective tendencies" as he persistently calls them. With considerable ingenuity he detects and exhibits the feeling element present in all the higher forms of thought, in reasoning, in logic, in "the nebulosities of metaphysics," and in the "mathematical mysticism" of the relativists. He dismisses the problems of ethics in a few sentences, and holds out the hope that if we are good altruists we shall survive death in the form of specific nervous energy.

The conception of life as a form of memory is of first importance to the philosopher; we doubt its utility for the biologist. For those who would use Semon's thesis profitably, the time order must take precedence over the space order. Time and memory are intertwined. The philosopher is at liberty to attempt to think in terms of time. The biologist is bound by charter to think in terms of space. The philosopher may indulge his fancy for speculation. The biologist is confined to a definite practical programme. His task is not to penetrate the secret of existence, but to extend human power over the organic world. He does so by exploring and explaining in terms of matter those parts of the field of life which are amenable to the experimental procedure. In the process no doubt he becomes conscious of the limits of his science; but there he only shares the experience of his fellow-workers in neighbouring fields.

REVIEWS

MATHEMATICS

History of the Theory of Numbers. Vol. III. Quadratic and Higher Forms. By L. E. Dickson, wth a Chapter on the Class Numbers by G. H. Cresse. [Pp. v + 313.] (Washington: Carnegie Institute, 1923.)

This volume deals with many of the most important and the most difficult parts of the theory of numbers and is indispensable for those interested in this subject. It is not concerned so much with the consideration of single or isolated questions as of general theories. The necessity for these is frequently overlooked by a number of those interested in questions naturally suggesting themselves and apparently remote from any general theory. Thus the ideas utilised in the solutions in integers for x, y of

$$ax^2 + 2hxy + by^2 = m$$

where a, h, b, m are given, and the discussion both arithmetically and analytically of the processes and the functions involved, require for their systematic study some of the most important and general developments of the mathematics of the last sixty years. Even at the present day, the possibilities of such simple equations have not been exhausted and emphasise the necessity of continued study if research is to be successful or knowledge is to be up-to-date.

There is the obvious extension to quadratic forms of more than two variables requiring as detailed and delicate an investigation as is to be found in mathematical history. Then there are the forms of degrees higher than the second with two or more variables, to bilinear forms, to complex variables and coefficients, etc.

The fact that a considerable part of the theory and of the results have not yet found their way into the standard treatise adds greatly to the value of the volume and our indebtedness to Profs. Dickson and Cresse.

L. J. Mordell.

A Treatise on the Theory of Bessel Functions. By G. N. Watson, Sc.D., F.R.S. [Pp. vi + 804.] (Cambridge: at the University Press, 1922. Price 70*s*. net.)

Prof. Watson has produced a monumental treatise which is not likely to be superseded for years to come. It is large, expensive, and full of good stuff. It is very unlikely that any known formula relating to Bessel functions is not given somewhere in the book, but it is perhaps doubtful whether the mathematical physicist who has need of it will be able to find it in finite time. His attempt, however, will be profitable and instructive; his eye will be caught by some interesting theorem wherever he may happen to open the book, and he will read on, fascinated, forgetting perhaps his original quest, until either the subtlety of the logic or the complication of the formulæ becomes too much for him. And he will not only get information about Bessel functions, he will also find brief accounts of various branches of theory which are not given in the ordinary textbooks, but which are necessary

for special applications. Thus, on pp. 111–20 there is an interesting summary of Liouville's theory of the "elementary transcendental functions," without which it seems to be impossible to prove the impossibility of solving Riccati's equation "in finite terms" except in the classical cases discovered by Daniel Bernoulli; on pp. 229–35 there are accounts of the principle of stationary phase and the method of steepest descents; on p. 517 there is a note on Sturm's methods for investigating the oscillations of solutions of linear second order differential equations. The "general reader" also will find matter of interest.

He will learn (p. 77) how the relative positions of Pure and Applied Mathematics on the Continent as compared with this country are remarkably illustrated "by the attitudes adopted towards the Bessel functions of imaginary argument"; how Mittag-Leffler has laid down the principle "that it is, in general, undesirable to associate functions with the names of particular mathematicians" (p. 83); how the discovery by Stokes of the phenomenon of the discontinuity of the constants in asymptotic expansions of certain integral functions "was apparently one of those which are made at three o'clock in the morning" (p. 202). He may sympathise with Lord Kelvin's statement: "I have no satisfaction in formulas unless I feel their arithmetical magnitude" (p. 654); he will be amused at the sentence: "Researches based on the theory of integral equations are liable to give rise to uneasy feelings of suspicion in the mind of the ultra-orthodox mathematician" (p. 578); and he will be interested to find that the theory of Bessel functions, like most branches of modern analysis, has its unproved hypothesis—Bourget's conjecture that when n is a positive integer, the functions $J_n(z)$, $J_{n+m}(z)$ have no common zeros, other than the origin, for all positive integral values of m—and to read Prof. Watson's note: "I consider that the theorem is probably true; it is an abstruse theorem, and I have not succeeded in proving it."

A comparison with the new edition of Gray and Mathews is inevitable. The difference is obvious: Gray and Mathews give 110 pages of theory and 130 pages of applications; Watson gives about 650 pages of theory and the only applications of which there is more than a bare mention are contained in the historical introduction (Chapter I) and, somewhat unexpectedly, on p. 551, where the true, mean, and eccentric anomalies are defined.

The most striking superiority of Watson's book, apart from its greater detail and from the tables, is in his treatment of the complex theory, asymptotic expansions and functions of large order (Chapters VI–VIII). This, of course, was only to be expected by reason of the author's well-known valuable contributions to this part of the subject. A good part of Chapter VIII appears to be quite new; in particular, the asymptotic expansions of Bessel functions of large complex order are treated with more care and in more detail than in Debyé's memoir.

The tables, of which there are 87 pages, form a most valuable part of the book. A very great improvement in the usefulness of the tables of the Bessel functions of both kinds of order 0 and 1 is effected by means of auxiliary tables of the modulus and phase of the Hankel-Nielsen function $H_n^{(1)}(z)$. In these tables the first differences are sufficiently steady to enable interpolations to be effected with but little trouble. It is interesting to note (p. 662, note) that the idea of constructing the auxiliary tables grew out of a conversation with Prof. Love, who remarked that it was frequently not realised how closely Bessel functions of any given order resemble circular functions multiplied by a damping factor in which the rate of decay is slow. Other novelties in the tables are the inclusion of Struve's function in Table I, part of Tables II–IV, and Table VIII, which gives the values of the indefinite integrals of functions of order zero, together with their maxima and minima. In the note (p. 659) on tables of functions of order $\pm \frac{1}{3}$, $\pm \frac{2}{3}$ it might, per-

haps, have been pointed out that Dinnik's tables contain several misprints (see *Proc. Roy. Soc.*, A 100, 1922, 522).

One may, perhaps, be allowed to express a regret that Prof. Watson has not made a thorough examination of the symbolic methods of Heaviside, instead of contenting himself with the remark: "It is difficult to decide how valuable such researches are to be considered when modern standards of rigour are adopted."

F. P. W.

La Composition de Mathématiques dans l'examen d'admission à l'école polytechnique de 1901 à 1921. Par F. MICHEL et M. POTRON. [Pp. xii + 452.] (Paris: Gauthier-Villars et Cie, 1922.)

THE interest of this book for anyone outside France will be very slight indeed. It may possibly be of use to the examiner looking for questions, but he, presumably, will not require the very elaborate and detailed solutions which are here provided, nor will he need the second part, in which the questions are most curiously dissected, their component parts being then classified and numbered according to a complicated scheme. The questions are quite difficult, and are somewhat of the type which has sometimes crept into the Essay Paper of Part II of the Mathematical Tripos, the steps of à longish argument being indicated and the candidate being required to supply the proofs. There is a curious rigidity about them, too; the notation is carefully laid down and "les candidats conserveront toutes les notations indiquées."

P. F. W.

Calculus and Probability for Actuarial Students. By ALFRED HENRY, F.I.A. [Pp. iv + 152.] (London: Published by the authority and on behalf of the Institute of Actuaries, 1922.)

THIS book is the official textbook of the Institute of Actuaries, and as such has been written with the special requirements of the actuary in view. It begins with Finite Differences and gives numerous formulæ for interpolation by means of advancing, central, and divided differences. It then goes on to differential and integral calculus, developing the elementary rules of manipulation, naturally without any attempt at a proof. It is clearly written and excellently printed, and will no doubt prove useful for the class of students for whom it is intended.

F. P. W.

Dimensional Analysis. By P. W. BRIDGMAN. [Pp. iii + 112.] (New Haven: Yale University Press, 1922. Price 25*s*. net.)

APPLICATIONS of the principle of similitude or, as Prof. Bridgman calls it, "dimensional analysis" are common enough in mathematical physics, but are liable to leave one with a feeling of dissatisfaction. How are we to choose the list of physical quantities between which we are to search for a relation? When is it necessary to introduce "dimensional constants," such, for instance, as the constant of gravitation or the velocity of light? What kinds of quantity should be chosen as the fundamentals in terms of which to measure the others, and, in particular, how many kinds of fundamental units are there? It was with a view to suggesting answers to such questions as these that Prof. Bridgman delivered a course of lectures at Harvard in 1920, out of which this book has grown. He begins with a gossipy introductory chapter analysing a few typical problems, and then proceeds to a formal development of the subject. He defines a "complete" functional relation between certain measured quantities and certain dimensional constants as a relation of such a form that it remains true without any change in the form of the function when the size of the fundamental units

is changed in any way, and introduces the Π Theorem, first explicitly stated by E. Buckingham in 1914, but used before by Jeans (1905). If n quantities α, β, γ . . . are connected by *one* complete relation, and there are m fundamental units, then the relation must be of the form $F(\Pi_1, \Pi_2, \ldots) = 0$, where the Π's are the $n - m$ independent products of the arguments α, β, γ which are dimensionless in the fundamental units. The author insists that to apply the method we must know enough about the situation to know what the elements are which would be introduced in writing down the equations which determine the motion (in a general sense) of the system. "Dimensional analysis," he says (p. 52), "is essentially of the nature of an analysis of an analysis." Chapters of illustrative examples and applications to model experiments follow, and he concludes with a collection of thirty-two problems.

The book is, perhaps, rather long-winded, but it is very readable; one may not agree with all the author's contentions, but one will almost certainly agree with the remark (p. 5) that "the untutored savage in the bushes would probably not be able to apply the methods of dimensional analysis . . . and obtain results which would satisfy us."

F. P. W.

Géométrie Descriptive. Par Gaspard Monge. Les maîtres de la pensée scientifique. Collection de mémoires et ouvrages. Publiée par les soins de Maurice Solovine. Two volumes. [Pp. xvi + 144, 138.] (Paris: Gauthier-Villars et Cie, 1922. Price 6 frs.)

We welcome this addition to the excellent series of reprints in course of publication by Gauthier-Villars under the editorship of M. Solovine.

Monge is undoubtedly to be regarded as the founder of the science of descriptive geometry, and not only so—to him is due in great measure the revival of interest in pure geometry, both through his published works and through his activities as teacher at the École Polytechnique. His *Descriptive Geometry*, of which he conceived the ideas as early as 1775, was elaborated in lectures in 1794, but on account of its applications to military topography its publication was forbidden until 1798. The work is preceded by a "programme" in which, after declaring his aim—"tirer la nation française de la dépendance où elle a été jusqu'à présent de l'industrie étrangère"—he enunciates the two objects of the art: (1) representation of three-dimensional figures upon a plane, and (2) deduction of the properties of a figure from its representation. The method is that of double orthogonal projection, upon two planes at right angles which are then made to coincide by rotation about their line of intersection. The two projections of any point thus always lie upon a straight line perpendicular to this line of intersection, and Monge gives an easy method, still used to-day, for finding the distance between two points whose projections are given. The representation of a curve follows at once, but for a surface it is necessary to have recourse to its generation by the motion of a curve. It is most convenient to represent the surface by means of two systems of curves lying upon it, so chosen that through any point of the surface there passes one curve of each. The representation of planes, cones, cylinders, and surfaces of revolution affords interesting examples. The first section then closes with the solution of nine examples, concerned with planes and straight lines, which in simplicity leave nothing to be desired. The second section deals with tangent planes and normals to surfaces. The tangent plane at a point of a surface is determined by the tangents at that point to the two generating curves; Monge does not prove, or mention the necessity of proving, that the plane is independent of the choice of these curves. After constructing tangent planes to various surfaces, he determines the shortest distance between two skew lines by means of the tangent planes of a circular cylinder; he leaves the more elementary construction as an exercise to the reader. There

follow constructions for tangent planes satisfying other conditions, such as passing through a given line, or touching more than one surface; the case of spheres leads Monge to two digressions, on polar properties with regard to circles, conics, and quadrics, and on centres of similitude of circles and spheres. He then (Section IV) treats of the intersections of curved surfaces, prefixing some remarks on the correspondence between the operations of algebra and those of descriptive geometry. His method for finding the curve of intersection of two surfaces F′, F″ is to cut them by a series of auxiliary surfaces F, usually planes or spheres, and determine the intersections of the curves FF′ and FF″. This, with Monge's examples, is reproduced without essential change in all subsequent textbooks on the subject. The section ends with an account of Roberval's method for drawing tangents to a curve, and an attempt to extend this to space curves; unfortunately the curve which he gives as an example (p. 140) is not a space curve. The fourth section (t. ii) gives a variety of applications both of theoretical and practical interest; the first referring to the determination of the centres of the in- and circum-scribed spheres of a tetrahedron, and the others to supplementing maps by means of angle observations. Here the descriptive geometry, strictly so-called, ends, but there is a further section dealing with the properties of developable surfaces, with the curvature of surfaces and with lines of curvature, matters which form the basis of his *Applications de l'analyse à la Géométrie*. In his lectures Monge also dealt with the applications of descriptive geometry to the theory of shadows and to perspective; after his death his notes thereon were edited by his pupil, B. Brisson, and published at the end of the fourth edition of the *Géométrie*; they are reproduced in this reprint.

F. P. W.

Cours Complet de Mathématiques Spéciales. Par J. Haag. Tome III. **Mécanique.** [Pp. viii + 191.] (Paris: Gauthier-Villars et Cie, 1922, Price 12 frs.)

In a series of works Prof. Haag intends to set forth the main facts and theories of mathematics and mechanics as required by technical and other students. Volumes I and II have already appeared, dealing with Analysis and with Geometry: the present volume deals with Statics and Dynamics. Volume IV, it is mentioned, will contain Descriptive Geometry and Trigonometry.

Less than two hundred pages are devoted to the principles of mechanics. It is, therefore, not to be expected that the author would go into any kind of detail as regards applications of these principles. He does, it is true, give some applications, as, for instance, to simple harmonic motion, central orbits under the inverse square law, some of the easier types of problems in statics, notably the simple machines. But on the whole the spirit of the book is essentially theoretical.

A brief study of kinematics of points and rigid bodies, based largely on the vectorial method already developed in a former volume, is followed by a statement of the fundamental principles of dynamics with application to the motion of a particle. General theorems applicable to dynamical systems are then studied, and the question of units and dimensions is discussed. A brief account of statical principles follows—notably virtual work—with some easy examples.

The book is in no sense a textbook—one cannot conceive a student learning his mechanics from it. It is another example of the type of books that French writers seem to delight to produce, and which are a delight to all to read. The student of mechanics who takes the trouble to study Prof. Haag's treatment of the subject will not thereby be helped to use the principles of the subject more subtly or with greater ease, but he will be encouraged

to use them with more certainty owing to the clarifying effect of the discussion presented to him.

The author emphasises the experimental basis of the principles of dynamics, and declines to be involved in any philosophic discussion of their origin or validity. He bases himself to a considerable extent on the teaching of Painlévé, using the notions of relative and absolute forces due to him. One feels grateful to the author for not omitting to mention that the fundamental equation in central orbits is due to Binet, and not neglecting to give a brief account of rolling friction, including friction at a pivot: instead of giving the student the impression created by so many English books, that only sliding friction exists.

ASTRONOMY

Cours de Mécanique Céleste. Par M. H. ANDOYER, Membre de l'Institut et du Bureau des Longitudes; Professeur à la Faculté des Sciences de Paris. Tome I. [Pp. vi + 439.] (Paris: Gauthier-Villars, 1923. Price 50 fr.)

THE volume under review is the first of two which together are to constitute a complete course in celestial mechanics. The whole work is divided into six sections, which deal respectively with the general problems of celestial mechanics, the practical study of the Keplerian motion and of perturbations, the theory of the planetary motions, the theory of the moon, that of the movement of the earth and moon around their common centre of gravity, and the theory of the major satellites of Jupiter. The three former sections are contained in the present volume, the three latter will constitute the second volume.

The work is planned on different lines from existing treatises on celestial mechanics. It is written for the practical computer by one who has had much experience in this field. The theoretical methods explained are those which the author considers to be the most useful for the purpose, and the method of adaptation to numerical computation is fully explained. Such subjects as methods of interpolation, and the solution of Kepler's equation are discussed in detail, and where necessary numerical tables are included to facilitate computation. It contains, therefore, all that the practical astronomer is likely to want in the way of theory and in the actual methods of computation.

H. S. J.

The Marine Chronograph. Its History and Development. By LT.-COMMANDER R. T. GOULD, R.N. (retd.), F.R.G.S., with a foreword by SIR FRANK DYSON, LL.D., F.R.S., Astronomer Royal. [Pp. xvi + 287, with 39 plates and 85 figures.] (London: J. D. Potter, 1923. Price 25s. net.)

THE history and development of the marine chronometer have been closely bound up with the problem of finding the longitude at sea. The determination of a ship's latitude is a comparatively easy matter; it is evident that an observation of the altitude of the pole star will suffice in the northern hemisphere, as the altitude decreases steadily from 90° at the pole to zero at the equator. The meridian altitude of the sun or of a star also determines the latitude. But the problem of finding the longitude is a different matter. If a ship is sailing due east or due west, the aspect of the heavens is practically the same at the same local time wherever she may be. The most important exception to this statement is the moon, which on account of her rapid change of position relatively to the stars can serve as a clock by which longitude or time (measured relatively to some fixed place) may be determined. The longitude may also be found if the ship can carry with her the time at Greenwich or some other fixed place. The two principal directions along which the solution of the problem of determining longitude at sea was sought were

therefore by the method of lunar distances and by the construction of a timepiece whose rate would be sufficiently regular for the longitude to be deduced without serious error. The method of lunar distances has gradually receded into the background, as it possesses the disadvantage that a small error in the observed distances gives rise to a relatively large error in the deduced longitude.

Before the development of the marine chronometer, the method generally used to reach a given port was to get into the correct latitude at some distance to the east or to the west of the port and then to sail along a parallel of latitude until the required port was reached. But not infrequently it would happen that the dead reckoning was so much in error that the ship would actually be on the wrong side of the port and would be sailing in the wrong direction, with the result that many days' sailing would be lost. It was the pressing need for a solution of the problem of finding the longtitude which induced the British Government in 1714 to offer a prize of £10,000 for any method capable of determining a ship's longitude within one degree, of £15,000 if it determined it within 40′, and of £20,000 if it determined it within half a degree.

Commander Gould gives an historical account of extreme interest of the various attempts to construct a timepiece suitable for use at sea, from the Nuremberg egg to Harrison's No. 4, which actually obtained the £20,000 prize. With this timepiece the history of the modern chronometer may be said to have commenced. Considerable space is therefore given to an account of Harrison and of his successive timepieces and of the struggle to obtain the award after he had actually won it. The latter reflects discreditably upon the Board of Longitude, which was the scientific body set up to consider the inventions submitted to it. The matter is discussed impartially by the author. It may here be mentioned that great care has been taken to obtain accuracy and original documents have been consulted wherever possible. As an example of the care which has been taken the reference to the very rare work by Sully quoted on p. 36 may be instanced. To this there is the following footnote: "There is a perfect copy of this work in the Vulliamy Collection. The British Museum copy only contains about a quarter of the complete work."

The account of Harrison is followed by accounts of the early English and French makers, Kendall and Mudge, Le Roy and Berthoud, Arnold and Earnshaw, by whose time the chronometer had to all intents and purposes assumed its modern form. Many improvements in detail followed and these are best considered with reference to the various parts of the mechanism concerned, the escapement, the balance, etc., rather than by an historical treatment. This course is therefore adopted. At the end is given a brief account of the modern chronometer and of its care.

The work has been a labour of love with Commander Gould. That is at once apparent on reading the book. It contains a vast amount of information and can fittingly go on the bookshelf alongside Britten's *Old Clocks and their Makers* as a standard work of reference. But one must not infer from this that the work is a mere encyclopædia. It is a volume of intense interest. All technical terms are explained as they arise so that no technical knowledge on the part of the reader is necessary, although much will be acquired in the course of the reading.

It may be mentioned that Commander Gould has not merely a paper knowledge of clocks. He has also the technical skill of the maker, and during the preparation of this book he voluntarily undertook to clean Harrison's first and fourth machines, which are kept at the Royal Observatory, Greenwich.

We congratulate the author upon his valuable book, which should make a wide appeal.

H. S. J.

The Meaning of Relativity. By ALBERT EINSTEIN. Translated by EDWIN PLIMPTON ADAMS, Professor of Physics, Princeton University. [Pp. v + 123.] (London: Methuen & Co., 1922. Price 5*s.* net.)

IN this volume is given a translation of four lectures which were delivered by Prof. Einstein at Princeton in America during 1921. The subjects of the individual lectures were: (1) Space and Time in Pre-relativity Physics. (2) The Theory of Special Relativity. (3) and (4) The General Theory of Relativity. The four lectures together give a brief but connected account of the whole theory. The argument is mathematical, and, because of its conciseness, is not suitable for a reader approaching the theory for the first time. Those, however, who are familiar with tensor calculus and have already some knowledge of the theory may read the volume with profit. As may be expected from Einstein himself, the exposition is admirably clear, and the translation is a good one. One statement may be disputed. On p. 101 it is stated, in reference to the displacement towards the red of the solar spectral lines required by the theory that " results obtained during the past year seem to make the existence of this effect more probable, and it can hardly be doubted that this consequence of the theory will be confirmed within the next year." It is very probable that the confirmation of this deduction will be long delayed yet.

The printing of the mathematics is not above reproach. In particular, it is misleading to print the square root of minus unity, commonly denoted by i, as $\mathbf{i}$. This type is by fairly general consent reserved for vectors or tensors, and in fact a vector $\mathbf{i}$, the electric current, is used in this book. As examples of the sort of errors which are rather common in the volume may be mentioned that on p. 15, line 4 from bottom, the right hand $A_{\mu\nu\rho}$ should read $A_{\nu\mu\rho}$. On p. 55, line 10, T_{u} should be $T_{\mu\nu}$, and in equation 49, d_3 should be dx_3. On p. 72, equation 57, ∂x_μ should be ∂x^1_μ. On p. 74, line 8, $A^{\beta}_{\alpha 4}$ is omitted from the middle equality.

H. S. J.

The Theory of Spectra and Atomic Constitution. Three Essays by NIELS BOHR, Professor of Theoretical Physics in the University of Copenhagen. [Pp. x + 126, with figures.] (Cambridge: at the University Press, 1922. Price 7*s.* 6*d.* net.)

THIS volume contains the translations of three addresses given by Prof. Bohr. The first, entitled " On the Spectrum of Hydrogen," was given before the Physical Society of Copenhagen in 1913. The second, entitled " On the Series Spectra of the Elements," was given before the Physical Society of Berlin in 1920, whilst the third, on " The Structure of the Atom and the Physical and Chemical Properties of the Elements," was given before a joint meeting of the Physical and Chemical Societies of Copenhagen in 1921. The first two addresses are reprinted as delivered, the third has been slightly modified in one or two minor details. The three essays, dealing as they do with closely related subjects, necessarily overlap somewhat, and as they correspond in time of delivery to different stages in the development of Bohr's theory of spectra, they are not in some details in mutual agreement. It is a pity that the three lectures were not recast into one homogeneous whole, which would have avoided repetition and contradiction, and within the same compass would have allowed of fuller treatment on certain points which have had to be passed over very briefly. We hope, however, that it may yet be found possible by Prof. Bohr to give a full and connected account of his brilliant theories of atomic constitution.

These theories are based upon the quantum conception of energy which is necessary in order to account for the observed law of radiation of a black body, which classical mechanics completely fails to do. Bohr adopts the

atomic model of Rutherford, and supposes that radiation and absorption occur when an electron jumps from one orbit in the electron to another, a definite amount of energy being given out or absorbed in the process. With this conception he couples that which he terms the correspondence principle. According to this principle, if the motion of the atom is analysed into harmonic components, the possibility that any particular transition from one electron orbit to another may occur is due to the presence of the corresponding harmonic component in the motion. This principle, which is really empirical, but which is found to be a safe guide and to accord with experience in such instances as it has been tested, provides a general law for determining the occurrence of transitions between the various stationary states. It is a very powerful weapon for determining which transitions actually take place, and in Bohr's hands has enabled him to give a rational explanation of the periodic table and to predict the main physical and chemical properties of the elements which correspond to the gaps in the table. It may be recalled that since the publication of this book, one of these elements, called hafnium, has been discovered, and its properties are found to correspond with Bohr's predictions. These subjects are dealt with in the third essay.

The book is non-mathematical, and can therefore be read with profit by the general reader. It tells a fascinating and remarkable story, and we are grateful to Prof. Bohr for having provided this account of his theories, for such an account has not hitherto been available in English.

H. S. J.

PHYSICS

Atomes et Électrons. Rapports et Discussions du Conseil de Physique tenu à Bruxelles du 1er au 6 Avril 1921. [Pp. 271.] (Paris: Gauthier-Villars et Cie, 1923. Price 20 frs.)

It is now just eleven years since the great Belgian industrialist, Ernest Solvay, convoked an international conference of physicists to discuss the effect on their science of the invasion of that disconcerting stranger, the Quantum. Shortly after, he founded the International Institute of Physics, whose task was "d'encourager des recherches qui soient de nature à étendre et surtout à approfondir la connaissance des phénomènes naturels." Since that time two further "Solvay Conferences" have been held, and this volume contains the reports presented and discussions held thereon at the third conference, which took place in April 1921.

These reports and discussions centred, as the title suggests, round the properties of atoms and electrons. The volume opens with some notes by Prof. Lorentz on the theory of electrons, emphasising the difficulty of reconciling the current views on the structure of the atom with the absence of radiation and stability. Lorentz, however, seems to think that it is not quite impossible to relegate this difficulty to the interior of the atom and still maintain Maxwell's equations for describing the phenomena of the surrounding space. Sir Ernest Rutherford presents a report on the structure of the atom, describing the nuclear theory and discussing the question of the dimensions of the nucleus. A short account of his recent work on artificial disintegration of the elements is given together with a reference to the existence of isotopes. A brief reference to the structure of the nucleus concludes this report. It is followed by one written by M. de Broglie, in the first part of which he deals with the photo-electric effect, using that term in its broadest sense. In the second part he treats the inverse photo-electric effect, *i.e.* the production of radiation by the impact of electrons on atoms, and resonance potentials. The report illustrates the importance of the well-known Einstein law and discusses the difficulties occasioned by the appearance of "condensation" of energy of radiation into the electrons. The pressing nature of this difficulty is exemplified by the animated discussion which follows this report.

The startling change in properties which takes place in matter at very low temperatures is one of the most remarkable of recent discoveries. It is but natural that the director of the famous cryogenic laboratory at Leiden, Prof. Kamerlingh Onnes, should expound the present state of our knowledge on these matters. He presents two reports, one on paramagnetism at low temperatures, and one on the puzzling phenomenon of the superconductivity which makes its appearance in metals as their temperature approaches the absolute zero. Sir William H. Bragg and Prof. de Haas contribute two short reports, the former on the intensity of reflection of X-rays by diamond, the latter on the theory of the relation between the magnetic moment of a magnetised body and its intrinsic angular momentum and the experimental work verifying this relation. The volume concludes with two reports by Prof. Bohr and Prof. Ehrenfest on the application of the Quantum Theory to atomic problems and on the " Principle of Correspondence " with which Bohr has recently enriched the postulates of the Quantum Theory.

It is impossible to overrate the importance of such periodic gatherings of the foremost physicists of the world. The subject is so ramified that the general worker needs to have his attention focused at times on the difficulties which confront the several avenues of research. He soon learns that they are all various aspects of the perennial paradox of intellectual life, the contest between continuity and discontinuity.

It is sad to relate that M. Solvay, by whose munificence the International Institute of Physics came into being, died just at the time when this Report was published.

J. R.

Physique Elémentaire et Théories Modernes. Première Partie. Par J. Villey. [Pp. x + 197.] (Paris: Gauthier-Villars et Cie, 1921. Price 15 frs.)

This is the first of two volumes forming a work on Elementary Physics, in which the author makes the reading of the subject more pleasant by the infusion of ideas from modern physical theories. The book is not a textbook in the accepted sense of the word. It makes a wider appeal, expounding and explaining the body of essential phenomena which no intelligent person can afford to overlook, surrounded as he is by such numerous industrial applications of the physical sciences. By adopting a method of exposition which is not burdened with too much detail the author contrives in 200 pages to deal in an interesting manner with Mechanics, Hydrostatics, Heat, Elasticity, and Sound. The sub-title of this part, " Molécules et Atomes," indicates the fact that the last chapter of the volume is devoted to an outline of the atomic and molecular theory of matter and the manner in which it co-ordinates the explanations of the phenomena dealt with previously.

J. R.

Le Principe de Relativité et la Théorie de la Gravitation. Par M. Jean Becquerel. [Pp. iv + 342.] (Paris: Gauthier-Villars et Cie, 1922. Price 25 frs. net.)

It is now eighteen years since Einstein, with the audacity of genius, made the first of those two great steps which have resulted in a theory that has revolutionised the fundamental notions on which Mechanics and Physics are based. For the greater part of that time the literature of Relativity naturally consisted in the main of communications to various scientific journals; but within the past three years the output of books on the subject has been amazing. To be sure, the majority of these have been of the popular or semi-popular type, and the number of systematic and formal treatises on the subject have been comparatively few. The volume before us is a welcome addition to that small group of works. It is a systematic

treatment of the Relativity principle, the first part of which is devoted to the restricted Relativity of 1905 with its reactions on the treatment of Dynamics and Electromagnetism, and with its introduction of the new concept of an "interval of Space-Time." In the second part the necessity for generalisation in view of the phenomena of gravitation is explained, and the close connection between the mathematical treatment entailed by the generalised principle and the theory of multi-dimensional manifolds expounded. A full account of the application of the tensor calculus to Dynamics, Gravitation, and the Electromagnetic Field follows. A chapter is devoted to those speculations concerning the finite extent of the universe which arose in the first instance as a means of surmounting the difficulties entailed in the treatment of boundary conditions (difficulties not unknown in philosophic considerations of the Newtonian system) and which, as M. Becquerel shows, have an important bearing on a consistent view of the electric constitution of matter. The book is brought up to date by a final chapter on the theories of Weyl and Eddington as to the union of the field of gravitation and the electromagnetic field and the identification of the tensors which represent the physical concepts based on experiment and observation with the tensors which arise naturally in a deductive geometric treatment of a four-dimensional manifold.

The book is written in that direct and lucid style which is characteristic of the French physicists and mathematicians. One misses the picturesque language and the semi-mystical, almost whimsical, flashes of Prof. Eddington's latest work; but the business-like way in which M. Becquerel strives to make straight the rough road to Relativity will amply repay any English student, familiar with the French language, for a perusal of this very interesting book.

J. R.

An Introduction to Electrodynamics. By LEIGH PAGE, Ph.D. [Pp. vi + 134.] (London and New York: Ginn & Co. Price 10*s*. 6*d*. net.)

UNLIKE the rest of the books on the electromagnetic theory, the author deduces the fundamental equations of electrodynamics directly from the principle of relativity. In this way he is able to treat the subject at once rigorously and lucidly. The lucidity of the treatment is further increased by the employment of vector methods. The required theorems in vector algebra are described in an introductory chapter, the notation used being that of Gibbs. Purely vector methods are not always used, as, for example, in the deduction of the equation of Fresnel's Wave Surface and the Faraday and Zeeman effects. Admirable vector treatments of the former of these were given long ago by Hamilton and Tait, using linear vector functions, which here go under the name of dyadics. There are many novel touches given throughout the work which may be best gathered by reading the book.

The book is to be recommended as a sound and interesting treatment of electrodynamics.

J. R.

Atoms. By JEAN PERRIN. Authorised translation by D. Ll. Hammick. [Pp. xv + 230.] (London: Constable & Co., 1923. Price 8*s*. 6*d*.)

THIS translation of Perrin's famous book constitutes the second edition in English. The first English edition appeared in 1916, but the work has in the meantime gone through several editions in French. The present translation is based on the eleventh French edition and has had the advantage of a special revision by Prof. Perrin, who has added some new matter.

As one makes a fresh acquaintance with the pages of this masterly little work, with its brilliant array of the powerful evidences for atomicity in matter and electricity, one wonders will it ever be possible for any physicist of

the future to take up once more the attitude (now practically forgotten) of scientists and philosophers such as Ernst Mach and Wilhelm Ostwald, who, in the latter part of the nineteenth century, regarded the atomic hypothesis as merely a transient phase of general physical and chemical theory. At the moment the atomic theory has triumphed. One by one its opponents have abandoned their sceptical position, and even Ostwald, the most stubborn of them, has at last been converted. To many readers of this journal, especially the young, it may come as a shock to learn of the existence of this once powerful school of "energetists" who regarded atomism as an unverifiable and unnecessary hypothesis. The writer would suggest to such readers that, if they were suddenly faced with the task of collecting and classifying from their general reading of the literature of Physics and Chemistry the observational data and reasoned arguments upon which belief in atoms rests, it might prove a rather difficult task. However, they can take heart. Here in the compass of rather more than 200 pages the task is achieved in a manner that compels the utmost admiration for the gifted author.

One of the new features of this edition is an account of the author's speculations as to the activity of radiation in provoking chemical reaction, *viz.* that the essential mechanism of all chemical reaction is to be sought in the action of light (in the general sense of radiation) upon atoms. The reader is warned that this theory (developed independently in England by Prof. W. C. McC. Lewis) is looked on with disfavour in certain quarters. It certainly has some difficulties to meet; but it seems equally difficult to suggest a suitable alternative hypothesis, at all events in the case of unimolecular reactions.

The translation is well done and the style and appearance of the book are attractive.

J. R.

CHEMISTRY

Vat Colours. By J. F. Thorpe, C.B.E., D.Sc., F.R.S., Professor of Chemistry in the Imperial College of Science and Technology, and C. K. Ingold, D.Sc. (Monographs on Industrial Chemistry.) [Pp. xvi + 491.] (London: Longmans, Green & Co., 1923. Price 16*s.* net.)

Vat colours occupy a unique position in being at once the oldest and the newest class of dyes known to man.

The earliest representative of the class, indigo, was known to the Egyptians at least 5000 B.C., and as woad to the early Britons, whilst Tyrian purple (6:6′-dibrom-indigo) was also known to have been in use in 1500 B.C. The peculiar manner of dyeing in a fermented "vat" and the extraordinary fastness of the resultant dyeings made these two colouring matters of special interest in the early days, but they remained unique for many thousands of years until towards the end of last century, when the genius of von Baeyer elucidated the structure of the indigo molecule and showed how the dye could be made synthetically.

Since that time a continuous stream of new vat dyes—chlor- and brom-indigos, indanthrenes, algol colours, thioindigos, hydron blue, and so on—has been pouring forth from research laboratories so that by 1914 the patents alone were to be counted by the hundred, with scientific literature to correspond. But for the war there is little doubt that the absolute monopoly in vat-dyes held by the German and Swiss firms would have been as complete as any monopoly could be.

Fortunately for this country, the great efforts made here to re-establish the dye industry have met with a large measure of success, so that many of the very latest and most complex of the vat colours are now manufactured here on the large scale.

In view of the enormous literature of the subject, all attempts to summarise our present knowledge must be welcomed, as at the moment—apart from the present work—Truttwin's *Enzyklopædie der Küpenfarbstoffen* and Friedlaender's *Fortschritte* are the only works that can be consulted, so that the authors of *Vat Colours* have endeavoured to fill a very noticeable gap in chemical literature.

The contents are divided into two main sections, the first dealing with indigo and its cousins, and the second with dyes derived from anthraquinone, and related dyes; the first three chapters, on the history of natural indigo and Tyrian purple, are very interesting.

The careful subdivision of the anthraquinone dyes should greatly facilitate the task of searching for any particular class of dyes.

The value of the book is, however, somewhat marred by various errors of omission and commission: thus, reference might have been expected to such matters as hydron blue, pyrazole-anthrone yellow, the remarkable series of new indigoid vat dyes discovered by Cassella & Co., and to the recent class of vat dyes derived from simple quinones. No mention, again, is made of the synthesis of indigo from dianilido-succinic acid, nor of the remarkable synthesis of phenyl-glycine directly from aniline and trichlorethylene discovered in the laboratories of the British Dyestuffs Corporation.

The following errors may also be noted: the formula for indigo on p. 64 is incorrect; the nitration of para-cresol (p. 103) does not give the nitro-compound indicated but its isomer; on the same page *methoxy-* is given instead of *hydroxy*-nitrobenzaldehyde. On p. 195 algol scarlet G and algol pink R (translated as algol rose R) are stated to be the "products obtained from the other hydroxybenzoic acids and from the methoxybenzoic acids." This is, of course, incorrect; the hydroxy and methoxy groups are on the anthraquinone nuclei and not on the benzoyl nuclei. On p. 417 the dye derived from acenaphthalimide is described as a "strongly coloured *red-violet* vat dye," and later as a member of a class of "powerful *violet* dyes." Actually this colour—Kardos' "aceanthrene green"—dyes cotton an intense *emerald-green* from a violet vat.

The purpose of Part IV, "Preparation of Intermediates for Vat Dyes," is not altogether clear, as the methods given are in many cases of little practical value. Anthranol, for example, is not made at the present time by reducing anthraquinone with tin and hydrochloric acid, nor is *o*-nitro-*p*-cresol prepared on the large scale from dinitrotoluene in the manner described.

The moderate price of the book is a pleasant surprise having regard to the large number of complicated formulæ and the general excellent printing, and the work will doubtless be perused with interest by organic chemists.

F. A. M.

The Destructive Distillation of Wood. By H. M. Bunbury, M.Sc. [Pp. xx + 320, with 115 illustrations.] (London: Benn Brothers, 1923. Price 35*s*. net.)

As stated in the preface, no English book dealing exclusively with wood distillation has yet appeared, and this in spite of the fact that the destructive distillation of wood has been practised for generations in Europe and America and stands second in importance only to that of coal. The book deals exhaustively with every aspect of the subject; besides describing the plant and methods of modern wood distillation practice, it contains chapters devoted to the physical and chemical properties of wood, the factors influencing the thermal decomposition of wood and the properties of the resulting products, the production of illuminating and power gas from wood, etc. The information regarding the chemistry of wood is quite up-to-date, including as it does a description of the most recent views on the constitution of the cellulose unit, and also such views as are at present put forward regarding the consti-

tution of lignin. The chapter on the thermal decomposition reactions contains some rather rash speculations regarding the mechanism of the process, including the implied statement on p. 132 that glucose (the hexose from cellulose) yields mucic acid on oxidation; incidentally, also, the formula given for a pentosan on p. 131 is that of a pentose. The last chapter deals with analytical methods employed in the analysis of the crude and refined wood distillation products; it is perhaps permissible to express a regret that the author did not see his way to extend this chapter to include details also of the methods evolved in America, more especially by Schorger and Dore, for the complete analysis of wood, especially in view of the fact that many of the results obtained by those authors are quoted in tabular form. But where there is so much that is good it is perhaps ungracious to ask for more, seeing that the text is supported throughout by a liberal supply of analytical results and other data together with full references to literature. The book is attractively got up and well illustrated, and may be recommended as a storehouse of information on the subject of wood distillation and all its ramifications.

P. H.

Fundamentals of Biochemistry in Relation to Human Physiology. By T. R. Parsons, B.Sc., M.A. [Pp. x × 281.] (Cambridge: W. Heffer & Sons, 1923. Price 10*s*. 6*d*. net.)

The author has sat down with the clear purpose of painting a broad canvas uncrowded by detail. He has felt that, whilst there are admirable treatises of biochemistry for those who have time to get to grips with their bulk, there is the lack of a book of the dimensions demanded by the first beginner or the passing student. Mr. Parsons has had in mind the presentation of what biochemistry has set out to understand and what sort of distance it has got on the road to understanding. So much is gathered from his introduction, in which he hopes that "any merit my book may possess may result from its containing rather less information than more than other books contain."

It is fortunate that the discussion of such problems as metabolism and the energy changes of the organism, of nutrition and respiration, present no difficulties to the working out of this principle and lose nothing in the process. But when one turns to the chapters on the chemistry of the carbohydrates, proteins, and fats one sees the author in difficulties. He will not be mastered by a mass of data, whilst he is conscious that without such matter these chapters add little to the picture.

However, a book which has one defined object must be kept within the prescribed limits of size, and this object has been quite delightfully achieved. We know of no better book to put into the hands of a student of some previous scientific training who, for the first time, is turning his attention to physiological or medical problems. He will not then lose his sense of proportion in the larger books and in the monographs it will be his business to handle. The medical student, likewise, would do well to relax awhile in these pages, and he will return to his textbooks with heart of grace. Indeed, a larger public will find here much that is fascinating in the romance of Life.

R. K. C.

Oxidations and Reductions in the Animal Body. By H. D. Dakin, D.Sc., F.R.S. Monographs on Biochemistry. Second Edition. [Pp. ix + 176.] (London: Longmans, Green & Co., 1922. Price 6*s*. net.)

Since the appearance of the first edition eleven years ago our views on the mechanism of oxidation have undergone considerable change; even such an apparently simple oxidation as that of carbon monoxide to carbon dioxide involves the intermediate formation of formic acid, hydrogen and hydrogen-

peroxide, and, as stated by the author "a closer analysis of biochemical oxidations will reveal similar analogies, and indeed they are already being discovered." The second chapter has been extended to include an account of the more modern "dehydrogenation" theory of oxidation as expounded by Wieland and others, and considerable space is also devoted to the work of Hopkins upon Glutathione. The chapter on the oxidation of carbohydrates has been substantially rewritten to include the advances made in this field of investigation, and the opportunity has been utilised for giving an account of the constitution of the various modifications of glucose which are now recognised. Despite the numerous valuable contributions which Dr. Dakin has made to our knowledge of the formation of intermediate products of oxidation with the use of hydrogen peroxide *in vitro*, he explicitly states his disbelief in the view that this substance is the active oxidising agent in the animal body. Hydrogen peroxide may be formed in small amounts during the processes of auto-oxidation, and moreover when oxygen acts as an acceptor for hydrogen, hydrogen peroxide and not water is the initial product, but in the author's opinion the old idea that catalase may serve to prevent excessive accumulation of hydrogen peroxide has a good deal to recommend it. The book is written in an attractive manner, and must be consulted by all who wish to obtain an insight into the very complicated subject of tissue oxidations.

E. H.

Practical Plant Biochemistry. By MURIEL WHELDALE ONSLOW. Second Edition. [Pp. 194.] (Cambridge: The University Press, 1923. Price 12*s*. 6*d*. net.)

To students of plant chemistry this little book is already well known, and that it should have already reached a second edition is evidence of its popularity. The general arrangement and size of the new edition is essentially the same as that of the first. The chief new features are the inclusion of a chapter on vegetable acids and a somewhat different treatment of the subject of oxidising enzymes.

A Comprehensive Treatise on Inorganic and Theoretical Chemistry. By J. W. MELLOR, D.Sc., Vol. III, Cu, Ag, Au, Ca, Sr, Ba. [Pp. x + 927, with 158 diagrams.] (London: Longmans, Green & Co., 1923. Price 63*s*. net.)

DR. MELLOR continues to achieve the almost impossible task he has set himself, and the third volume of his treatise fully maintains the standard of its predecessors. Copper, silver, and gold are discussed and the group of the common alkaline earths; the radium and actinium families are to be dealt with at the beginning of the fourth volume, so that the issue of this will be awaited with special interest to see what Dr. Mellor has to say on this fascinating but complex group of elements.

An amazing amount of material is included, and most of the attempts to catch the author out by seeking for unlikely substances or reactions which might pardonably have been overlooked ended in the discomfiture of the reviewer! There are, of course, one or two omissions and misprints. Thus, there is no reference under copper to the important contribution of Sidgwick and Tizard to the solution of the problem of the blue colour of copper salts; but such omissions are rare, and the enormous number of references, ranging from the book of Genesis to scientific papers published in 1922, indicates the encyclopædic character of the work.

The book is quite readable (it is almost unnecessary to add that it is very well and clearly printed), and the absence of the quotations and headings with which we have become familiar in the author's writings is, on the whole, not to be greatly regretted in a reference book of this type.

Dr. Mellor is certainly constructing for himself a memorial more lasting than brass, and the publishers may rest content that the "Treatise" will be like the goods concerning which the notice appeared, "We do not sell these goods; they sell themselves. We only wrap them up!"

F. A. M.

Chemical Principles. By ARTHUR A. NOYES and MILES S. SHERRILL. [Pp. xviii + 310.] (New York: Macmillan & Co., 1922. Price 18s. net.)

THE evolution of the methods of teaching physical chemistry is a necessarily slow process, not only on account of the slight degree of correlation which exists between some of the branches of the subject, but also because of the rapid extension of its boundaries since the nineties of the last century. It is increasingly evident that the course of training of the student, who is to become the future investigator in physical chemistry, must be grounded on an ever-broadening basis of mathematics and physics.

The value of the textbook, *Chemical Principles*, by Noyes and Sherrill, lies mainly in the success with which these authors have presented a systematic and logical course of training in the groundwork of the science. This treatise, in which is embodied the results of many years' experience in the teaching of fundamental chemical principles, is intended to give "that intensive training which is essential for pursuing successfully more specialised courses of scientific study or for applying chemical principles to industrial problems." "The book consists mainly in a development of the atomic, kinetic, and ionic theories . . . and with the aid of these theories, from mass action, phase rule, and thermodynamic view-points, of the principles relating to the rate and equilibrium of chemical reactions." It is interspersed with problems which form an integral part of the course of training which is set forth, and these, in themselves, will be of great service to teachers of physical chemistry. The book would possess an added value if the authors could see their way to include in the next edition a list of references to the papers from which their problems have been chosen.

A systematic notation has been employed throughout the book, which is summarised in a convenient form at the end. Although this does not agree in all respects with the notation recommended by the Chemical Society, the differences are not sufficiently marked to give rise to any difficulty in its use in this country.

The omission of the Nernst heat theorem from the course of instruction is to be regretted, since this theorem possesses considerable importance, both in pure and applied science, for the study of chemical processes.

W. E. G.

The Theory of Allotropy. By PROF. A. SMITS, Ph.D. Translated by J. Smeath Thomas, D.Sc. [Pp. xiii + 397, with 239 figures in the text.] (London: Longmans, Green & Co. Price 21s.)

BOYLE in *The Sceptical Chymist* pleads for a simplification of the phrases current among the *Chymists* of his day. "I have long observed," he says, "that those dialectical subtleties, that schoolmen too often employ about physiological mysteries, are wont much more to declare the wit of him that uses them, than increase the knowledge or remove the doubts of sober lovers of truth." Science still suffers from the "tyranny of phrases," and Boyle's criticisms of the "vulgar spagyrists" and the "hermetik philosophers" might with equal justice be repeated with regard to many writers of scientific treatises of the present day.

Prof. Smits has chosen to present his theory of allotropy in a language which is as difficult to understand as that of the alchemists, and this is all

the more regrettable as his theory should prove very useful for the interpretation of many phenomena, hitherto only obscurely understood. The work is not only overburdened with phase diagrams which so confuse the reader as to destroy his interest before he has read many pages, but is also elaborated much further than is justified by the slender experimental basis on which it stands.

The view that substances, which are pure in the chemical sense, may occur in several modifications in the liquid and solid state has been accepted by chemists for many years. Normally, the establishment of equilibrium between these modifications takes place sufficiently rapidly for the substance to be treated as a one-component system. In a very few cases, mainly in organic chemistry, such as in tautomeric change, etc., the equilibria were known to take place sufficiently slowly for measurements of the rate of change to be made. The author shows that these slow changes are of more frequent occurrence than is generally assumed, and in the first six chapters of the theoretical part of the book and in the first chapter of the experimental part he discusses the relationships between the unary, pseudo-binary, and pseudo-ternary systems which are present in these cases. The theory is extended to embrace electromotive behaviour, and the method of treatment adopted brings out many novel and suggestive features. The slow changes occurring during the ionisation and the discharge of metals are regarded as being analogous to the corresponding equilibria between the various molecular species in the liquid and solid states. Anodic and cathodic polarisation, overvoltage, and passivity are interpreted from this point of view.

The explanations put forward by the author are often restatements of the problem in terms of a complicated phase diagram, and throw little new light on the phenomena under discussion. Thus he deals with the Ostwald law of successive transformations on p. 98, with the significance of small concentrations on p. 174, and with the problem of passivity on p. 358.

While the book is of value as a work of reference to a number of little-known fields, it has very little merit as a textbook and is not one that can be recommended to students.

W. E. G.

The Chemistry of Urea. By Emil A. Werner, M.A., Sc.D., F.I.C. (Monographs on Biochemistry. Edited by R. H. A. Plimmer and F. G. Hopkins.) [Pp. xii + 212.] (London: Longmans, Green & Co., 1923. Price 14s. net.)

This excellent series of monographs needs no introduction. The volumes already published have a place in the laboratory and by the desk of every student of biological problems, and an addition to their number is awaited with a keen impatience. We would say at once that Prof. Werner's book ably maintains the tradition of its predecessors, and in it the serious reader will not be disappointed. This said, the greater part of criticism is made, and we may the easier confess to one regret. Other contributors to the series have found a particularly happy balance between the treatment expected of a textbook and that required of an original thesis. Prof. Werner, we feel, has erred in this particular in the direction of elaboration of detail and a reiteration which is a little tiresome. His argument would have yielded nothing in force had he been content with something less than two hundred pages.

The purpose of the monograph is no less than a massed attack on the accepted structure of urea. For more than ten years the author has pursued a reinvestigation of the reactions and syntheses of this familiar substance, and he now presents us with a considered verdict and an alternative. It is his conclusion that the main syntheses of urea may be expressed as the

union of ammonia with the keto form of cyanic acid and the hydrolysis the reversal of this reaction.

$$NH_3 + HN.C:O \rightleftharpoons HN.C\left\langle\begin{array}{l}O\\|\\NH_3\end{array}\right.$$

The new structure brings urea into relation with the ammonium salts in general and goes far to overcome the anomalies associated with the conventional carbamide formula. Of wider interest, perhaps, is the biological problem raised. Physiologists will have seriously to consider the claims of cyanic acid to a place in any general scheme of nitrogen metabolism. The author presents a formidable argument that will be read with a lively interest.

In appendices are collected the physical constants of urea, a critical survey of the methods for its detection and estimation, and a brief discussion of the possibility of the economical fixation of nitrogen as urea.

R. K. C.

Practical Chemistry. By E. J. Holmyard, B.A. [Pp. xvi + 267.] (London: G. Bell & Sons, 1923. Price 4*s*.)

We may say frankly that we have been disappointed in Mr. Holmyard's new book. After reading through his excellent work on *Inorganic Chemistry*, we had expected from him in a book on *Practical Chemistry* something quite different, something much more out of the ordinary than the present treatise. Not that it is in any way inferior to its forerunners of the same type, in fact, it is probably much better; but, nevertheless, we are not sure that the ideal or nearly ideal method of presenting the practical aspect of a scientific subject has been attained in this type, especially if the book is intended to replace to a certain extent the personal side of practical teaching.

The book is intended as a laboratory companion to the author's *Inorganic Chemistry* and covers the schedules of the School and Higher Certificates. It is divided into two parts, elementary and more advanced. In the former, after dealing with differences between mixtures and compounds and fundamental methods of separation, experiments dealing with the proof of the simple chemical laws and methods of determining equivalent weights are described. This is a good point, though possibly the use of silver and silver salts in many of the experiments may be objected to in many schools on the score of expense. The next seventy pages are devoted to preparative work. We think that a great number of the experiments described here would be better carried out in the lecture-room than by individual students in the laboratory.

In the advanced part a considerable number more of unconnected inorganic experiments and a few organic preparations are given. In addition, sections are devoted to Volumetric Analysis, Gravimetric Analysis, and Simple Physical Chemical Measurements. We should have preferred to have seen more simple gravimetric quantitative experiments in place of much of the preparative work. In volumetric analysis it is generally found better to use as the unit the equivalent weight and to carry out all calculations using this unit instead of the gram. One further small point of criticism is the heating of Gooch crucibles with easily reducible substances directly in the Bunsen flame. We rather sympathise with the schoolboy attempting to get copper oxide to constant weight under these circumstances.

On the whole, however, the experimental work is extremely well described, and to those who prefer the direct method of teaching the book may be strongly recommended.

H. T.

MINERALOGY

A Textbook on Ore Dressing. By S. J. Truscott, A.R.S.M., M.I.M.M. [Pp. xii + 680, with 446 illustrations.] (London: Macmillan & Co., 1923. Price 40*s.* net.)

The practice of ore dressing is ever advancing and changing. Since the beginning of this century the magnetic method of separation has been greatly developed and most widely applied, and in 1910 a new system of treatment, based on principles only partially applied to mineral dressing before, was successfully introduced and has since assumed a most important position among the methods of concentration.

For at least the last twelve years no new book on ore dressing has been published in this country, so that there is, undoubtedly, a place for a work which presents, within a reasonable space, the methods in use to-day. Such an account has been given by Professor Truscott, who is to be congratulated on having written a clear and comprehensive treatise on modern ore dressing practice. The book bears evidence of much thought and hard work. Care has been taken in the selection of the various appliances, which are well described and excellently illustrated. The subject of mineral dressing consists of two parts; that which deals with the physical principles, and that which consists of mechanical details of the machines and appliances used. In this book one would like to have seen the physical principles more fully discussed, for it is in this direction that greater clearness of thought is needed at the present time. It is true that authorities do not yet agree on many of these principles, but that is a reason for making an attempt to arrive at a clear conception of the bases of dressing practice. Probably the desire to produce a book of convenient size and yet not to omit any modern appliances has prevented this part of the subject from being more fully considered.

The subject-matter of the book is arranged in logical sequence, and consists of the following sections: introduction; washing and sorting; comminution (under which term is included breaking and crushing as well as grinding); laboratory, screen, and water sizing; water concentration; flotation; magnetic, electrostatic, pneumatic, and centrifugal separations; heat treatments; sampling and control of operations; dressing systems and plants. Crushing is well and carefully done; also, the sections on classification and water concentration are comprehensive, no important appliance having been overlooked. The most noteworthy chapters are those on flotation—the new system of concentration referred to above. This subject is most excellently dealt with; the development of the various processes and the machines used are well described and the theory clearly set out. Magnetic separation, which from the point of view of theory and forms of separators is an extensive subject, might, perhaps, have been more fully considered. The type and illustrations are all that can be desired, being such as one is accustomed to see from this well-known publishing house.

The book can be recommended with confidence to mining engineers and metallurgists and particularly to those who are students in these professions.

E. Courtman.

BOTANY AND AGRICULTURE

The Story of the Maize Plant. By Paul Weatherwax. [Pp. xv + 247, with 174 figures.] (Chicago: University of Chicago Press, 1923. Price $1.75.)

The outstanding importance of maize as a food and forage crop has led to a great output of literature dealing with the plant from various standpoints, both scientific and economic, but no concise summary of the morphology of the species has hitherto been available. In the volume under discussion

the author gives an outline of our present botanical knowledge of the maize plant, indicating, in several cases, various controversial points upon which further light is needed. The morphological and anatomical characters of the various organs are illustrated by a series of original text figures admirable for their clarity and excellent reproduction, and which are probably among the best hitherto published in this connection. Following a consideration of the general structure of the plant attention is directed to its ecological relations and to methods of harvesting and tillage, thus clearing the ground for a more detailed exposition of the grain or "seed," and leading up to an outline of the genetics of the species, with an indication of the possibilities that lie in the intelligent application of the principles of plant breeding.

An interesting feature is introduced by an account of the part played by maize in aboriginal America, its importance being emphasised by the numerous Indian myths associated with it. With the coming of the *Mayflower*, and subsequent colonisation, the maize crop grew steadily in importance until at the present time the United States produce each year three times as much as all the other countries in the world put together.

This book, with others in the same series, is avowedly written for the educated layman as well as for the specialist. Unnecessary details are eliminated and technical terms are reduced to a minimum, resulting in a study which provides a welcome addition to our literature on the subject, and which should prove useful both to the general reader and to the student.

W. E. B.

The Beginnings of Agriculture in America. By LYMAN CARRIER, B.S., M.Agr., Agronomist, Bureau of Plant Husbandry, U.S. Dept. of Agric. [Pp. xvii + 323, and 30 figures.] (London: McGraw-Hill Publishing Co., 1923. Price 15*s*. net.)

THERE is no study more profitable than that of noting the legacies which history has handed down to us, and in recent years agricultural literature has been enriched by several works dealing with the progress of agricultural life in different countries. *The Beginnings of Agriculture in America* is a book which, like these others, teems with interest. The author possesses a very intimate knowledge of agricultural practices, and this familiarity with his subject is responsible for an added interest which the book possesses. It might be supposed that such a work would have a relevancy purely for the country it concerns, but when it is remembered that the early days of the colonisation of America were closely linked up with Europe, and with this country in particular, the subject is therefore more fascinating.

The agricultural conditions obtaining in the Old World prior to the discovery of America are set forth as a means of showing the immense influence which the New World had on the agriculture of the Old World. Especially interesting is the account of the agricultural activities of the American Indians. The popular conception is that they were a warlike people, but they were also well versed in the tilling of the ground and the cultivation of crops for their sustenance. They had made greater strides in plant breeding than any other people, and amongst other things we are indebted to them for maize (Indian corn), tobacco, potatoes, tomatoes and strawberries. Their cultural methods were similarly well advanced, and modern methods such as intertillage and intercropping were then established practices. The subsequent development of the country by the Spaniards and English features largely, and the early agricultural activities in the various States are noted in detail.

The book is well written, and can be thoroughly recommended as a sound exposition of the subject.

H. G. R.

Quantitative Agricultural Analysis. By EDWARD G. MAHIN, PH.D., Professor of Analytical Chemistry in Purdue University, and RALPH H. CARR, PH.D., Professor of Agricultural Chemistry. [Pp. xiii + 329 and 62 figures.] (London: McGraw-Hill Publishing Co., 1923. Price 13*s*. 9*d*.)

FOR a long time the increasing practical value of agricultural analysis has been recognised, yet it must not be assumed that there is any short cut to specialisation in this pursuit, or that it differs in importance from the other allied branches of chemical analysis. In this work the authors have taken pains to demonstrate that the fundamentals are the same in all the applied sciences, for which reason considerable attention has been paid to foundation training. This view concurs with the increasing tenancy amongst agricultural chemists to acquire a thorough knowledge of pure science before specialising in purely agricultural matters.

The book is divided into three sections, the first part dealing with General Analysis. This includes the theory and general principles involved; the means of performing analysis, together with a description and illustration of the apparatus employed. The last chapter in this section deals with quantitative determinations.

The second part is devoted to the theory and practice of special measurements necessitated by analytical requirements. These chapters cover specific gravity, calorimetry, index of refraction, polarimetry, and hydrogen-ion concentration.

The third part deals with the application of quantitative analysis to agricultural requirements. This is a very wide field, and covers feeding stuffs; saponifiable oils and fats; dairy products and by-products; soils; manures; insecticides and fungicides. These matters have been dealt with in a thorough manner, and the book has been designed on very sound lines.

H. G. R.

Productive Farming. By KARY CADMUS DAVIS, Ph.D., Professor of Agriculture, Knapp School of Country Life. Fifth edition. [Pp. viii + 403 + xxxix, with 252 illustrations.] (London: J. B. Lippincott Co., 1922. Price 6*s*. net.)

THIS is an elementary treatise covering practically the entire field of practical farming. Written in the usual American textbook style, it is a work which is primarily intended for those who require an introductory survey of all that the term "farming" implies. In the U.S. and Canada, agricultural studies are included in the curriculum of many of the country schools, which give pupils an early interest in matters which prove of value in after-life. This is regarded as the best means of preventing the migration of country children to the towns and cities, by enabling them to appreciate the advantages which country life possesses. This book caters for such schools.

Much of the text deals with purely North American conditions, is admirably written, and possesses the additional value of being illustrated with numerous explanatory photographs. The first section deals with plant production; the formation of soils and their management; farm crops, their diseases and pests. The second part is devoted to animal production, with a brief survey of breeds, management, and feeding. Cattle products are discussed in the third part, while the final portion deals with the business and mechanical side. It is a book which should prove helpful in farm schools, while the exercises included in the text should prove suggestive to teachers.

H. G. ROBINSON.

The Diseases of the Tea Bush. By T. Petch, B.A., B.Sc., Botanist and Mycologist to the Government of Ceylon. [Pp. xii + 220, with 3 coloured plates and 69 figures.] (London: Macmillan & Co., 1923. Price 20*s.* net.)

It is just twenty years since Watt and Mann published their *Pests and Blights of the Tea Plant*, and the great strides made during that period in acquiring knowledge of the diseases of this valuable plant will be apparent from a glance through the two books. For much of this advance Mr. Petch has been himself responsible. In the present volume he has brought together the fruits of many years of observation and investigation on the Ceylon tea plantations and in the Peradeniya Laboratory. To his personal experiences he has added a survey of the work in other tea-growing countries, notably India, Java, and Japan, and the whole forms by far the most complete account of the diseases of the tea bush that has ever been published.

The book is written in popular language, and its aim is stated to be to enable the tea-planter to recognise the diseases hitherto recorded in his crop, and to take steps to control them when they appear or to lessen the probability of their occurrence. This aim is well fulfilled. The descriptions are admirably clear and sufficiently full to allow of ready identification of most of the diseases mentioned. As the total number of these exceeds sixty, seventeen being recorded affecting the roots alone, some assistance is obviously required if the planter is to know what he has to fight. The numerous illustrations, especially the thirty-one figures in the three coloured plates, are of great help in following the descriptions.

An introductory chapter deals with the classification of fungi (most of the diseases mentioned being caused by this group of organisms), spraying and other preventive measures are considered in a separate section, technical notes and descriptions of the parasites are given in the last two chapters, and a bibliography and index terminate an excellent book.

E. J. Butler.

Botany of the Living Plant. By F. O. Bower, Sc.D., F.R.S., Regius Professor of Botany in the University of Glasgow. [Pp. xii + 634, with 482 figures.] (London: Macmillan & Co., 1923. Price 25*s.*)

The first edition of this volume appeared in 1919 and was "framed on the lines of the annual Course of Elementary Lectures on Botany given in Glasgow University."

The present second edition follows the main lines of the older one, but with one very important and a few minor changes and additions. The important change referred to is the inversion of the order in which the Cryptogams are considered, the simplest being described first and such descriptions leading on to that of the more complex forms. This is the more usual method, and its acceptance adds considerably to the utility of the book. Another change is that a new chapter is added in which some of the activities of the cell are considered, this chapter following after the one in which the structure of the cell and the differentiation of the tissues are dealt with. Another new chapter is provided at the end of the first division of the work and this deals in a general way with evolution and morphological problems.

The chapter on sex and heredity might be improved by amplification and also by providing more illustrations, the two diagrams after Punnett are not in themselves sufficient to enable one to visualise Mendelian segregation.

The book is simply and clearly written, and provided with numerous and excellent illustrations, a few new drawings being reproduced in this edition.

E. M. C.

Pests of the Garden and Orchard, Farm and Forest. By RAY PALMER, F.E.S., and W. PERCIVAL WESTELL, F.L.S. [Pp. 413, with 44 plates, containing 132 illustrations and 3 coloured plates.] (London: Henry J. Drane. Price 25s. net.)

MANY of the standard varieties of plants are merely derived from chance seedlings, and there appears to have been, until recent years, little attempt to obtain new varieties having definite characteristics and constitution. More especially is this the case with fruit trees. The breeder of new varieties of plants has as his objective a number of qualities of which resistance to pests and freedom from disease are often subordinated to other and more showy points. One of the most obvious examples is the raising and putting into commerce of seedlings derived from the Victoria plum, a most prolific variety, but the susceptibility of which to the silver-leaf disease is notorious. Only in the case of the potato is there any systematic attempt made to classify varieties from this standpoint, immunity from wart disease being now of great commercial importance.

The grower of fruit, flowers, or vegetables is thus faced with the task of supplementing the resistance of his plants to their natural enemies and, although successful resistance is primarily dependent on a healthy plant, this in itself is rarely sufficient. Knowledge of pests and of the correct remedial and preventive measures of which use should be made is essential, and this book is the practical man's encyclopædia of the subject. No claim is made to any large measure of originality or to technical description; these would be out of place in a work which is confessedly a compilation for the use of the non-scientific. The identification of injurious insects, fungoid diseases, and other evils is rendered easy and sure by clear description and excellent illustration, and the necessary treatment is based on the most recent scientific work. The arrangement is intensely systematic, and one gets the impression that insects, fungi, weeds have been thoroughly card-indexed and cross-referenced. The book is divided into parts which deal with insects, animals other than insects, fungoid diseases, weeds, insecticides and fungicides, and sundry useful information and tables. This is followed by a calendar dealing with various spraying operations, a glossary, references and indexes, the whole making reference from any point of view a simple matter. One or two slight omissions are scarcely worth mention, and the sensitive reader may object to the somewhat free use of the split infinitive; but, taken as a whole, it is a book to be commended and recommended.

H. J. E.

Poisonous Plants of all Countries. By A. BERNHARD-SMITH. [Pp. xii + 112, with 185 figures.] (London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2. Price 6s. net.)

IT is difficult to imagine any large section of readers to whom this book would be of value. Presumably the medical man is expected to use it, but it hardly seems fair to foist on the innocent medico such a number of chemical and botanical inaccuracies. Moreover, any person who has to rely on the information here provided is scarcely fit to be entrusted with cases of poisoning by the substances described. We suspect the compiler of being at heart a literary man, as valuable space has been used for quotations bearing on the various plants mentioned which might have been devoted to the business in hand. But that might have provided more opportunities for comic chemistry.

H. J. E.

ZOOLOGY

Great and Small Things. By Sir Ray Lankester, K.C.B., F.R.S. [Pp. xi + 246, with 38 illustrations.] (London: Methuen & Co. Price 7s. 6d. net.)

Great and Small Things is the latest addition to the Easy Chair Series of works on Natural Science by Sir Ray Lankester, our greatest living interpreter and clearest expositor of Biological Science. The variety of subjects on which he discourses in his own peculiar lucid style covers a vast range. Passing from the baby gorilla of Sloane Street, he suddenly introduces the reader to phagocytes, then to the pond-snail's flea, the liver-fluke and wasps, then goes on to discuss the senses and sense-organs, longevity, the morphology of monsters and tobacco.

In the chapter " Is Nature cruel ? " Sir Ray Lankester tells us that pain " is mostly short and sharp and of a directive and protective character," and that it is " the beneficent guide of the development of sentient beings."

The peculiar habits of gorillas beating their chests when frightened or excited has been described quite recently by Alexander Barns in his admirable book *The Wonderland of the Eastern Congo.* In it the author describes the noise made by the adult male gorilla which he surprised in the forest as follows: " It started with an indrawn whine, which quickly increased in volume until it broke out into a hoarse grunt, accompanied by a heavy resonant clopp-clopp-clopp. I had of course heard of both the gorilla and orang-utan beating their chests to frighten away an intruder, but when I first listened to this extraordinary ' clopping ' noise, I scarcely realised that it was being made by the great ape beating his chest."

R. E. Drake-Brockman.

A Naturalist's Holiday by the Sea. By Arthur de Carle Sowerby, F.R.G.S., F.Z.S., M.B.O.U. [Pp. xv + 262, with 20 plates and 21 diagrams.] (London: George Routledge & Sons; New York: E. P. Dutton & Co., 1923. Price 7s. 6d. net.)

This is a holiday book and its aim is to interest the visitor in his surroundings, —in this case the area round Penzance, where a great deal of information is collected on a large variety of subjects, making a readable book which will be appreciated by all those who visit Cornwall.

There is an out-of-door atmosphere about the whole work, and it is easy to see that the author is a bird lover, and the chapters on the sea-birds are perhaps the best part of the book. Surely, however, the cormorant at least may be allowed to be common on the East Coast. Any visitor to Northumberland knows how common these are in the neighbourhood of the Farnes. All the notes on the gulls are interesting, and we find that, whilst it is stated that the Kittiwake is very common and nests on the cliffs, the Black-headed Gull, so frequent on the Devonshire coast, except at the breeding season, is dismissed as only a winter visitor, which seems to indicate that it is never much in evidence.

The story of the life-history of the eel stops at information received in 1920, and makes no reference to the successful conclusion of Schmidt's last expedition, which returned in 1922. Up-to-date information is also lacking elsewhere, *i.e.* in the chapter on Whales which, according to the author was prompted by the stranding of " a large herd of Pilot Whales in Mount's Bay." Here the remark (" no doubt the event was duly reported in the papers ") suggests that reference had not been made to the British Museum Publication *Report on Cetacea stranded on the British Coast*, by Sir S. F. Harmer, of which the eighth report has just been published since instructions were issued during 1912 that the stranding of whales should be reported by telegram to the

Museum. These records show, contrary to the author's statement, that at least two specimens of the Lesser Rorqual (*Balænoptera acutorostrata*) have been stranded on the Cornish coast. On p. 157 of Notes on these stranded whales (*P.Z.S.*, 1918) we read of "A school of about 50 individuals of the Blackfish or Pilot Whale (*Globicephala melæna*, Traill) which were stranded at Penzance on July 1, 1911."

The nomenclature and descriptions of the whales are somewhat loose, and this applies also to some of the other groups dealt with, and as is inevitable in a work with so large a scope there are certain inaccuracies and misleading statements. Misprints abound, the same word being spelt in two different ways in adjacent paragraphs.

The figure on p. 29 of the calcareous tabes of an annelid on a Pecten shell which are named *Serpula vermicularis* seem undoubtedly to be *Pomatoceros*, another very common serpulid which is distinctly ridged and lies flat on stones and shells. It is also somewhat doubtful whether the "small specimens of the Edible Sea-urchin (*Echinus esculentus*)" in the rock-pools, are correctly named. *E. miliaris*, a smaller and flatter species, is known to occur there between tide-marks, *E. esculentus* usually living farther from the shore in deeper water.

The statement that "Gastropods are mainly carnivorous" is surprising, as by far the larger part of the shore forms dealt with are mainly plant eaters—limpets, periwinkles, and, to a large extent, topshells, all browsing on the sea-weeds growing on the rocks where they live; *Purpura, Buccinum, Nassa, Murex*, and *Natica*, as he states, being truly carnivorous.

Unfortunately, there is no index, and a few references to books such as Percy C. Lowe's *Our Common Sea Birds* would greatly improve the work.

M. V. LEBOUR.

The Animal and its Environment. By L. A. BORRADAILE, Sc.D., Fellow and Tutor of Selwyn College, Cambridge. [Pp. vii + 399, 426 figures and plates.] (London: Henry Frowde and Hodder & Stoughton, 1923. Price 18*s*. net.)

THIS is a very interesting book on the relations of individual animal organisms to their surroundings. It is profusely illustrated—in some cases there are many illustrations to bring out facts stated in a few lines. From the point of view of the professional zoologist much of the material is familiar, yet one could wish that, when one was a student oneself, such an attractive book existed. Even without text, the figures and their explanations are splendid, and Dr. Borradaile manages to bind all together by means of a readable story. On all the subjects he mentions he has something interesting to say.

J. BRONTË GATENBY.

The Evolution of Man. By R. S. LULL and OTHERS. [Pp. x + 202, with 27 illustrations]. (New Haven: Yale University Press; London: Humphrey Milford, Oxford University Press. Price 15*s*. net.)

THIS book is based on a series of six lectures delivered at Yale University in the year 1921–22, and is a continuation of *The Evolution of the Earth and its Inhabitants*, which book was based on the lectures of 1916–17 at the same University. The book is, accordingly, divided into six chapters. The first, by Prof. R. S. Lull, sets forth the palæontological evidence for the evolution of man. Prof. Lull adopts the view that Asia is the birthplace of mankind. He gives a brief résumé of the discoveries of the actual osseous remains of prehistoric man which have so far come to light.

Chapter ii, by Prof. H. B. Ferris, gives in detail, largely from the anatomical and embryological standpoints, some of the evidence for evolution

which is to be found in the development and structure of present-day man. The illustrations in this section are particularly good.

Chapters iii and iv, by Prof. G. H. Parker and J. R. Angell respectively, deal with the evolution of man's nervous system and of his intelligence.

Chapter v deals with the evolution of society. Prof. A. G. Killer does not seem to like altogether Spencer's famous analogy of Society and "an organism." The last chapter, in which Prof. E. G. Conklin discusses the trend of evolution, contains one point, at any rate, which we feel we cannot leave unchallenged. Prof. Conklin accepts R. Pearl's statement that within 200 years the American population will have reached its maximum of about 200 millions. Now it seems to us quite absurd that a nation with the rich and varied resources of America, and whose area is—if we remember rightly—at least twenty-five times that of the United Kingdom, should only be able to support a population about four times as large. But perhaps Prof. Conklin (or Mr. Pearl) believes the derelict farmlands of the Eastern States to be uninhabitable.

C. C. R.

ANTHROPOLOGY

Ancient Man in Britain. By RONALD A. MACKENZIE. [Pp. xv + 257, with 17 plates and other illustrations.] (London: Blackie & Son, 1922. Price 12*s*. 6*d*. net.)

THE science of prehistoric archæology, like the Christian faith, sometimes suffers from the unrestrained enthusiasm of its supporters. Mr. Ronald Mackenzie has written a book which, as he informs us in the preface, deals with "the history of man in Britain from the Ice Age till the Roman period." The author, however, does not confine his remarks and conclusions to this short period of human history, but indulges in speculations regarding the earlier sojourn of mankind upon this earth. It is unfortunate that Mr. Mackenzie should state (p. 17) that "Breuil and others place the pre-Chellean, Chellean, Acheulean, and Early Mousterian stages (of man's development) in the Third Interglacial epoch," and proceed to claim that such a view is now being generally accepted. The exact reverse is the case, as the author would have known had he read the latest literature upon this question. The omission, too, in any book dealing with ancient man in Britain, of any reference to the now very widespread belief, amongst archæologists, in the existence of man in England in the Pliocene period is to be regretted. Mr. Mackenzie has, it would seem, used the few facts known as to the Aurignacian and later races inhabiting this country in ancient times as pegs upon which to hang his views and opinions upon philology, ethnology, and kindred subjects. These views, in themselves, are interesting and suggestive, and will be appreciated by many readers; but it must be remembered that, at present, so far as they relate to the actual discoveries of archæologists, they are merely suggestive inferences, and not facts to be used in the slow building up of a very complex and difficult science. It is necessary, also, to point out that Mr. Mackenzie is, in all probability, incorrect in imagining that the Aurignacian cultural phase is post-glacial in England, as there is in existence carefully sifted evidence tending to show that very cold conditions, accompanied by an arctic flora, obtained in the Eastern Counties in Magdalenian times. *Ancient Man in Britain* is well and learnedly written, and is illustrated by good, but not over-numerous plates and line-drawings, and though it has seemed necessary to sound a note of warning regarding the advisability of accepting many of Mr. Mackenzie's theories, yet they are stimulating, and afford pleasurable reading.

J. REID MOIR.

ENGINEERING

Mechanical Testing. Vol. II., Testing of Prime Movers, Machines, Structures, and Engineering Apparatus. By R. G. Batson, M.Inst.C.E., M.I.Mech.E., and J. H. Hyde, A.M.Inst.C.E., M.I.A.E., A.M.I.Mech.E. [Pp. xi + 446, with 313 illustrations.] (London: Chapman & Hall, 1922. Price 25*s.* net.)

This volume covers the mechanical testing of finished parts of machines and engines as distinct from the first volume, which deals with tests for strength of the various parts.

The authors state that prominence has been given to certain details upon which to a great extent the accuracy of a gear depends. This matter is all-important, especially to the technical engineer and manufacturer, who, in devising some new test, have generally too little experience in standard test methods outside their own particular branch, so that to have the experience of two experts in this matter in so concise a form is undoubtedly of great assistance.

There are few books attempting to cover the ground of this one, so that in view of its quality it should become universally popular.

The second volume commences with a discussion on various dynamometers, of absorption, solid and fluid friction, transmission and traction types. Following this is a section on lubrication, also friction of bearings and belting. In chapter xiii are found descriptions of the standard machines for vibration tests and in natural sequence, the next chapter is devoted to balancing of rotating parts. Two chapters are given to testing of slabs and beams in concrete, followed by one on columns and struts. One of the most valuable chapters is that on measurements of movements and stresses. Some of the apparatus described here could be well applied to the analysis of vibration troubles in machines such as turbo-alternators. Tests on cutting tools are next given, a considerable amount of original work being discussed, as well as standard tests. Apparatus for pressure tests is dealt with in chapter xx, whilst chapter xxi gives a good account of gear used in tests of aircraft models.

The authors in writing such a text have certainly had a difficult problem before them, that of describing a considerable number of standard machines and tests, yet keeping the work from appearing like a collection of manufacturers' catalogues.

The work is very free from errors, although eqn. (4) on p. 378 is obviously incorrect. A number of references to original investigations is quoted at the end of each chapter.

Great regret is felt, in reading this concluding volume, that the authors have adhered so rigidly to their title "Mechanical Testing," excluding even the elements of microscopic examination of metals, so valuable in conjunction with mechanical tests.

The excellence of the treatment makes it desirable, therefore, that at an early date the authors add a volume covering such work as hydraulic, and engine and boiler testing.

B. Lloyd-Evans.

Electrons, Electric Waves, and Wireless Telephony. By J. A. Fleming, M.A., D.Sc., F.R.S. [Pp. iv + 326, with 112 illustrations.] (London: The Wireless Press, Ltd., 1923. Price 7*s.* 6*d.* net.)

The popularity of radio-telephony as a scientific hobby has been responsible for the production of a considerable amount of literature, dealing with both the practical and theoretical aspects of the subject. Among such publications Prof. Fleming's book deserves to take a very prominent, if not the foremost, place. Those who heard and enjoyed the Christmas Lectures at the Royal

Institution in January 1922 will find all that fascinating material presented afresh, with considerable additions.

A wealth of scientific ideas, the book is presented in a most attractive and readable fashion. Nothing is left out, from the classical theory of wave motion to the conceptions of Planck, Rutherford, and Bohr, which is necessary for a complete understanding of the physical mechanism of "wireless."

About two-thirds of the book is taken up with the development of these fundamental ideas, and the remaining third with the technical exposition of wireless telephony. Students of Physical Science could with advantage use the book as a kind of ground plan on which to build their studies of Modern Physics, while the genuine amateur of wireless will find it a splendid introduction into those extensive regions of thought and practice, where his hobby leads him.

Of the actual discussion of wireless telephony, let us say how thankful we are that it is really so simple after all, for here we have it, on the authority of a distinguished pioneer, stripped of the complications with which it is usually associated, and within the grasp of those whose relations with science consist in having "done a little chemistry at school years ago."

R. C. Richards.

Line Charts for Engineers. By W. N. Rose, B.Sc. [Pp. xii + 95.] (London Chapman & Hall, 1923. Price 6*s.* net.)

The value of nomographic methods of handling design and other formulæ, in daily use by the engineer, is now fully recognised. The nomogram materially reduces the time and labour of arithmetical computation, and also the chances of error.

There has in consequence, during recent years, been an increasing demand for readily understood information regarding the principles and design of nomograms, by a considerable number of engineers, who have found the original mathematical treatise of d'Ocagne too advanced in treatment to be of much use to them. Several smaller books have been written to meet this demand. The volume in question is the latest, and gives probably the simplest explanation that has yet appeared.

In the ordinary run of engineering formulæ three or four types of nomogram are sufficient. The principles and methods of construction are comparatively simple and do not require a knowledge of advanced mathematics. In *Line Charts for Engineers* the author has brought out this point clearly. In the preface it is stated, "All that can be done is to indicate on broad lines how to adapt a formula for representation on a chart and to show how the chart can most easily be built up. The object of this present work is primarily the latter." The author might have gone a little further, and given a section dealing, in general terms, with the important subject of the "type" equation. A knowledge of this is essential for a beginner, in order that he may not waste his time attempting impracticable constructions.

Six type equations have been given in the summary in Col. H. K. Hezlet's admirable pamphlet on the subject of "nomography"; and in a second edition the inclusion of these would enhance the value of the book. Also, there seems to be no reason why the ordinary functional notation, $f(x)$, $F(y)$, etc., should not be used. There is nothing mysterious about these symbols after a definition of their meaning is given, and their use is conducive to clear and concise explanation.

The work is divided into four chapters. The author first deals with the construction of functional scales, and the treatment should give the engineer a clear idea of this fundamental element of the nomogram. The type of nomogram with rectilinear axes carrying natural scales is next dealt with, and the algebraical expressions for the derived scale values are worked out.

This section is followed by a discussion of the important type of nomogram with the rectilinear axes carrying natural scales and one curved axis. The author states that the chief disadvantage of this form " is the large amount of calculation necessary for the graduation of the curved axis."

While it is desirable that the designer should know how to locate the points on the curved axis by calculation of co-ordinate values, it is, as a rule, unnecessary to calculate them. The curved axis and points thereon can be obtained directly by cross alignment from the scales on the parallel axes. This condition, in fact, holds for all types of alignment charts. When the beginner has mastered the method of construction of the function scale, and recognises from the type equation what form the nomogram will take, his real difficulty is the choice of scale values and lateral disposition of the axes, to obtain a nomogram which will read to the degree of accuracy required.

The location and graduation of the third axis follows mechanically by alignment, and no calculation of co-ordinate values is necessary.

The remaining section of the book deals with the type of nomogram with rectilinear axes carrying logarithmic scales, the treatment being similar to that of the first section. The value of the ordinary slide-rule scale, which can be ticked off on a strip of paper and used directly for graduating the axes is not mentioned. Another point not touched on is the complete scale graduation of an axis, after the main divisions have been carefully located by calculation. The laborious arithmetical work often necessary can be greatly reduced by plotting a graph with the scale axis as base, and projecting the intermediate divisions from this graph, to the scale axis. This method can be applied to any form of unequally divided scale.

The text is clearly written and illustrated by a considerable number of worked-out examples.

It can be confidently recommended to students and others who desire information regarding the design and construction of nomograms.

Electrical Engineering Laboratory Experiments. By C. W. Ricker, S.B., S.M., M.E.E., and Carlton E. Tucker, S.B. [Pp. xiv + 310, with 138 diagrams.] (London: McGraw-Hill Publishing Co., Ltd., 1922. Price 11*s*. 3*d*. net.)

The aim of the authors appears to have been to produce a treatise having as wide a field as possible for general electrical laboratory work and at the same time to explain thoroughly the elementary propositions on which all such later work is based.

From the table of contents the volume will be seen to cover the syllabus of laboratory work required of candidates in electrical engineering for B.Sc. (Eng.) London or the graduated evening school courses of the technical institutes. With so much ground to cover the authors may be forgiven for quoting many methods of measurement without proof.

This drawback is minimised by the copious references that are given. These, however, are in most cases to American publications and this is a disadvantage to English readers.

Although of American origin, the book is international in character, and no difficulty is found due to ambiguous statements giving wrong impressions.

Mathematical treatment of problems is limited to a minimum and is undoubtedly an advantage in such a publication. It is the experience of most demonstrators that, at the commencement of a course, the contemporary mathematical knowledge of students is slight. Hence they are discouraged by the apparent difficulty of the electrical problems, and their work suffers in consequence.

The preliminary chapters are a résumé of methods of measurement, losses in instruments, and the application of bridge methods to the measurement of resistance, capacity, and inductance. This is followed by a sequence of

experiments on D.C. machine characteristics succeeded by a chapter on the determination of candle power and the rating of incandescent lamps.

The A.C. section is introduced by a consideration of the properties of circuits containing inductance and capacity passing on to the general testing of A.C. machinery both static and rotary, concluding with a chapter on the mercury arc rectifier.

Each experiment is preceded by a description of what is to be attempted and followed by a summary of the results required.

It is to be regretted that an index to subject-matter is not provided, but the work certainly bears out the statement in the preface, " This book is the result of an extended period of growth and experience."

A. N. JACKSON.

MISCELLANEOUS

Chaos or Cosmos? By EDGAR L. HURMANCE. [Pp. xxi + 358.] (New York: E. P. Dutton & Co., 1922. Price $3 net.)

IN this book we find another attempt to solve the riddle of the universe. The author asks whether " the world in which we live is a chaos, a welter of blind forces and brutish passions," or whether it is " a co-operative enterprise, through which man and God are slowly working out an order of justice and brotherhood," and he proves, to his own satisfaction at any rate, that it is the latter. Whatever view we may take of the contents of this volume, there is no doubt as to the author's great courage in tackling his tremendous task, for it is not everyone who would care to give a complete exposition of the universe in under four hundred pages! Unfortunately, such an attempt infallibly gives rise to many errors and dogmatisms. To start with, it is only fair to say that Mr. Hurmance bases his inquiry on the Christian system of ethics, which he tests by means of applying it to the universe so far as we know it to-day. There is, therefore, little or nothing which can be called new in the way of scientific knowledge.

The first part of the book is devoted to the problem of the universe as a whole, and the author seeks to destroy physical materialism by means of the new physics, with its electrical explanation of matter. He does not touch the problem of personality, which he hopes to discuss in a later volume. Mr. Hurmance rejects the possibility of there being other habitable planets, though we cannot see how this really affects his main argument.

The second part deals with the question of the individual, and discusses the relative merits of Monism and Pluralism, while the third deals at some length with the morality of the universe, and is divided into three sections —individual relations, industrial relations, and national relations, of which we think the second the most valuable.

C. C. R.

Human Character. By HUGH ELLIOT. [Pp. xvi + 272.] (London: Longmans, Green & Co., 1922. Price 7s. 6d. net.)

AN attempt to analyse human character must of necessity be a difficult task. Many writers—some of the highest order—have tried to construct a science of human character, but all attempts have been abortive. Whatever view, therefore, we may take of Mr. Elliot's work, we must recognise the admirable way in which he has given us a presentation of his views.

Perhaps the chief of the conclusions he comes to is that character and motives depend on differences of emotion rather than of education or intellect. Mr. Elliot believes that the existence of the human race depends on what he calls the three major passions—egoism, which enables the individual to survive; love, which enables the species to be perpetuated; and the

social and moral instinct which maintains society. But besides these major passions there are many derivative passions, and to each of these Mr. Elliot devotes a chapter. One of the most interesting of these chapters deals with the relation between genius and disease, while another deals with a new feature of character which the author names "bovarysm," a name derived from Flaubert's famous novel *Madame Bovary*.

A few statements, such as "among the higher races there is now a tendency towards baldness of the head, and the tendency is most noticeable among individuals whose mental development is highest," seem rather far-fetched. We were under the impression that abundant head and body hair was a characteristic of the highest as well as of the lowest of races of mankind. We should have thought baldness to be due to the wearing of headgear, worry, etc.

C. C. R.

The Biology of Death. By RAYMOND PEARL, The Johns Hopkins University. [Pp. 275, with 64 illustrations.] (Philadelphia: J. B. Lippincott Company, 1922. Price 10*s*. 6*d*. net.)

THIS book is one of a series of Monographs on Experimental Biology which the publishers are bringing out, and in it the author discusses the present-day state of our knowledge of the answers to the following questions which occur on p. 21: 1. "Why do living things die? What is the meaning of death in the general philosophy of biology?" 2. "Why do living things die *when* they do? What factors determine the duration of life in general and in particular, and what is the relative influence of each of these factors in producing the observed result?"

The author presents to us the answers to these questions provided by inquirers in the various fields of general biology, experimental biology, vital statistics, and actuarial science. Naturally, as the author is careful to state in his preface, the book is intended rather to stimulate the reader to further incursion into the literature of the subject, than to satisfy immediately to the full his desire for knowledge. To this end a useful bibliography is attached.

The first part of the book consists of a discussion of the nature of the problem considered, and an indication of the lines along which theories of death have developed. He then proceeds to the subject of cellular immortality, and discusses the result of biological researches on these lines. The next part of the book is concerned with the Life Table, a brief account of its history, and an interesting comparison between the general information obtainable from modern Life Tables and that obtainable from Life Tables which were constructed some years ago, from figures relating to samples from populations of two thousand years ago.

The author then proposes a grouping of the causes of death according to a biological classification, and shows how the Life Table will then indicate the relative importance of the different organ systems of the body, when we inquire which organ system, by its breakdown, is more responsible for death, at different ages and for the sexes. Thus far the author has obtained certain answers to his questions. He sees that certain cells of the body may be considered, in a sense, immortal, and that the more developed cellular systems are liable, by their breakdown, to lead to the breakdown of the whole body; and his Life Tables have shown which are the more vulnerable organ systems. He proceeds to the consideration of the possible methods by which this apparently inevitable breakdown may be avoided, and immediately is led to the discussion of longevity as an inheritable character, quoting in favour of this the results of statistical researches into Family Records, and the results of experimental researches in the duration of life of certain flies. As a corollary, he discusses the effect of Public Health activities as measured by Death Rate Curves, and although led to the conclusion that one of the

strongest elements in determining the duration of life of human beings is inheritance, yet he realises the importance of Public Health activity—in his own words, " When based upon a sound foundation of ascertained fact it (*i.e.* Public Health work) may, and does, proceed with a step as firm and inexorable as that of fate itself, to the wiping out of preventable mortality " (p. 238).

One reads this book with great pleasure, because the author has co-ordinated in one small volume a bird's-eye view of the results of workers in many fields of inquiry. It is often difficult for individual workers in adjacent fields to appreciate the work of neighbours, because the results are sent to different markets, and often the workers use an entirely different scientific language. The author should certainly achieve his purpose in causing the reader to pursue further researches independently.

E. C. Rhodes.

Maps and Survey. By A. R. Hinks, C.B.E., M.A., F.R.S. Second Edition. [Pp. xvi + 258, with 26 illustrations and 26 plates.] (Cambridge: at the University Press, 1923. Price 12*s.* 6*d.* net.)

Mr. Hinks's book on Maps and Survey is now so well known that in a review of the second edition it is scarcely necessary to do more than mention the new features.

Following the procedure adopted in another book by the same author, the text of the old edition remains practically unaltered. A chapter has been added containing additions and corrections. Some small corrections which are included in the list appear to have been already made in the text, which is a little confusing to students (*e.g.* on p. 129 and p. 173). Many of the corrections and additions are important, and it is to be hoped that the author will soon be allowed to prepare a completely new edition, according to the wish expressed in the preface.

There is also a " Further Chapter on Maps," which contains many matters of interest and importance to geographers. On this part of the subject, especially, the book is full of useful notes and suggestions, and if one may venture a little criticism, it seems a pity that the chapters dealing with surveying are comparatively sketchy, particularly with regard to the astronomical work. Doubtless, however, this was unavoidable from pressure of space.

The remaining new chapters are devoted to " Maps and Survey in War " —(one almost grudges the space devoted to this, though the chapter is interesting enough)— and " New Methods of Survey," which contains notes on mapping from photographs, and very interesting articles on Stereoscopic Plotting.

The appearance of the new edition is most welcome, and there is no doubt that the added matter has given additional value to an already good book.

M. T. M. O.

The Evolution of Consciousness. By A. Wyatt Tilby. [Pp. 256.] (London: T. Fisher Unwin, 1922. Price 15*s.* net.)

This work is a somewhat ambitious attempt at a very comprehensive philosophy. The subjects discussed range from the development of the senses and the intellect and the basis of memory to the foundations of ethics and social institutions and developments of the idea of God. The arguments are presented in a persuasive manner and with very considerable literary ability and charm. Yet, it is to be feared that the book will satisfy neither the chemist and biologist to whom it especially appeals, nor the philosophers. The general point of view may be described as a sort of scientific naturalism

which regards the phenomena of life and mind as a series of successive increases in the concentration of energy within a growingly complex system. But the difficulties inherent in such a view are dismissed much too lightly, and the argument is only plausible, because terms like "life," "will," "choice," "purpose," and the like are used without any really serious attempt being made to explain their nature in harmony with the view that would reduce everything to forms of energy of material systems. Is the author really entitled to say (p. 73), in the present stage of our knowledge, that consciousness is a purely chemical phenomenon, or even that one can ascertain in any desired instance the chemical elements of which any particular form of life is compounded? And is it in any way illuminating to say of consciousness that it is "the sensitised and concentrated essence of an organism"? The arguments adduced to demonstrate the "chemical nature of consciousness" are all based on the empirical facts of the dependence of consciousness upon the chemical condition of the living organism, but concomitant or dependent factors of a system are not necessarily identical in nature, and no one has as yet succeeded even "theoretically" in expressing consciousness itself "as a chemical equation" (p. 75). The metaphysical position of the book is highly disputable, and certainly insufficiently argued, yet as an individual expression of a point of view the work is interesting and the arguments are presented in a fresh and vivid way.

MORRIS GINSBERG.

Tractatus Logico-Philosophicus. By LUDWIG WITTGENSTEIN. With an Introduction by BERTRAND RUSSELL, F.R.S. [Pp. 189.] (London: Kegan Paul, 1922. Price 10*s.* 6*d.*)

MR. WITTGENSTEIN seeks in the symbolism of language a guide to philosophic orientation. Language is somewhat like geometrical projection: just as a figure in space may be projected in different ways without altering the properties of the original figure, so a fact may be described in different ways without affecting the fundamental relation between the fact and the descriptions. Propositions are either elementary or constructed from elementary propositions. The way in which a proposition represents facts varies. "The gramophone record, the musical thought, the score, the waves of sound, all stand to one another in that pictorial internal relation which holds between language and the world."

The following may serve as an example of the author's aphoristic method:

"From an elementary proposition no other can be inferred."

"In no way can inference be made from the existence of one state of affairs to the existence of another entirely different from it."

"There is no causal nexus which justifies such an inference."

"The events of the future cannot be inferred from those of the present."

"Superstition is the belief in the causal nexus."

Such dogmatic utterances are not very illuminating. The author, indeed, does not appear to believe in the possibility of philosophical explanation. "Most propositions and questions that have been written about philosophical matters are . . . senseless." Presumably no such elucidations are necessary. "The deepest problems are really *no* problems." Apparently, the main purpose of this oracular treatise is to make people somehow *see* the world rightly in a kind of mystic vision. Philosophy, according to Mr. Wittgenstein, is essentially such a vision.

A. WOLF.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Vector Analysis. By C. Runge. Translated by H. Levy, M.A., D.Sc., F.R.S.E. London: Methuen & Co., 36 Essex Street, W.C. (Pp. viii + 226.) Price 9*s.*

The Elements of Co-ordinate Geometry. By S. L. Loney, M.A. Part II. Trilinear Co-ordinates, etc. London: Macmillan & Co., St. Martin's Street, 1923. (Pp. viii + 228.) Price 6*s.* net.

Introduction géométrique a l'Étude de la Relativité. Par Henri Marais. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1923. (Pp. xii + 185.) Price 15 frs.

Physics in Industry: Lectures delivered before the Institute of Physics. By Archibald Barr, D.Sc., LL.D., M.Inst.C.E., F.Inst.P., Sir James Alfred Ewing, K.C.B., LL.D., F.R.S., M.Inst.C.E., and Clifford C. Paterson, O.B.E., M.Inst.C.E. With a Foreword by Sir J. J. Thomson, O.M., F.R.S. Volume I. London: Henry Frowde and Hodder & Stoughton, 1 Bedford Street, Strand, W.C.2. (Pp. 59.) Price 2*s.* 6*d.* net.

A Textbook of Intermediate Physics. By H. Moore, B.Sc., A.R.C.Sc., F.Inst.P. London: Methuen & Co., 36 Essex Street, W.C.2. (Pp. ix + 824, with 560 diagrams.) Price 22*s.* 6*d.* net.

The Elements of Applied Physics. By Alpheus W. Smith, Ph.D. London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4, 1923. (Pp. xiv + 483, with 418 figures.) Price 12*s.* 6*d.* net.

Practical Heat. By Terrell Croft. London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4, 1923. (Pp. xiii + 713, with 630 diagrams.) Price 25*s.* net.

L'Électrodynamique des Milieux Isotopes en Repos, d'après Helmholz et Duhem. Par Louis Roy. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins. (Pp. 94.) Price 10 frs.

Atomic Structure and Spectral Lines. By Arnold Sommerfeld. Translated from the Third German Edition by Henry L. Brose, M.A. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xiii + 626, with 125 figures.) Price 32*s.* net.

Heat and Energy. By D. R. Pye. Oxford: at the Clarendon Press, 1923. (Pp. xii + 211, with 58 figures.) Price 5*s.* net.

Probabilités Erreurs. Par Émile Borel et Robert Deltheil. Paris: Librairie Armand Colin, 103 Boulevard Saint-Michel, 1923. (Pp. vi + 197, with 10 figures.) Price 7 frs.

La Relativité vraie et la Gravitation universelle. Par Georges Fournier. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1923. (Pp. viii + 130.) Price 7 frs.

The Structure of the Atom. By E. N. da C. Andrade, D.Sc., Ph.D. London: G. Bell & Sons, 1923. (Pp. xiv + 314, with 49 figures.) Price 16*s*. net.

La Technique industrielle des Parfums synthétiques. Par René Sornet. Paris: Gauthier-Villars et Cie; 55 Quai des Grands-Augustins, 1923. (Pp. xi + 136.) Price 10 frs. net.

Theoretical Chemistry. From the Standpoint of Avogadro's Rule and Thermodynamics. By Prof. Walter Nernst, Ph.D. Revised in accordance with the Eighth-Tenth German Edition by L. W. Codd, M.A. London: Macmillan & Co., St. Martin's Street, 1923. (Pp. xx + 922.) Price 28*s*. net.

An Introduction to Theoretical and Applied Colloid Chemistry. By Dr. Wolfgang Ostwald. Authorised translation from the Eighth German Edition by Dr. Martin Fischer. Second and enlarged American edition. New York: John Wiley & Sons; London: Chapman & Hall, 1922. (Pp. xiii + 266, with 45 diagrams.) Price 12*s*. 6*d*. net.

A Treatise on Chemistry. By the Right Honourable Sir H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S. Volume II—The Metals, Parts I and II. New edition, completely revised by B. Mouat Jones and others. London: Macmillan & Co., St. Martin's Street, 1923. (Part I, pp. xii + 830; Part II, pp. vii + 831–1564.) Price 50*s*. net.

An Introduction to the Study of the Compounds of Carbon or Organic Chemistry. By Ira Remsen. Revised and enlarged, with the collaboration of the author, by W. R. Orndorff, Ph.D. London: Macmillan & Co. (Pp. xi + 567.) Price 10*s*. net.

Elementary Physical Chemistry. By W. H. Barrett, M.A. London: Edward Arnold & Co. (Pp. viii + 247, with 61 figures.) Price 6*s*. net.

Alcoholic Fermentation. By Arthur Harden, Ph.D., D.Sc., F.R.S. Third Edition. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1923. (Pp. iv + 194.) Price 6*s*. 6*d*. net.

Supplementary Notes on Gravimetric Analysis for Beginners. By W. Lowson, B.Sc., F.I.C. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1923. (Pp. iv + 55.) Price 2*s*. 6*d*. net.

Quantitative Chemical Analysis and Inorganic Preparations. By R. M. Caven, D.Sc., F.I.C. In Two Parts. Part I—Preparation of Inorganic Salts and Simple Exercises in Gravimetric and Volumetric Analysis. London: Blackie & Son, 30 Old Bailey, 1923. (Pp. vii + 156, with 15 figures.) Price 3*s*. 6*d*. net.

Petrology for Students: An Introduction to the Study of Rocks under the Microscope. By Alfred Harker, M.A., LL.D., F.R.S., F.G.S. Sixth Edition, revised. Cambridge: at the University Press, 1923. (Pp. vi + 302, with 100 figures.) Price 8*s*. 6*d*. net.

El Arte de Los Metales (Metallurgy). Translated from the Spanish of Alvaro Alonzo Barba by Ross E. Douglas and E. P. Mathewson. New York: John Wiley & Sons; London: Chapman & Hall, 1923. (Pp. ix + 283.) Price 17*s*. 6*d*. net.

Metals and Metallic Compounds. By Ulick R. Evans, M.A. In Four Volumes. Volume III—The Transition Elements. (Pp. xii + 270, with 42 figures.) Volume IV—Metals of the "B" Groups. (Pp. xii + 350, with 32 figures.) London: Edward Arnold & Co., 1923. Price 14*s*. and 18*s*. respectively.

The Ore Magmas. A Series of Essays on Ore Deposition. By Josiah Edward Spurr. In Two Volumes. New York: McGraw-Hill Book Company, 370 Seventh Avenue; London: 6 Bouverie Street, 1923. (Pp. x + 915, with 175 figures.) Price 40*s*. net.

Textbook of Agricultural Bacteriology. By F. Löhnis, Ph.D., and E. B. Fred, Ph.D. London: McGraw Hill Publishing Company, 6 Bouverie Street, E.C.4, 1923. (Pp. ix + 283, with 66 figures.) Price 15*s*. net.

Practical Plant Ecology. A Guide for Beginners in Field Study of Plant Communities. By A. G. Tansley, M.A., F.R.S. London: George Allen & Unwin, 40 Museum Street, W.C.1; New York: Dodd, Mead & Co. (Pp. 228.) Price 7*s*. 6*d*. net.

Life Movements in Plants. By Sir Jagadis Chunder Bose, Kt., M.A., D.Sc., LL.D., F.R.S. Bombay: Longmans, Green & Co.; and London: 39 Paternoster Row, E.C.4. *Transactions of the Bose Institute*, Vols. III and IV, 1920, 1921. (Pp. xx + 600–902, with 107 figures.) Price 20*s*. net.

Elementary Zoology. By Oswald H. Latter, M.A. London: Methuen & Co., 36 Essex Street, W.C. (Pp. x + 333, with 114 diagrams.) Price 12*s*. net.

Outlines of Evolutionary Biology. By Arthur Dendy, D.Sc., F.R.S. Third Edition, revised and enlarged. London: Constable & Co., 10 Orange Street, Leicester Square, W.C.2, 1923. (Pp. xiii + 481, with 140 figures.) Price 16*s*. net.

Asterias. By Herbert C. Chadwick, A.L.S. Liverpool Marine Biology Committee Memoirs. Edited by W. A. Herdman, C.B.E., F.R.S., and James Johnstone, D.Sc. Liverpool: The University Press; London: Hodder & Stoughton, April 1923. (Pp. xiii + 63, with 9 plates.) Price 4*s*. 6*d*. net.

Practical Bee Anatomy. By Annie D. Betts, B.Sc. Oxford: The Apis Club, 1923. (Pp. 88, with 12 plates.)

Manual of Entomology. By H. Maxwell Lefroy, M.A. London: Edward Arnold & Co., 1923. (Pp. xvi + 54, with 179 figures.) Price 35*s*. net.

A Biology of the British Hemiptera-Heteroptera. By Edward A. Butler, B.A., B.Sc., F.E.S. London: H. F. & G. Witherby, 326 High Holborn, W.C., 1923. (Pp. viii + 682, with 7 plates.) Price £3 3*s*.

Outline of Genetics. By Merle C. Coulter. Chicago: The University of Chicago Press. (Pp. vii + 211.) Price $1.50.

Medical Climatology of England and Wales. By Edgar Hawkins, M.A., M.D., D.P.H. London: H. K. Lewis & Co., 1923. (Pp. xiv + 302, with 149 charts.) Price 25*s*. net.

A Textbook of Dental Anatomy and Physiology. By John Humphreys, M.D.S., F.G.S., F.L.S., and A. W. Wellings, M.D.S., L.D.S. London: Edward Arnold & Co., 1923. (Pp. viii + 323, with 254 figures.) Price 16*s*. net.

Ductless and Other Glands. By Fred E. Wynne, B.A., M.B., D.P.H. London: George Allen & Unwin, Ltd., 40 Museum Street, W.C.1. (Pp. 127.) Price 4*s*. 6*d*. net.

Practical Bacteriology for Chemical Students. By David Ellis, Ph.D., D.Sc., F.R.S.E. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1923. (Pp. viii + 136, with 35 figures.) Price 4*s*. 6*d*. net.

A Manual of Histology. By V. H. Mottram, M.A. London: Methuen & Co., 36 Essex Street, W.C. (Pp. ix + 293, with 224 figures.) Price 14*s.* net.

Handbook of Physiology. By W. D. Halliburton, M.D., LL.D., F.R.C.P., F.R.S. Sixteenth Edition. London: John Murray, Albemarle Street. (Pp. xix + 968, with 580 figures.) Price 21*s.* net.

A Treatise on Engine Balance, using Exponentials. By P. Cormac. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1923. (Pp. xi + 151.) Price 21*s.* net.

The Properties of Engineering Materials. By W. C. Popplewell, M.Sc., A.M.Inst.C.E., and H. Carrington, B.Sc., A.M.I.Mech.E. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xii + 546, with 290 figures and 34 plates.) Price 28*s.* net.

Théorie des Surfaces Portantes. La Théorie de Prandtl. Par Maurice Roy. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins. (Pp. iv + 130.) Price 12 frs. net.

The Automobile Chassis. By Ben G. Elliott, M.A. London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4, 1923. (Pp. ix + 233, with 233 diagrams.) Price 12*s.* 6*d.* net.

Radio, Télégraphie, Téléphone, Concert. Par E. Reynaud-Bonin. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1923. (Pp. 178, with 88 figures.) Price 10 frs.

Principles of Chemical Engineering. By William H. Walker, Warren K. Lewis, and William H. McAdams. New York: McGraw-Hill Book Company; London: 6 Bouverie Street, 1923. (Pp. ix + 637.) Price 25*s.* net.

Structural Members and Connections. Compiled by a Staff of Specialists. Editors-in-Chief, George A. Hool, S.B., and W. S. Kinne. New York: McGraw-Hill Book Company, 370 Seventh Avenue; London: 6 Bouverie Street, 1923. (Pp. xvii + 611.) Price 30*s.* net.

Foundations, Abutments, and Footings. Compiled by a Staff of Specialists. Editors-in-Chief: George A. Hool, S.B., and W. S. Kinne, B.S. New York: McGraw-Hill Book Company, 370 Seventh Avenue; London: 6 Bouverie Street, 1923. (Pp. xiv + 414.) Price 20*s.* net.

Practical Wireless Sets for All. By Percy W. Harris. London: The Wireless Press, Ltd., 12 Henrietta Street, W.C.2; New York: Wireless Press Inc., 326 Broadway, 1923. (Pp. 77.) Price 1*s.* 6*d.* net.

Elements of Glassblowing. By H. P. Waran, M.A., Ph.D., F.Inst.P. London: G. Bell & Sons, 1923. (Pp. ix + 116, with 40 figures.) Price 2*s.* 4*d.* net.

Mine Management: A Practical Handbook for Use in Vocational Schools, Mining Colleges and Universities, also for use of Firemen, Deputy Underground Managers, and others who aspire to become Managers. By Robert Z. Virgin. London: Sampson Low, Marston & Co. (Pp. x + 86.) Price 6*s.* net.

Pioneer Railway Engineering. By H. Stringer, B.A., A.M.I.C.E. London: H. F. & G. Witherby, 326 High Holborn, W.C., 1923. (Pp. 267, with 10 diagrams.) Price 12*s.* 6*d.* net.

Tool Engineering: Fixtures for Turning, Boring, and Grinding. By Albert A. Dowd and Frank W. Curtis. London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4, 1923. (Pp. xi + 340.) Price 17*s*. 6*d*.

Machine Tools and their Operation. By Fred H. Colvin and Frank A. Stanley. London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4, 1922. (Pp.: Vol. I, vii + 341; Vol. II, vii + 409.) Price 20*s*. per vol.

Steam-turbine Principles and Practice. By Terrell Croft. London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4, 1922. (Pp. xi + 347.) Price 15*s*. net.

Design of Machine Elements. By James Alexander Mease, M.E., and George Fred Nordenholt, M.E. London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4, 1923. (Pp. vii + 237.) Price 12*s*. 6*d*. net.

Technical Analysis of Steel and Steel Works Materials. By Frank T. Sisco, London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4. 1923. (Pp. xiii + 543.) Price 25*s*. net.

Handbook of Steel Erection. By M. C. Bland. London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4, 1923. (Pp. ix + 240.) Price 12*s*. 6*d*. net.

Practical Factory Administration. By Matthew Porowsky. London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4, 1923. (Pp. ix + 244.) Price 12*s*. 6*d*. net.

Problems in Machine Design. By O. A. Leutwiler, M.E. London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4, 1923. (Pp. ix + 244.) Price 7*s*. 6*d*.

Gliding and Soaring Flight. A Survey of Man's Endeavour to Fly by Natural Methods. By J. Bernard Weiss, with a Preface by C. G. Grey and an Appendix by W. H. Sayers. London: Sampson Low, Marston & Co. (Pp. xx + 164, with 11 illustrations.) Price 5*s*. net.

Le Ballon et l'Avion la Route aérienne. Par Maurice Larrouy. Paris: Librairie Armand Colin, 103 Boulevard Saint-Michel, 1923. (Pp. viii + 216.) Price 5*s*. net.

Electrons, Electric Waves, and Wireless Telephony. By J. A. Fleming, M.A., D.Sc., F.R.S. Being a Reproduction with some Amplification of the Christmas Lectures (96th Course) delivered at the Royal Institution of Great Britain, December 1921, January 1922. London: The Wireless Press, 12 Henrietta Street, Strand, W.C.2. (Pp. viii + 326, with 112 figures.) Price 7*s*. 6*d*. net.

How to Build Amateur Valve Stations. By Philip R. Coursey, B.Sc. F.Inst.P., A.M.I.E.E. London: The Wireless Press, 2 Henrietta Street, W.C.2; New York: The Wireless Press, 326 Broadway, 1923. (Pp. 70.) Price 1*s*. 6*d*. net.

The Radio Experimenter's Handbook. Part II, Data and Design. By Philip R. Coursey, B.Sc., F.Inst.P., A.M.I.E.E. London: The Wireless Press, 12 Henrietta Street, W.C.2; New York: The Wireless Press, 326 Broadway, 1923. (Pp. vi + 72, with 63 figures.) Price 3*s*. 6*d*. net.

Evolution and Christian Faith. By H. H. Lane. Princeton: Princeton University Press; London: Oxford University Press, 1923. (Pp. xi + 214.) Price 9*s*. net.

Labyrinth and Equilibrium. By Samuel Steen Maxwell, M.S., Ph.D. Philadelphia and London: J. B. Lippincott Company. (Pp. 163, with 11 figures.) Price 10s. 6d. net.

The Valley of Ten Thousand Smokes. By Robert F. Griggs, Ph.D. Washington: The National Geographical Society, 1922. (Pp. xv + 341, with 9 maps and 233 illustrations.)

Tutankhamen and the Discovery of his Tomb by the late Earl of Carnarvon and Mr. Howard Carter. By G. Elliot Smith, M.A., M.D., Litt.D., F.R.S. London: George Routledge & Sons; New York: E. P. Dutton & Co., 1923. (Pp. 133, with 22 illustrations and 2 maps.) Price 4s. 6d. net.

Scientific Method. By A. D. Ritchie. London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace & Co., 1923. (Pp. viii + 204). Price 10s. 6d. net.

An Outline of Psychology. By William McDougall, F.R.S. London: Methuen & Co., 36 Essex Street, W.C.2. (Pp. xvi + 456.) Price 12s. net.

Psychology and Morals. By J. A. Hadfield, M.A., M.B., Ch.B. London: Methuen & Co., 36 Essex Street, W.C.2. (Pp. vii + 186.) Price 6s. net.

An Introduction to Stratigraphy. By L. Dudley Stamp, M.A., D.Sc., A.K.C., F.G.S., F.R.G.S. London: Thomas Murby & Co., 1 Fleet Lane, E.C.4. (Pp. xv + 368, with 84 illustrations.) Price 10s. net.

Statistical Bibliography in Relation to the Growth of Modern Civilisation. By E. Wyndham Hulme, B.A. London: Butler & Tanner, Grafton & Co., 7 Coptic Street, W.C.1, 1923. (Pp. 44.) Price 6s. 6d. net.

The Domain of Natural Science. By E. W. Hobson, Sc.D., LL.D., F.S.S. Cambridge: at the University Press, 1923. (Pp. xvi + 510.) Price 21s. net.

Our Phantastic Emotions. By T. Kenrick Slade, B.Sc. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1923. (Pp. xii + 177.) Price 6s. 6d. net.

Analytical Microscopy. By T. E. Wallis, B.Sc., F.I.C., Ph.C. London: Edward Arnold & Co., 1923. (Pp. viii + 149, with 45 figures.) Price 6s. net.

Mirrors, Prisms, and Lenses. By James P. C. Southall. New York: The Macmillan Company, 1923. (Pp. xix + 657, with 287 figures.)

Principles of Psychology. By Arthur Lynch. London: G. Bell & Sons, 1923. (Pp. xxiii + 408.) Price 21s. net.

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SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

PURE MATHEMATICS. By F. PURYER WHITE, M.A., St. John's College, Cambridge.

History.—G. A. Miller (*Scientific Monthly*, **17**, 1923, 216–28) makes more interesting and useful remarks upon the difficulties of ensuring accuracy in an account of the development of a branch of mathematics.

The current number of the *Acta Mathematica* is a part of vol. xliii, but vol. xxxix has not yet been completely published. Parts 1 and 2 have recently appeared (1923) and contain three articles dealing with the development of mathematics in the nineteenth century. G. Mittag-Leffler (*Acta Math.*, **39**, 1923, 1–57) prints a lecture given in 1916 to the fourth Scandinavian Mathematical Congress on the first forty years of the life of Weierstrass, which is full of interesting personal details besides being of great value for the history of the theory of functions.

The second article is part of an unpublished memoir by H. Poincaré on the Fuchsian functions (*ibid.*, 58–93). If $f(x)$ and $\phi(x)$ are two integrals of a linear differential equation of the second order and we put $z = f(x)/\phi(x)$, then Fuchs noticed that under certain conditions x is a meromorphic function of z. Poincaré received Fuchs's memoir at the beginning of May 1880; the direct result was the paper now printed, which was sent to the Paris Academy on May 28, 1880—an example, as the editor points out, of the extreme rapidity with which Poincaré worked. He proved that Fuchs's conditions are not sufficient, and then, taking the two cases in which (1) the integration can be performed with the help of the doubly-periodic functions and (2) the differential equation has only two finite singular points, he developed the general theory of the functions to which he attached the name of Fuchs.

There follows (*ibid.*, 94–132) a correspondence between Poincaré and Klein on the subject of the automorphic functions. It begins in June 1881 with a letter from Klein, and it comes to an end fifteen months later owing to Klein's illness. N. E. Nörlund adds a few notes and references which enable one to

follow more easily this most interesting chapter in the history of mathematics.

Finite Groups.—A group is said to be the direct product of two sub-groups which generate it, provided that the two sub-groups have only the identity in common and that every element of the one is commutative with every element of the other. If now a finite group can be expressed in two ways as the direct product of undecomposable groups, then the number of factors is the same and to each factor of one decomposition corresponds a factor of the other centrally isomorphic with it. This theorem was enunciated by Maclagan Wedderburn in 1909, but was first completely proved by R. Remak (*Crelle*, **139**, 1911, 293–308; see A. Speiser: *Theorie der Gruppen von endlicher Ordnung*, Berlin, 1923, p. 88). In 1913 Remak gave another proof based on the methods of Wedderburn; he now points out (*Crelle*, **153**, 1923, 131–40) that this is defective. He proceeds to fill up the gap in the argument and also to give a third version.

G. A. Miller (*Trans. Amer. Math. Soc.*, **24**, 1922, 70–8) investigates the I-conjugate operators of an Abelian group—two operators being called I-conjugate if they correspond in at least one of the possible automorphisms of the group. He determines how many sets of I-conjugate operators there are in any Abelian group and how many operators are found in each of these sets.

Algebraic Numbers.—The concept of the absolute value or modulus of a number, real or complex, is capable of application to any algebraic field. To any element a of the field K is co-ordinated a real number $\|a\| \geqq 0$, satisfying the following conditions:

1. $\|0\| = 0$, and for $a \neq 0$, $\|a\| > 0$; and there is at least one element a for which $\|a\| \neq 1$.
2. $\|ab\| = \|a\| . \|b\|$.
3. $\|1 + a\| \leqq 1 + \|a\|$.

$\|a\|$ is called by Kürschák (*Crelle*, **142**, 1913, 211–53) the ***value*** (*Bewertung*) of the element a, and the field for which such a co-ordination has been established is said to be ***valued*** (***bewertet***). It follows immediately that $\|a/b\| = \|a\|/\|b\|$, for $b \neq 0$; $\|-a\| = \|a\|$, and $\|a+b+c+\ldots\| \leqq \|a\| + \|b\| + \|c\| + \ldots$ If instead of condition 3 we have—

3′. If $\|a\| \leqq 1$, then $\|1+a\| \leqq 1$, we have a non-Archimedean system of values; this case has been discussed by A. Ostrowski (*Acta Math.*, **41**, 1917, 271–84). In this case we have—

$$\|a+b+c+\ldots\| \leqq \mathrm{Max}(\|a\|, \|b\|, \|c\| \ldots).$$

We can now define the limit of a sequence and convergent sequences in a field K, on the lines of the ordinary Cantor theory of irrational numbers. A sequence of elements of K, $a_1, a_2 \ldots a_n$ has for limit the element a of K if it is possible for any real positive number ϵ to find a positive integer N such that for any $n > N$ we have $\|a_n - a\| < \epsilon$. A sequence is convergent if for any real positive ϵ we can find a positive integer N such that $\|a_{n+k} - a_n\| < \epsilon$ for all integers $n > N$ and for all positive integers k.

In the non-Archimedean case there is a simplification. It is sufficient for the convergence condition to be satisfied for $k = 1$. Hence the infinite series $b_1 + b_2 + \ldots + b_n \ldots$ is convergent in a non-Archimedean field if, and only if, $\lim\limits_{n \to \infty} b_n = 0$.

If a sequence have a limit it is convergent; but it does not follow that every convergent sequence has a limit. If it does, the field is said to be perfect. We can, however, clearly extend the field K so as to get a perfect field K′.

A fundamental result due to Ostrowski is that besides the non-Archimedean fields only such fields admit of valuation which are isomorphic with a sub-field $\bar{K}$ of the field of all ordinary complex numbers, and that, if to the element a of K we make correspond the element $\bar{a}$ of $\bar{K}$, then $\|a\| = |\bar{a}|^{\rho}$, $0 < \rho \leq 1$. Moreover, all non-Archimedean valuations of the field of rational numbers R are obtained in the following way. If p is a prime, then every rational number a can be expressed in the form $a = p^r u/v$, where u, v are integers not divisible by p and r is an integer. We put $\|a\| = c^r, 0 < c < 1$. The perfect field R′ is then the field of Hensel's p-adic numbers.

Can we value the algebraic extension of a valued field K in such a way that the valuation of K still holds? Every algebraic element a with respect to K satisfies an equation $x^n + a_1 x^{n-1} + \ldots + a_n = 0$, with coefficients in K, which is irreducible in K. Kürschák lays down that all roots of an equation irreducible in K have the same value. Then it follows immediately from condition (2) that the element a, if the problem is to be solved, must have the value $\|a\| = \|a_n\|^{1/n}$. But are conditions (2) and (3) then satisfied? It is easy to prove this for (2), but for (3) it must be assumed that the valued field is perfect. We thus get a valuation of any algebraic extension of the perfect field K, and Ostrowski shows (*Crelle*, **147**, 1917, 196) that this is the only way of solving the problem.

Kürschák makes use of the theory of power series and of Hadamard's theorem, but expresses the opinion that in the case of non-Archimedean valuation the theorem might be proved more easily by a method based on that of Hensel for the

algebraic extension of the field of the p-adic numbers. This has now been done by K. Rychlik (*Crelle*, **158**, 1923, 94–107).

Quadratic Forms.—As far back as 1798 Gauss proposed as the two principal problems of the theory of quadratic forms, first the determination of criteria for the possibility of representing a given number n by a quadratic form f, and secondly the determination of conditions for the equivalence of two quadratic forms f and g. Recently H. Hasse (*Crelle*, **152**, 1922, 120, 205) has dealt with these questions, taking for the domain of all quantities entering (coefficients, variables, numbers represented) the field K (1) of all rational numbers. His method depends on Hensel's theory of p-adic numbers. Both problems are, however, special cases of a more general one, which he now discusses (*Crelle*, **153**, 1923, 12–43). If A, B are the symmetrical square matrices which arise from the coefficients of two quadratic forms, what are the necessary and sufficient conditions for the existence of an equation $S'AS = B$, where S is some rectangular matrix in K (1) and S′ is the transposed matrix? It is possible to reduce the problem to the more simple case in which A and B are of ranks n and m, where A has n and B m rows and columns; S must then have n rows and m columns. If $m = 1$ we have the first problem; if $m = n$, the second. The criteria can be expressed by means of the complete system of invariants with respect to rational transformations.

Hasse then goes on to discuss the case, hitherto excluded, in which the matrix B is identically null, *i.e.* the conditions for the equation $S'AS = 0$, in K(1), with A and S not vanishing identically. This problem is closely connected with one discussed by Minkowski, *viz.*, When is a quadratic form equivalent to a rational multiple of another? The criteria are now expressed by means of the complete system of invariants with respect to rational transformations *and multiplication*. In a subsequent paper (*ibid.*, 113–30) the author attacks the first problem for any algebraic number field.

Minkowski, by means of the methods developed in his *Geometrie der Zahlen*, proved the theorem that if $\xi = ax + by + \xi_0$ and $\eta = cx + dy + \eta_0$ are linear functions of x and y whose determinant $(ad - bc)$ is unity, then there are always integral values of x, y such that $|\xi\eta| \leqslant \frac{1}{4}$. The theorem is a generalisation, and at the same time the best possible improvement of an inequality due to Tschebyscheff. W. Sherrer (*Math. Ann.*, **89**, 1923, 255–9) gives a simpler proof of the theorem.

Theory of Functions.—Three directions in which the theory of functions of a complex variable has been extended in recent years are (1) the behaviour of a function as we approach along

a straight line to an essential singularity ; (2) the distribution of the zeros of integral functions ; and (3) the analytic continuation of a function-element defined by its power-series development. G. Pólya (*Math. Ann.*, **88**, 1923, 179–91) gives a general account of work along these three lines, showing how they may be brought into connection by means of a suitable graphical representation. He confines himself, however, to simple special cases. Suppose we take an integral function of the exponential type, *i.e.* a function $F(z)$ which satisfies throughout the complex plane an inequality

$$|F(z)| < Ae^{a|z|},$$

where A, a are positive constants. Let us approach the essential singularity $z = \infty$ along the straight line from the point $z = 0$ in the direction ϕ. Form the expression

$$\overline{\lim_{r \to \infty}} \frac{\log |F(re^{i\phi})|}{r} = h(\phi).$$

Then $h(\phi)$, a function of the variable angle ϕ, expresses how the growth of $F(z)$ along such a straight line depends on its direction ; $h(\phi)$ may be called the *indicator* of the function $F(z)$. It is real function of period 2π, but it satisfies an inequality which may be expressed geometrically by saying that it is the *support-function* (*Stützfunktion*) of a convex region lying wholly in the finite part of the plane. The edge of this convex region, the envelope of the straight lines $x \cos \phi + y \sin \phi - h(\phi) = 0$, is completely determined by the integral function $F(z)$ of exponential type ; the region may be called the *indicator-diagram* of $F(z)$. An example is afforded by the exponential sum

$$A_1e^{a_1z} + A_2e^{a_2z} + \ . \ . \ . \ + A_ne^{a_nz}.$$

Draw the smallest convex polygon which encloses the points $a_1, a_2, \ . \ . \ . \ a_n$ of the complex plane. Then the image of this polygon in the real axis is the indicator-diagram of the function. Next, let the number of zeros of $F(z)$ within the circle $|z| \leqq r$ be denoted by $N(r)$. Form the greatest and least of the limits of $N(r)/r$ as r tends to infinity ; in the case of an integral function of exponential type these limits are finite, they may or may not be equal. Call them the upper and lower density of the zeros. We then have the theorems that the lower density of the zeros is not greater than the circumference of the indicator-diagram divided by 2π, and that the lower density of the zeros on a straight line from $z = 0$ is not greater than the breadth of the indicator-diagram, perpendicular to the direction of the line, divided by 2π, *i.e.* is not greater than

$$\{h(\phi - \tfrac{1}{2}\pi) + h(\phi + \tfrac{1}{2}\pi)\}/2\pi.$$

The connection with the third line of research depends on the remark that the power series

$$a_0 + \frac{a_1 z}{1!} + \frac{a_2 z^2}{2!} + \ldots = F(z)$$

represents an integral function of exponential type if, and only if, the power series

$$\frac{a_0}{z} + \frac{a_1}{z^2} + \frac{a_2}{z^3} + \ldots = f(z)$$

converges in a non-vanishing neighbourhood of the point ∞. The function $f(z)$ is called the Borel-transform of $F(z)$ (and may be denoted by $BF(z)$); as an example, the Borel-transform of the exponential sum mentioned above is $\Sigma A/(z-a)$. We then have the following theorem: The function-element $BF(z)$ can be continued from the point $z = \infty$ to any point lying outside the convex region obtained by reflecting the indicator-diagram of $F(z)$ in the real axis. Every point on the boundary of this region is a singular point, unless it is an inner point of a straight portion of the boundary. Thus, for example, in the case before quoted the corners of the polygon are the singular points.

P. A. MacMahon (*Proc. Roy. Soc.*, A, **104**, 1923, 39–47) has been led in combinatory analysis to consider a class of transcendental functions of which the Bessel functions are a particular case. They are defined by

$$E_{n,m}(z) = \sum_{0}^{\infty} \frac{(-)^r z^{n+rm}}{2^{n+rm}(r!)^{m-1}(n+r)!},$$

and reduce to the Bessel functions $J_n(z)$ for $m = 2$. The function E satisfies a differential equation of order m—

$$[(\vartheta - n)^{m-1}\{\vartheta + (m-1)n\} + (\tfrac{1}{2}mz)^m]y = 0, \text{ where } \vartheta \text{ denotes } z(d/dz),$$

and various difference relations analogous to those for Bessel functions can be obtained.

Orthogonal Systems.—A normal orthogonal system (ϕ) for the interval (a, b) is a system of real functions $\phi_0(x)$, $\phi_1(x)$ . . . $\phi_n(x)$ . . . , continuous in (a, b) for which

$$\int_a^b \phi_\mu(x)\phi_\nu(x)dx = \begin{cases} 0, \text{ if } \mu \neq \nu \\ 1, \text{ if } \mu = \nu \end{cases} (\mu, \nu = 0, 1, 2 \ldots).$$

If $g(x)$ is any function continuous for $a \leqslant x \leqslant b$, the formally constituted series $F = \sum_{0}^{\infty} k_r\phi_r(x)$, where $k_r = \int_a^b g(x)\phi_r(x)dx$, is called the Fourier series of $g(x)$ belonging to (ϕ). The question then arises: Is F convergent, or if not, can it be summed by the method of arithmetic means?

Criteria have been given by Haar for a system (ϕ) to be summable to the kth order (in Cesàro's sense), and it has been proved that the trigonometric functions, the Legendre functions, and the Sturm-Liouville functions are summable to the first order (Fejér, *Math. Ann.*, **58**, 1904, 57–69; Haar, *Diss.*, 1909; Gronwall, *Math. Ann.*, **74**, 1913, 213–70). A recent contribution to the subject has been made by G. Wiarda (*Crelle*, **153**, 1923, 44–60). He first develops the theory of the Laguerre polynomials defined by $\dfrac{e^{-uz/(1-z)}}{1-z} = \sum\limits_0^\infty L_n(u)z^n, |z| < 1$, obtaining results about the zeros of $L_n(u)$, which, as is known, are all real, positive, and different, and shows also that the arithmetic mean of $L_0^2 + \ldots + L_n^2$ tends to zero as $n \to \infty$. [Other recent papers on these functions are by Szegö (*Math. Zs.*, **1**, 1919) and E. R. Neumann (*Jahresber. D. Math. Ver.*, **30**, 1921, 15–35).] Wiarda then obtains an orthogonal system connected with the Laguerre polynomials, namely:

$$\phi_n(x) = \frac{\sqrt{2}}{1+x}\, e^{-\frac{1}{2}\frac{1-x}{1+x}} L_n\left(\frac{1-x}{1+x}\right),$$

for which use is made of the equation

$$\int_0^\infty e^{-u} L_m(u)\, L_n(u)\, du = \begin{cases} 0, \text{ if } m \neq n \\ 1, \text{ if } m = n. \end{cases}$$

These functions ϕ are not Sturm-Liouville functions as the second order differential equation which they satisfy is not self-adjoint. The author proves that this system is certainly summable to the second order, but he cannot decide about the first order.

He then builds up a system which is summable to no finite order.

Spherical trigonometry made a great advance from the theoretical point of view by the work of Study: *Sphärische Trigonometrie, orthogonale Substitutionen und elliptische Funktionen* (*Abh. Leipzig*, **20**, 1893, 85), in which he considered not only an ordinary spherical triangle, but with it the *Nebendreiecke*. H. Wolff (*Crelle*, **153**, 1923, 66–75) considers from an elementary point of view the same extension for plane triangles; Study had referred to it, but in connection with orthogonal substitutions and the addition theorem of the Weierstrass σ-function.

Three real points in a plane are thus the corners of 16 triangles, for each of which the sum of the exterior angles is a multiple of 2π. The exterior angle a is the positive rotation about A which brings $+ b$ on to $+ c$. From the sides and angles of one of these triangles, say the ordinary Euclidean triangle, we can get to the sides and angles of the others by means

of the substitutions of a group. The ordinary trigonometric formulæ, expressed in terms of the exterior angles, have now a unity and a region of validity which were concealed before. For instance, the cosine formula, $a^2 = b^2 + c^2 + 2bc \cos \alpha$, is invariant for all the substitutions, but the formula for the radius of the inscribed circle, $2\rho = (-a + b + c) \cot \frac{1}{2}\alpha$, leads by two of the substitutions into the formula for the radius of the described circle touching a, $2\rho_a = (a + b + c) \cot \frac{1}{2}\alpha$, and so on.

For the study of such formulæ as $\cot \frac{1}{2}\alpha = \sqrt{(s-b)(s-c)/s(s-a)}$ the triple infinity of triangles are represented as the points of a three-dimensional space, taking the sides, positive or negative, as rectangular Cartesian co-ordinates. The three cotangents are two-valued algebraic functions of position in space, and we can trace their behaviour, the critical values occurring where $a \pm b \pm c = 0$, giving four planes through a point, which divide the space into 14 regions. The angles and form of a triangle only depend on the ratios of a, b, c, so that to the points of a line through the origin corresponds essentially one triangle. We have then to consider a division of the surface of a sphere into regular triangles and quadrilaterals.

Geometry.—Both Cremona and Sturm in their prize essays on the cubic surface (1868) gave a classification of real cubic surfaces according to the number of *real* straight lines which lie upon the surface. There are five varieties: (1) 27 real lines; (2) 15 real lines, the other 12 being imaginary and forming a double-six of which corresponding lines are conjugate imaginaries; (3) 7 real lines, there being also 2 pairs of conjugate-imaginary intersecting lines and 8 pairs of conjugate-imaginary skew lines; (4) 3 real lines, 6 pairs of imaginary intersecting lines and 6 pairs of conjugate-imaginary skew lines; (5) 3 real lines and 12 pairs of conjugate-imaginary intersecting lines. To obtain a representation of the surface upon a plane, we may draw the transversal from a variable point of the surface to two skew lines of the surface and take its intersection with a plane; such a transversal drawn from a real point will be real if the two lines be real or conjugate imaginaries. Thus in cases (1) to (4) we can get such a plane representation, real points of the plane corresponding to real points of the surface, and *vice versa*. But in case (5) the method fails to give such a real representation; and the alternative method of drawing the chord from a variable point of the surface to one of the twisted cubics on the surface likewise fails in this case—the surface has no real twisted cubics.

The fact is that while in cases (1) to (4) the cubic surface is unipartite, in case (5) it is bipartite. [Just as we distinguish in the geometry of plane curves between a branch and a circuit, so here we must distinguish between a sheet (Mantel) and a

part (Schale); a hyperboloid may have two sheets, but has only one part.]

The existence of bipartite cubic surfaces was first pointed out by Schläfli (*Annali di mat.*, **5**, 1873, 289); it is also mentioned by Klein and Zeuthen in their investigations of the form of the cubic surface, but they do not pay much attention to this particular variety. As a matter of fact this surface seems to possess only negative properties, in this resembling, as Sturm says, the general quartic surface, concerning which Steiner remarked in a letter to Schläfli: "Es wäre fatal, wenn hier die Geometrie ein Ende hätte."

In a recent paper R. Sturm (*Crelle*, **152**, 1923, 1–7) returns to the matter. He considers the generation of a cubic surface by means of a pencil of planes and a pencil of quadrics projectively related, taking as the axis of the plane pencil one of the real lines. All five varieties can be generated in this way; and he shows that it is only the fifth which is bipartite. The real lines belong to a part which consists of two sheets and thus meets the plane at infinity in a real curve; the second part does not meet the real lines and may or may not go off to infinity. The first part is odd, *i.e.* is met by any straight line in an odd number of points, its curve of intersection with a plane may consist of one or two circuits. The second part is even, is met by planes in curves with one circuit only, and has everywhere positive curvature. Since the generation of the surface by three (real) collinear sheaves or three (real) trilinear pencils necessarily leads to a real representation on a plane, which clearly requires that the surface should be unipartite, it follows that this type of surface cannot be generated in either of these ways.

Sturm further shows that the bipartite cubic surface cannot have a real twisted cubic, or any real rational twisted curve with an odd number of apparent double points, *e.g.* it can have no twisted quartic of the second kind. Also, it can have no real twisted sextic of genus 3, etc.

Surfaces of three of the five types can be represented by an equation of the form

$$2xyz + A(x^2 + y^2 + z^2) = B,$$

an equation investigated by Goursat (for rectangular Cartesian co-ordinates) as belonging to surfaces which are symmetrical with regard to the planes of symmetry of a regular polyhedron (1887). For $B/A > A^2$ we get type (1), for $A^2 > B/A > 0$ we get type (5), and for $B/A < 0$ we get type (4). Goursat did not distinguish between the last two cases.

A. Comessatti (*Math. Ann.*, **89**, 1923, 272–97) gives an introduction to a geometrical theory of binary forms, of which

he has given abstracts in various Italian journals. It is well known that a binary form $a_0x^n + na_1x^{n-1} + \ldots + a_n$ can be represented by means of the point of an n-dimensional space with homogeneous co-ordinates $a_0, a_1, \ldots a_n$, and that the rational normal curve of order n, $\rho x_i = \lambda^{n-i}$ $(i = 0, 1, 2 \ldots n)$ is then of fundamental importance, its points representing those binary forms which are perfect nth powers. Comessatti here examines the significance in the theory of forms of the osculating hyperplanes of different dimensions and of such varieties as the variety of chords of the curve. He further investigates in this connection the groups of homographies of the space under which the curve is invariant. He promises further papers on the matter, on the publication of which we may enter into more detail.

F. Klein and S. Lie investigated in 1870 in joint papers a family of curves, in general transcendental, to which, on account of the invariance of a certain cross-ratio (*Wurf* in von Staudt's phrase), the name W-curves has been given. They are defined as the curves of an r-dimensional space which are invariant under a continuous group of automorphic collineations of the space. A general account is given by G. Scheffers (*Encykl. math. Wiss.*, III, D. 4). H. Mohrmann (*Math. Ann.*, **89**, 1923, 260–71) determines all W-curves which are algebraic. His methods belong entirely to the theory of algebraic curves, it not being necessary for his purpose to investigate or integrate the system of differential equations satisfied by all W-curves. In fact, the problem is equivalent to the problem of finding all irreducible algebraic curves for which all the singular branches (*Zweige*) lie in one and the same simplex—a simplex being the figure of $r + 1$ points, not lying in any space of less than r dimensions, together with their k-dimensional joining spaces ($k = 1, 2, \ldots r - 1$). In the course of the work he obtains several theorems on algebraic curves, *e.g.* there is no algebraic curve with one, and only one, singular branch; an algebraic curve with not more than two singular branches is necessarily rational and of equal order and class.

METEOROLOGY. By E. V. Newnham, B.Sc., Meteorological Office, London.

Meteorological Conditions over Greenland.—In the Monthly Weather Report of the U.S. Department of Agriculture Weather Bureau for May 1923 Dr. C. F. Brooks gives a summary of the observations made in Greenland during the summer of 1912 by de Quervain's expedition across the central plateau, and also of the observations made by Mercanton at the same time on the west front of the inland ice at Jakobshavn, several hundred

miles from the west coast. An account is added of the observations of temperature and wind in the upper air at Godhavn, near the west coast, made by Jost and Stolberg during the ensuing winter. The great influence exerted by the Greenland plateau upon the weather of the North Atlantic and Europe makes a meteorological knowledge of that region very desirable.

During the summer under discussion no single case was observed of a depression passing across the inland ice north of de Quervain's route, although some secondaries passed across the southern part of the peninsula. The depressions of the west coast were observed to move north, and it is a curious fact that the warm element of these depressions was supplied by south-east föhn winds that had blown right across the cold uplands of Greenland.

Of particular interest is the region fringing the permanent inland ice sheet on the one hand and on the other the coastal zone where the bare rock becomes exposed during this season. Here the wind blew very frequently from the relatively cold ice sheet towards the heated rocks, and lapse rates of temperature greater than the dry adiabatic rate were invariable, ranging from 1·6° C. to as much as 5·5° C. per hundred metres of height. This wind did not cease towards midnight, when the sun approached the northern horizon and the supply of heat was in consequence discontinued, but a slight decrease occurred on the average.

Föhn winds were much in evidence both on the west and east coasts, but particularly on the east coast. On July 23 the temperature actually reached 16° C. (61° F.) on the east coast, with a westerly gale off the ice sheet.

Sea-breezes were observed frequently near the west coast; where the ice-strewn waters of Davis Strait washed the shores of the snow-free rocky belt, which was much heated by the powerful radiation from the sun. These inshore winds had a very chilling effect upon the climate of the coastal belt. The amount of cloud observed inland was small, averaging only 46 per cent., the principal forms observed being cirrus, cirro-stratus, and alto-cumulus. It is a curious fact that over the inland ice-sheet, where so much of the heating is spent in melting the surface ice, cumulus clouds were occasionally observed. The high clouds moved from some easterly point with only one exception, while those of medium height moved polewards. Many observations of the upper winds were made with the aid of pilot balloons at Godhavn, on the west coast, during the winter of 1912–13, the height reached averaging 6 kilometres. They showed that above the prevailing north-easterly wind near the ground—a deflected branch of the generally prevailing east to south-east winds—southerly winds usually prevailed.

This indicates that the Greenland anticyclone exists up to considerable heights, and must therefore be caused dynamically and not thermally, for were it due to the great density of cold air over Greenland it would disappear at a few kilometres' height, where general westerly winds would be set up as a result of the relative warmth of the air over the open ocean to the south. This conclusion is of the first importance, for it negatives the idea that a general circumpolar whirl of westerly winds exists in the middle and upper portions of the troposphere in the northern hemisphere, as has been supposed by Tesseirenc de Bort and others.

Some observations were made of temperatures aloft with the aid of a captive balloon. On February 24, a warm winter's day, an inversion of 4° C. was observed, the maximum temperature being found from 250 to 400 metres above the ground. At the time of the minimum temperature on that day the inversion must have amounted to 7° or 8° C.

Turbulence in the Lower Layers of the Atmosphere and the Development of Cumulus Clouds.—P. Moltschanoff, of the Aerological Observatory at Pavlovsk, discusses some aspects of turbulence in a paper which appears in the *Meteorologische Zeitschrift* for July 1923.

R. Wenger has shown that the greater ascensional velocity of pilot balloons in the lower layers of the atmosphere, as compared with the ascensional velocity higher up, is probably due to the influence of turbulence. This difference cannot be explained by the decrease of Reynolds' turbulence-coefficient with height, since the latter results in a retardation which at the best is only just compensated for by the decrease in the density of the air. The following figures show that the difference cannot be attributed to ascensional air-currents, for the mean ascensional velocity up to a height of 1 km., expressed in terms of the mean velocity V during the whole ascent, is greatest in the case of balloons with a large "lift," and consequently a relatively large value for V:

TABLE I

Lift in grams.	Velocity up to 1 km.	Mean deviation.	Number of cases.
6–99	1·03 V	± 0·03	6
100–170	1·03 V	± 0·09	11
171–199	1·09 V	± 0·08	42
200–320	1·10 V	± 0·08	19

The data in this table were obtained by W. Peppler in 1913 from observations made at Lindenberg with two theodolites.

In order to obtain an idea as to the real cause of the difference, the velocity of fall of free balloons containing air and a little sand was observed, the free balloons being released from a captive balloon. The observed velocity exceeded by 10 or 15 per cent. that which would have been expected from Reynolds's data. This result eliminates the possibility that the ascensional velocity in the lower layers of the atmosphere is augmented by the balloon being carried into rising currents, for these rising currents would retard a falling balloon.

The question arises as to the real nature of the ascending currents. The ordinary explanation of the formation of cumulus cloud supposes that it is due simply to the condensation of water vapour drawn up from the earth's surface by ascending currents. There are many facts, however, which are not consistent with this idea. For example, the idea of ascending currents implies differences of temperature and humidity in a horizontal direction, consequently condensation must set in at different heights according to the degree of excess of heat and moisture in the place where the ascending current originates. This implies considerable variations in the heights of the bases of individual cumulus clouds on a given occasion, whereas actually there is very little variation. Also in their early stages cumulus clouds show no dome-like structure: they begin as ragged pieces of fracto-stratus some of which disappear, while others develop further and become dome-like. Again, the development of ascending air currents over water is very difficult to explain, yet cumulus clouds are observed over the sea, although not so well developed as over the land.

It is possible, however, to explain the formation of cumulus clouds without having recourse to continued ascending currents. The mixing together of the various parts of the lower layers of the atmosphere by turbulence causes water vapour to be dragged up from the earth's surface to higher layers, there to be distributed according to Dalton's Law. There is, so to speak, an increased diffusion of water vapour upwards, and condensation occurs as a result of the lower temperature aloft. If the humidity of the surface air is so great that its dew-point occurs within the region affected by turbulence, a uniform layer of stratus or strato-cumulus is produced; but if the humidity is smaller and the dew-point occurs above the general layer of turbulence, cloud formation will occur only in those places where the turbulence reaches up to the dew-point. In such places ragged clouds will appear and form nuclei for cumulus formation. If sufficient moisture is condensed to liberate the requisite amount of latent heat, a genuine cumulus cloud will be formed; but it is important to observe that it is not in the first instance a rising current that causes the cumulus

cloud, but the formation of the preliminary fracto-cumulus, which liberates the energy sufficient for the development of such a current.

When the lapse-rate of temperature with height approaches the dry adiabatic the ascending current becomes intensified and a thundercloud may be developed, which will involve a considerable mass of air in the vertical movement. The phenomenon may then be described as one of ascending air. Such a phenomenon cannot continue for an unlimited time, however, for the moisture condensed liberates enormous quantities of heat, which warm the upper layers, while the descending precipitation cools the lower layers, both by reason of the heat absorbed in its partial evaporation and by direct absorption of heat from the relatively warm air. The cloud formation which is caused initially by turbulence thus necessitates the weakening of such turbulence. Only the turbulence of the lowest layers can continue, being maintained by the energy of the general circulation, and so store up energy for the condensation phenomena just described. Nevertheless the clouds themselves diminish the heating of the earth's surface by the sun, and thereby tend to weaken the turbulence near the ground.

The whole process described above constitutes a stabilising tendency in the atmosphere. It acts in such a way that the process, when invoked by any factor, results ultimately in the suppression of that factor, and the normal condition of the atmosphere tends to be restored.

The Characteristics of the Atmosphere up to 200 *Kilometres as indicated by Observations of Meteors.*—In the *Quarterly Journal of the Royal Meteorological Society* for July 1923 there appears an account of an attempt by G. M. Dobson to use the theoretical investigations of Prof. Lindemann (*Proc. R. Soc.*, **102** (Ser. A). 1922, p. 411) into the physics of luminous meteors as a basis for estimating the temperature and pressure of the air at levels above those that can be reached by registering balloons.

According to the theory, a meteor, on first entering the earth's atmosphere, strikes molecules of the air directly. The velocity of the meteor is very great—generally between 12 km./sec. and 100 km./sec. or more—and the impact of a molecule at such a speed will break up the atoms, and nearly all the energy will be at first used up in this way. At a later stage, however, when the meteor has entered less rarefied portions of the atmosphere, a cap of compressed air will be formed in front of the meteor. So great is the compression that the air in this cap will attain generally a temperature between 2,000° and 4,000° Absolute (Centigrade), and the solid substance of the meteor will be vaporised and quickly rendered luminous as a

result of friction between this vapour and the air. The height at which this occurs, together with the brilliance of the meteor and its velocity, all of which can be roughly estimated, enable the mass of the meteor to be calculated, on the assumption that most of the energy is converted into visible radiation. From observations of this kind it is found that the ordinary meteor has, while in the solid state, a diameter of the order of 1 mm. With the length of path and the difference between the heights of appearance and disappearance of the meteor as additional observational data, Lindemann's theory enables the density of the undisturbed air at these two heights to be calculated (the full theory of this calculation and the nature of the assumption that have to be made is not given by Dobson, but the reader is referred to Lindemann's original paper (*loc. cit.*) for full details). A diagram is given showing the variation of density with height obtained in this way from a large number of observations of meteors collected by W. F. Denning, for heights varying from about 20 to 170 km., using the constants for nitrogen in the calculations.

It may be recalled here that registering balloons have furnished information about the mean temperature and pressure up to 20 km. height, and it is known that from about 11 km. to 20 km. there is on the average no appreciable change of temperature, the mean in this region being about 220° A. In order to estimate the density of the air at higher levels, it has been customary to assume that this steady temperature continues to still greater heights. The curve showing the variation of density with height obtained by Dobson shows that the values of density based on a constant temperature of 220°A. agree well on the average with those obtained from observations of meteors up to 50 km., but that above that height Dobson's densities are considerably higher, and his lapse rate of density is such as to imply that above 50 km. the temperature is much higher than 220° A.—probably more nearly 300° A. Notwithstanding the rough nature of the calculations, there seems to be good reason for believing that they are accurate enough to justify some confidence in the existence of this warm region at great heights, for the discrepancy between the densities obtained by the two methods is very great between 100 and 150 km., the one being from 100 to 1,000 times as great as the other. A possible explanation of the high temperature is as follows: Ozone is formed when oxygen is acted upon by radiation of very short wave-length; at the highest levels where oxygen exists the solar radiation must be rich in ultra-violet radiations, which are completely eliminated by the time the sunlight reaches the ground. They doubtless convert a part of the oxygen into ozone, and since the ozone is able to

absorb ultra-violet as well as infra-red radiations, the radiation equilibrium temperature will naturally be high.

A curious characteristic of the statistics relating to the height of disappearance of meteors is the small number of cases of disappearance between 50 and 60 km. This phenomenon is shown clearly in statistics for both summer and winter. The assumption of a big rise in temperature above 50 km. affords a plausible explanation of this peculiarity, for a meteor will naturally volatilise more readily at a high than at a low temperature, and on descending into the colder region below 50 km. the volatilisation will be checked for a time, until the increasing density of the air compensates for the lower temperature. The existence of this minimum in the curve for frequency of disappearance is strong independent support for the correctness of Dobson's conclusions.

The following articles may also be noted :

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PHYSICS. By J. Rice, M.A., University, Liverpool.

The Principle of Correspondence.—No better evidence of the dominating position in Physics which has been attained by the Quantum Theory could be given than the amount of time which Section A of this year's British Association meeting devoted to the discussion of this theory and its consequences. The rapidity of its rise is one of the most remarkable phenomena of the history of Physical Science. Planck's tentative suggestion of 1901 towards a solution of the radiation problem was practically unnoticed at first. Even when Einstein brought it out of obscurity in 1905 by his treatment of the specific heat problem, we shrank quite naturally from the full implications of the entry of this alien idea into our minds. We might accept a discontinuous exchange of energy between matter and radiation, but to ascribe a discontinuous structure to radiation itself was asking too much, despite certain speculations of J. J. Thomson which were to be found in his books and papers of that time. Nor, indeed, was such an extreme idea necessary for the solution of the radiation and specific heat problems, as Planck himself was able to show. But the success of Bohr's theory of atomic structure has driven our thoughts still further, and to-day there is no evasion of the issue possible. Wherever we are concerned with interference and diffraction of radiation (that term being used to cover all wave-lengths, from extreme infra-red to γ-rays), the classical theory of the continuous, coherent electromagnetic wave renders an admirable account of itself. But wherever the production and transformation of radiation are in question (*e.g.* photo-electricity, secondary radiation), we must resort to the view that light is propagated, not in continuous waves, but in light-quanta. Each of these two ideas is appropriate to one of the two groups of phenomena, but is apparently irreconcilable with the other. The dilemma is complete. In the meantime we make shift as best we can, and search for such points of contact as may exist between the two views and exploit them as fully as experimental evidence will allow. It is Bohr who first clearly indicated that this method offers us the best hope for progress until such time as Physical Science is once more endowed with a consistent body of principles which embraces all the facts. In his earlier work of 1913 the point of contact between the classical and quantum theories lay in the fact that the stationary orbits in the atom are assumed to conform to the classical laws of motion. The points of contrast were the assumed stability of these orbits and the absence of radiation so long as the electron remained in one of them. During the past five years Bohr and his pupil Kramers have discovered

and developed another point of contact and named it the "Principle of Correspondence." It appears to the writer of this note that some information concerning this very important principle will be of service to the readers of this Journal, as at present the only references available are to books and journals read by the expert physicist rather than by the general scientific worker.

Previous references to Bohr's theory will be found in this Journal, Nos. 37, 47, 48, 49, 52, 63, 66, 70. As it stands at present, it postulates the existence of "stationary states" for any atom. In any one of these states each electron remains in an orbit which is endowed with an inherent stability unexplained by purely dynamic or electrodynamic reasoning, and it emits no radiation despite its accelerated velocity. Under the influence of external radiation, electronic impact, or some undefined spontaneous activity, an electron can jump from one orbit to another stationary orbit. It is during such a jump (or number of jumps performed simultaneously by different electrons in the atom) that radiation is emitted from the atom; this element of emitted light is assumed to be monochromatic and to possess a frequency given by dividing the difference of energy between the two atomic states by Planck's constant h. Of course, if the final state has greater energy than the initial, an act of absorption has taken place.

The manner of defining the particular orbits which possess the stationary property among the infinity of "mechanically admissible" orbits has been the occasion of much thought and investigation. At present, the method is crudely as follows. Every system such as an atom can be dynamically defined by a number of "co-ordinates" $q_1, q_2, \ldots q_n$. These are most usually a sufficient number of lengths, angles, areas, or geometrical magnitudes to prescribe without ambiguity or redundancy the positions of the nucleus and the several electrons. Each of these co-ordinates is a function of the time, and the kinetic energy of the atom can be expressed as a function of the "velocities" or rates of change of the co-ordinates, $\dot{q}_1$, $\dot{q}_2, \ldots \dot{q}_n$. This energy can also be expressed in the form

$$\tfrac{1}{2}(p_1\dot{q}_1 + p_2\dot{q}_2 + \ldots p_r\dot{q}_r)$$

where $p_1, p_2, \ldots p_r$ are functions of the $\dot{q}$, and are called the "momenta." Now in an atomic system the motion is essentially periodic in character, and we can conceive of an integration such as $\int p_r dq_r$ being effected through a complete period of the co-ordinate q_r. Such an integral is called a "modulus of periodicity." The principle proposed for the selection of "quantised orbits" is that for such an orbit all

its " moduli of periodicity " should be *integral* multiples of h. (It will be observed that h is a quantum of " Action," and the integrals $\int p dq$ are quantities of Action.) Of course the number of ways in which we can choose co-ordinates for a dynamical system is unlimited, and care has to be exercised in choosing co-ordinates suitable for quantisation of the orbits. The discussion of this point would, however, carry us too far into the dynamics of planetary motion. In short, each stationary orbit satisfies the following relations (as many as there are degrees of freedom):

$$\int p_1 dq_1 = n_1 h \;;\quad \int p_2 dq_2 = n_2 h, \text{ etc.,}$$

where n_1, n_2, n_3, etc., are a group of *integers*. These integers are called the " quantum numbers " of the orbit, and each number of the group relates to one co-ordinate.

So far the theory only permits us to calculate the frequency of the emitted or absorbed radiation by the rule given above. It makes no statement concerning intensity or polarisation. Intensity for a particular line will clearly depend on the number of atoms which during a given time jump from one stationary state to another whose difference in energy corresponds to the frequency of the line in question. Quantum theory until recently offered no principle to enable us to solve such problems, but Bohr has put forward as a temporary guide a postulate which links up with classical theory; he calls it the " Principle of Correspondence." According to classical theory, any electron in an orbit would be radiating energy on account of its acceleration, and the theory gave a very direct way for calculating frequency, intensity, and polarisation of such light. The method consists in projecting the motion along three axes, and then analysing each axial vibration into its harmonic components by Fourier's theorem. If there be r degrees of freedom, there would be in general r distinct fundamental frequencies involved, $\omega_1, \omega_2 \ldots \omega_r$, and any axial displacement would be given by the sum of such terms as

$$A \cos \left[2\pi \left(s_1\omega_1 + s_2\omega_2 + \ldots + s_r\omega_r\right) t + \delta\right]$$

where A and δ would be an amplitude and epoch (varying in general from term to term); $s_1, s_2, \ldots s_r$ would be a group of r integers (positive or negative), and theoretically there would be as many of these terms as there are numbers of such groups. (It should be noted that each of the frequencies $\omega_1, \ldots, \omega_r$ relates to one of the co-ordinates $q_1 \ldots q_r$ by means of which quantisation of orbits has originally been effected, and $s_1, s_2, \ldots s_r$ might be called the " order-numbers " in a given term of the Fourier expansion with regard to the individual

co-ordinates.) Now classical theory led to the view that the emitted radiation consists of lines whose frequencies are given by the values of

$$s_1\omega_1 + s_2\omega_2 + \ . \ . \ . \ + s_r\omega_r$$

occurring in such terms of the Fourier expansion as possess values of the amplitude A which are not negligible. That is, classical theory identified the optical frequencies with the mechanical frequencies occurring in the electronic vibration. Bohr's theory denies this, and postulates that radiation is not emitted during motion *in a quantised orbit.* It is emitted when electrons leap from orbit to orbit, and its frequency is determined by energy differences realised in such leaps. But the classical theory also presumed to give information concerning intensity and polarisation of the emitted light by means of the constants A and δ in the various terms of the expansions for the three component mechanical vibrations. For the present, Bohr suggests that the retention of this assumption may give us some help in further development of quantum principles. Briefly put, his Principle of Correspondence may be summarised thus: In every transition from one orbit to another there takes place a definite change in the quantum numbers of the orbit, each quantum-number and its change being related to a definite quantisation co-ordinate. In the Fourier analysis of the orbital motions there occur a series of simple harmonic vibrations, and each of these is characterised by a group of "order-numbers" each related to a definite co-ordinate. We set up a correspondence between each term of the series giving the mechanical vibrations of the electrons and each possible quantum leap by stating that a leap in which the quantum numbers of the orbit change by $s_1, s_2 \ . \ . \ . \ s_r$ corresponds to the term in the series whose order numbers are $s_1, s_2 \ . \ . \ . \ s_r$. Bohr postulates that the information given by the constants A and δ of this term *on the classical view* concerning polarisation and intensity of the light, which was presumed to have an optical frequency equal to the mechanical (*viz.*, $s_1\omega_1 + \ . \ . \ . \ + s_r\omega_r$), is approximately correct for the actual light emitted, whose frequency is given by the energy difference of initial and final orbits; further, the approximation is better the longer the period of light vibration and mechanical vibration involved.

The principle is, as Bohr says, purely "formal"; it finds no obvious support as a deduction from an accepted body of Quantum Dynamical and Optical laws; for such a body does not yet exist. Its approximate character is so far justified by the pragmatic test that it has been applied to a number of special cases where it certainly leads to results concerning polarisation and intensity in remarkably good accord with

experimental fact; notably for the fine structure of the hydrogen lines, the lines of ionised helium, and the Stark effect on such lines. The mathematical difficulties of the applications are formidable and require some ingenuity in the calculation of the A coefficients, concerning which a further and final word may be said. Thus in a quantum leap *two* orbits are involved, and the A and δ constants occurring in a term involving

$$\cos 2\pi (s_1\omega_1 + \ldots + s_r\omega_r) t$$

will differ in general for the two orbits. (The frequencies ω_1, ω_2, . . . ω_r might differ also, but that would not concern us in questions of intensity and polarisation, as frequency is not determined from mechanical vibration.) The method of surmounting this difficulty is as follows: Let the two orbits be defined by the quantum numbers n_1, n_2 . . . n_r and k_1, k_2 . . . k_r, where $n_1 - k_1 = s_1$, $n_2 - k_2 = s_2$, . . . , $n_r - k_r = s_r$. There is an infinity of *mechanically possible* orbits between these two. Each one is given by the fact that its moduli of periodicity are equal to $\{k_1 + \lambda (n_1 - k_1)\} h$, $\{k_2 + \lambda (n_2 - k_2)\} h$, etc., where λ has a value between 0 and 1. These orbits are not, of course, quantum orbits, since the multiples of h involved are not integral multiples. Bohr suggests that the average of all the A amplitudes for these orbits should be used in quantitative applications of his principle.

The original papers are to be found in the *Memoirs of the Royal Academy of Science and Letters of Denmark, Copenhagen*, 8th series, vols. iii and iv. Three reprints, two by Bohr and one by Kramers, translated into English can be obtained from the Academy. A fairly complete account can be obtained in the *Report of National Research Council on the Quantum Theory* (National Academy of Sciences, Washington, D.C.; U.S.A.). Accounts are also accessible in N. Campbell's monograph on *Spectral Series* (Cambridge University Press) and in the recently published English translation of Sommerfeld's *Atomic Structure and Spectral Lines* (Methuen & Co.).

PHYSICAL CHEMISTRY. By W. E. Garner, M.Sc., University College, London.

Electronic Theory of Valency.—Previous to the advent of the electron theory of matter, the study of the valency of chemical compounds had not led to any comprehensive theory. In inorganic chemistry, for example, the intractable nature of the problems of variable valency and electrovalency made a broad generalisation a matter of extreme difficulty. Investigators had perforce to be content with theories limited in their

application to comparatively narrow fields. A certain measure of success was achieved by Werner in the explanation of the structure of the co-ordination compounds, but his views did not appear to be capable of extension to other classes of compounds. On the other hand, the valency problems met with in organic chemistry are relatively simple, and it is possible to account for the structures of nearly all organic compounds without departing from the simple theory of van't Hoff.

At first it was thought that the valencies of carbon and inorganic compounds had little in common, but the electron theory of matter in the hands of G. N. Lewis, Langmuir, Kossel, and others has modified this view and led to a theory of valency which does not make any distinction between the two classes of compounds. The existence of two kinds of valency is postulated, which are known by the names "covalency" and "electrovalency" respectively. "Covalency" is the term given to a valency bond which is formed by electrons shared between the two atoms. The grouping of these two electrons has been the subject of much speculation, and the view has arisen that the valency electrons are normally arranged in pairs or octets. This kind of valency is typified by the carbon compounds and the non-ionisable inorganic compounds. "Electrovalency" occurs only when the atoms in chemical combination are oppositely charged, and in this type of linking it is assumed that electrons have been transferred from one atom to another. Compounds showing electrovalency form that class known as electrolytes. It is probable, however, that no hard-and-fast distinction can be drawn between electrovalency and covalency, and intermediate forms of valency may exist in which a partial transference of electrons between the two atoms occurs. It is customary to explain the polarity of certain groups in organic chemistry as originating in some such manner.

One of the most striking successes of the "octet" theory of valency is the ease with which it may be used to explain the structure of both organic and inorganic compounds. According to the views of Langmuir, the electronic arrangement of ammonium chloride is such that four pairs of electrons are arranged in the form of an octet around the nitrogen atom: this atom thus possesses four covalencies to which are attached hydrogen atoms. This group, NH_4, carries one positive charge, and is united by an electrovalency with a chlorine ion. There is no essential difference in electronic arrangement between NH_4^+ and CH_4 save for the excess of positive charge on the former group.

The earlier "octet" theories of valency, although believed to be correct in principle, have been modified in detail as more light was thrown on the arrangement of the electrons in the

outer shell of the atom. The limitation of the possible stable groups to those containing eight electrons made the explanation of the Werner co-ordination compounds impossible, for these show not only co-ordination numbers of 4, but also those of 6. These compounds are definite exceptions to the " octet " rule. Sidgwick (*J.C.S.*, 1923, **123**, 725) has applied Bohr's theory of the relationships between the groups in the periodic table (see SCIENCE PROGRESS, 1923, **70**, 201) to Werner's co-ordination compounds. He shows that in the case of the compounds of Cr, Fe, Co, Ni, Rh, Os, Ir, Pt, there are 26 electrons in the outer ring of the central atom, just as in methane or in the ammonium ion the central atom has the same number of electrons, 8, as in the outer shell of neon.

These elements lie in that part of the periodic table where the outermost group of electrons is building up subgroups of six and not of four, and the disposition of the attached groups as in $[Co(NO_2)_3(NH_3)_3]$ is governed by the structure of these subgroups of electrons. Thus in these compounds, with the Werner co-ordination number of 6, the octet must be replaced by a group of twelve shared electrons. The stereochemical results show that the six covalencies are arranged at the angular points of an octahedron. Even those compounds showing a co-ordination number of 4 do not form a normal octet based on the tetrahedron as in carbon compounds, for compounds of the type Pt A_2 B_2 are known to occur in geometrically isomeric forms. It would appear that the octahedron structure still persists, even when the number of electrons in the outer shell of the central atom is reduced to twenty-four.

Isotopes.—As the knowledge of the structure of matter increases it is possible that light may be thrown on the laws governing the synthesis of elements. With this end in view, investigations are being made into the relative abundance of the isotopes of the elements. Recently Harkins (*J.A.C.S.*, 1923, **45**, 1426) has summarised the results of his investigation on the relative stabilities, and abundance of the atomic species. He finds that those elements occur in the greatest amount in the earth's crust, and in meteorites, in which the atomic number and isotopic number are even. (The isotopic number is defined as the atomic weight—twice the atomic number.) He shows that, out of a total of 80 elements, 55 per cent. have both atomic and isotopic numbers which are even, 26·25 per cent. in which both of these numbers are odd, and 15 per cent. in which the atomic number is even and the isotopic number is odd, while in only 3·75 per cent. is the atomic number odd and the isotopic number even. From his analysis, Harkins claims to be able to predict the atomic numbers of the isotopes of any element from the chemical atomic weight. An interesting example, for which

the atomic weight has recently been determined by Richards (*J.A.C.S.*, 1923, **45**, 1155), is the element gallium. This has an atomic weight 69·716, and assuming the validity of the "whole number rule," this value indicates the existence of at least two isotopes of gallium. These isotopes, according to Harkins, will have the isotopic numbers 7 and 9, and atomic weights 69 and 71. Aston (*Nature*, 1923, **112**, 449), by use of the method of accelerated positive rays, has examined this element in the form of fluoride, and the results show that Harkins' prediction is correct. The intensity relation between the lines is in agreement with the newly found atomic weight. In the same paper, Aston determines the atomic weight of strontium. The atomic weight of this element used by chemists is 87·63, and if this value be correct, there should be at least two isotopes. By the positive-ray method, it is shown that only one isotope is present with atomic weight 88. This points to the chemical atomic weight being too low.

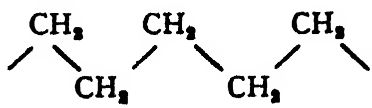
The discovery of the new element hafnium by the examination of the Röntgen spectrum of zirconium minerals is but one example of the important part which the new experimental and theoretical technique of physics is playing in the development of chemistry. The importance of these methods has been clearly emphasised by the method suggested by Coster (*Z. Electrochem.*, 1923, **29**, 344) for the analysis of mixtures of elements. An element may be defined without any possibility of error by its Röntgen spectrum, which may thus be used for purposes of identification. Owing to their simple character, these spectra are more easily interpreted than is the case with those of spectroscopy, although the degree of sensitivity at present attainable with Röntgen rays is much less than by means of the spectroscope. Mixtures of zirconium and hafnium phosphates, and of antimony and tin have been successfully analysed by the new method.

Length of Carbon Chains in the Fatty Acids.—Evidence is being accumulated as to the shape, size, and orientation of molecules in the crystalline and liquid crystalline states of matter. The structure of the long-chain aliphatic molecules is of special interest, as here the X-ray method of determining molecular dimensions may be contrasted with measurements of another kind. The length of carbon chains may be determined from the area to which a small quantity of organic substance will spread on water. Assuming an oriented monomolecular layer on the surface, Langmuir from such measurements was able to determine the lengths of carbon chains. These measurements have been repeated, recently, with an improved technique by Adams (*Proc. Roy. Soc.*, 1922, **101**, 452), and the lengths of the carbon chain in a number of acid films have been determined.

Thus myristic acid gives 21·1 Å; pentadecoic acid, 22·4 Å; stearic acid 26·2 Å; behenic acid, 31·4 Å.

The long chains of fatty acids may be detected by X-rays (De Broglie, *Compt. Rend.*, 1923, **176**, 738). Muller (*J.C.S.*, 1923, **123**, 2043) finds that when the fatty acids are melted in thin films on microscope slides, they give crystalline flakes which orient themselves parallel to the glass surface. When the acids are examined under these conditions, lines are obtained the distances between which are different for the different substances investigated. The spacings in the crystals corresponding with these distances, which are the lengths of the carbon chains, increase as a homologous series is ascended. From capric acid to behenic acid, the length of chain increases from 23·2 Å to 47·8 Å, and the increase for one CH_2 group is approximately 2 Å.

Results have been obtained by Piper and Grindley (*Proc. Phys. Soc.*, 1923, **35**, 269) from X-ray photographs of soap curds. These curds, which are liquid crystals in the smectic state, possess a fluidity which is limited to one set of planes. They are crystalline in two directions only. It is shown that the minimum distance between two slipping planes corresponds to twice the length of a molecule, the two molecules being arranged end to end with the carboxyl groups outwards. The effective length of the CH_2 group is given as 1·25 Å, and this distance is the same as that calculated from the dimensions of the carbon atoms in the diamond, assuming that the carbon atoms in the soap curd form a zigzag chain,



and that the angle between the lines joining the carbon centres is 109° 28′ as in the diamond.

An interesting confirmation of the view that acid films on water are monomolecular is supplied by the close agreement between the areas ascribed to the carboxyl group by Langmuir and the above investigators respectively. Langmuir, for thin films on water, found $21 - 22 \times 10^{-16}$ cm.^{2} for the cross-section of this group, and this value has now been confirmed by X-ray methods. The carboxyl group is shown to sit on a rectangular space 4·2 Å by 4·9 Å.

W. B. Hardy and Ida Doubleday (*Proc. Roy. Soc.*, 1923, **104** A, 37), from measurements of static friction of lubricants, deduce that the orientation of polar molecules on a metal or glass surface takes a measurable time before it is complete. The coefficient of friction for caprylic acid changes from 0·57

to 0·43 over a period of 60 seconds. It is believed that two primary films are formed, and that orientation is extended slowly into the bulk of the liquid in so far as it is not upset by heat motion. The liquid film between two metal surfaces would thus possess some of the properties of liquid crystals.

Niels Bjerrum (*Zt. Phys. Chem.*, 1923, **106**, 219) has calculated the molecular dimensions of carbon chains from the first and second dissociation constants of the dibasic organic acids. It would be anticipated on purely statistical grounds, from the number of acid groups and the collision of hydrogen ions with these groups, that the first dissociation constant of the dibasic acids would be four times as great as the second. The differences between the two dissociation constants are always greater than this, and are the more marked the nearer the two carboxyl groups are to one another. If $p_1 = \log k_1$, and $p_2 = \log k_2$, then $p_2 = p_1 - \log 4$, assuming that the ionisation of one carboxyl group is without influence on the other. It is shown that $n = p_2 - p_1 - \log 4$ ranges from 2·33 for oxalic (C_2) to 0·42 for sebacic acid (C_{10}). Thus even when the two carboxyl groups are separated by eight carbon atoms, the difference between the two dissociation constants is greater than that expected on purely statistical grounds. There must be a definite action between COO′ and the second carboxyl group, which is of such a nature as to hinder the ionisation of this group. It is shown that the transmission of any effect along the carbon chain is unlikely, and that the low value for k_2 can be explained on electrostatic grounds. There is an increase in concentration of hydrogen ions around the COO′ group, which will affect the second group in a manner which decreases with the length of the carbon chain. It is shown that d, the length of the carbon chain for C_9, is $7{\cdot}8 \times 10^{-8}$ cm.; for C_5, $d = 5{\cdot}3 \times 10^{-8}$; and for C_3, $d = 3{\cdot}8 \times 10^{-8}$ cm. The calculated values, assuming $\Delta\, CH_2 \times 1{\cdot}25$ Å, should be 11·25, 6·25, and 3·75 respectively. The divergence between the two sets of figures is conceivably due to the bending of the carbon chain.

Walden and Ulrich (*Zt. Phys. Chem.*, 1923, **106**, 49) have made measurements of the conductivity of a number of salts at 0°, 18°, and 100° C., and find that the mobility of the picrate ion, over the range of temperatures investigated, obeyed with sufficient accuracy Stokes' law. The increase in mobility with rise in temperature is due to changes in the fluidity of water.

Monochloramine NH_2Cl.—Markwald and Wille (*Ber.*, 1923, **56**, 1319) have succeeded in isolating this interesting substance from a 10 per cent. aqueous solution by desiccation of the vapour and its condensation in a U-tube surrounded by liquid air. It is a colourless crystalline substance, m.p. − 66°, decom-

posing, sometimes explosively, at − 50° into nitrogen, chlorine, and ammonium chloride.

Selenium Trioxide.—Many attempts have been made to prepare the selenium analogue of the trioxide of sulphur, but without success. Worsely and Baker (*J.C.S.*, 1923) have now prepared this substance by subjecting metallic selenium in solution in the oxychloride of selenium to the action of ozone. It combines with dry hydrogen chloride to give chlorselenic acid.

BIOCHEMISTRY.

By R. Keith Cannan, M.Sc., University College, London.

The Eleventh International Congress of Physiology attracted to Edinburgh last July some five hundred workers representing twenty-two countries. This—the largest congress yet held—gave almost the first opportunity since the war for a personal exchange of views between physiologists and biochemists of different countries. The opportunity was not neglected. A long programme of demonstrations and communications included much work of direct biochemical interest which, however, apart from that which has already appeared in the literature, was not of a nature capable of brief review. The subject of diabetes and the physiology of insulin was, naturally, much to the fore. Another significant feature was the growing tendency to turn to physico-chemical theories in the elucidation of biochemical problems.

The Neutrality of the Blood.—The difficulties connected with the vital control of the reaction of the tissues and body fluids continue to attract research workers. From the broad generalisations of the earlier investigations we are moving surely towards a more careful and quantitative definition of the many factors involved. It has been necessary to recede somewhat from the view that the reaction of the blood is incapable of appreciable alteration in life. Thus, a pH of 7·05 has been recorded in a normal individual after a short bout of strenuous exercise (Barr, Green, and Hamwich, *Jour. Biol. Chem.*, 1923, **55**, 495), whilst Davis, Haldane, and Kennaway report a blood pH of 7·85 after forced breathing in man (*Jour. Physiol.*, 1920, **54**, 32). The carriage to the lungs of the carbon dioxide constantly produced by the tissues is, of course, the function by which the buffering efficiency of the blood is chiefly tested under normal conditions. The predominant part played in this by hæmoglobin is now well established. The removal of oxygen from the blood by the tissues involving the change from the acid, oxyhæmoglobin, to the weaker acid, hæmoglobin, must liberate, at constant reaction, some base which will become

available for neutralisation of the incoming carbon dioxide. The excess of the latter not so neutralised will, by a reduction of the pH, increase the dissociation of the oxyhæmoglobin and so render available at the same time more oxygen to the tissues and more base to the carbonic acid. Thus hæmoglobin functions as a buffer, firstly by loss of oxygen—isohydrically—and secondly, together with the phosphates and serum proteins, under the influence of a change in hydrogen-ion concentration. Summing these two effects, Van Slyke, in an able review of the subject (*Physiol. Reviews*, 1921, **1**, 141), has calculated that the base liberated by the hæmoglobin in the change from arterial to venous blood is sufficient to account for about 84 per cent. of the bicarbonate of the latter. Recently Doisy, Briggs Eaton, and Chambers (*Jour. Biol. Chem.*, 1922, **54**, 305) have carried out a careful evaluation of the buffering systems of the blood which, in spite of the crude assumptions essential to such a project, provides a summary worthy of quotation.

Carbon dioxide carried by buffer systems studied in blood of W. H. C. (*Jour. Biol. Chem.*, 1922, **54**, 305):

	Vol. per Cent.	Per Cent. of Total.
Total CO_2 carried for R.Q. of 0·75	4·23	—
$BHCO_3$ carried isohydrically, $BHbO_2 \rightleftharpoons HHb$	2·262	53·5
$BHCO_3$ carried by change of pH—		
By hæmoglobin : $BHbO_2 \rightleftharpoons HHbO_2$		
$BHb \rightleftharpoons HHb$ . .	1·070	25·3
By B_2HPO_4 in cells	0·012	0·3
By separated serum	0·198	4·7
CO_2 physically dissolved	0·511	12·1
Sum, per cent. of total	4·053	96·0
Per cent. of total carried by hæmoglobin .	—	78·8

pH (recalculated) of arterial blood . .	7·310
pH (recalculated) of venous blood . .	7·280

The significant fact emerges, therefore, that the main buffer of the blood is contained wholly within the corpuscles. Although the corpuscle is freely permeable to carbon dioxide, only about half of the 84 per cent. of the total carbonic acid for which the hæmoglobin supplies the base is carried within the corpuscle. Since the corpuscle is impermeable to the cations present, it follows that some acid must diffuse in from the serum to unite with this base and make room in the serum for the incoming carbon dioxide. It is now accepted that it is hydrochloric acid which migrates. Doisy and Beckmann (*Jour. Biol. Chem.*, 1923, **54**, 883) have shown experimentally a difference between the concentrations of plasma chlorides of arterial and venous blood. It has been calculated (Doisy and others, *Jour. Biol. Chem.*, 1922, **53**, 61) that of the total base furnished by the

blood in changing from a pH of 7·45 to one of 7·25 to form serum bicarbonate, 80 per cent. is obtained by the migration into the corpuscle of hydrochloric acid. The phenomenon receives a plausible explanation in terms of Donnan's theory of membrane equilibria (Warburg, *Biochem. Jour.*, 1922, **16**, 153).

The part played by the kidney in regulating blood and tissue reaction has been extended by the observation of Nash and Benedict (*Jour. Biol. Chem.*, 1921, **48**, 463) that the kidney can produce ammonia to neutralise excess of acid, and thereby enable the latter to be excreted without loss of base to the body. Thus is the liver—that much-overworked organ—relieved of one more of its manifold functions!

J. B. S. Haldane and his associates have indicated an interesting relation in urinary secretion. Administration of alkali to an animal may result in the excretion of considerable amounts of bicarbonate in the urine, but these workers show that the chloride excretion is correspondingly reduced, so that the combined urinary Cl' and HCO_3' shall not exceed a concentration of 0·3 normal (*Jour. Physiol.*, 1923, **57**, xli).

The Physiology of the Sulphur Bacteria.—A notable contribution to biochemistry will be found in a recent study of a member of the group of sulphur bacteria—*Thiobacillus thio-oxidans.* In a series of papers S. A. Waksman and his co-workers report researches on the isolation, culture, and morphology (*Jour. Bact.*, 1922, **7**, 231, 239, 605, 609; and *Soil Science*, 1922, **13**, 329), and on the physiology (*Jour. Gen. Physiol.*, 1923, **5**, 285) of this fascinating organism. Originally found in soil and rock phosphate composts, *thio-oxidans* was successfully isolated in pure culture and grown in a simple inorganic medium. It derived its energy chemosynthetically by the oxidation of sulphur or thiosulphates to sulphuric acid, the optimum reaction for growth being in the neighbourhood of pH 2·5. An active culture may attain a reaction of pH 0·6, and the organism will grow in normal sulphuric acid! The author believes that no other authenticated case of an organism so resistant to acid has been reported till now. An organism capable of deriving its energy from sulphur, its carbon from carbon dioxide without the intervention of light, its nitrogen from ammonia, and whose mineral requirement is small—this is an intriguing subject for biologists. As Waksman suggests, *thio-oxidans* may well represent the type of some of the earliest forms of life on this planet.

A study of the quantitative relations between sulphur oxidised and carbon dioxide assimilated leads Waksman to calculate an efficiency of 6·65 per cent. in the process of carbon assimilation. It is interesting to compare this with the efficiency of 5 per cent. found by Meyerof for the two other autotrophic

bacteria—the nitrate and the nitrite bacteria—and with the value of 0·5 to 3 per cent. derived from studies of the higher photosynthetic plants. Apart from the intrinsic interest of this study and of the possible practical value to the agriculturist, it will perform a further service if it direct the attention of biochemists to the neglected problems of the metabolism of unicellular organisms.

The Anti-rachitic Factor and the Action of Light in the Prevention of Rickets.—The competing views of the ætiology of rickets seem to be drawing into a measure of sympathy as a result of studies of the relation of light to the prevention of this condition. Whilst Mellanby has held that rickets was a purely deficiency disease (*Med. Res. Spec. Rept. Ser. No.* **61**, 1921), the Glasgow school were of the opinion that its cause was to be found in confinement and defective hygiene generally (*Lancet*, 1922, 1594). It would appear that both factors are involved. As long ago as 1912 Raczynski (*Compt. rend. soc. Ass. intern. de pediat.*, 1912, 308) showed that lack of sunlight was an ætiological cause of rickets, and quite recent work has confirmed this again and again. The active rays seem to be somewhere in the ultra-violet region, about 300 $\mu\mu$ or shorter (Hess and others, *Proc. Soc. Exper. Biol. and Med.*, 1923, **20**, 14). Goldblatt and Soames (*Biochem. Jour.*, 1923, **17**, 294) find, however, that irradiation of animals by the mercury vapour quartz lamp will not completely replace the fat-soluble vitamin in growth or development. There is still a requirement, reduced in amount, for the anti-rachitic factor. On the other hand, Sherman and Pappenheimer (*Proc. Soc. Exper. Biol. and Med.*, 1921, **18**, 193) find that lack of this factor results in rickets only when there is a simultaneous disproportion in the dietary calcium and phosphorus. It is to be noted in this connection that Kramer and Howland (*Johns Hopkins Hosp. Bull.*, 1922, **33**, 313) have found that the calcium and phosphorus content of the blood on a diet deficient in these elements is increased by exposure to sunlight.

The view of McCollom and his associates (*Jour. Biol. Chem.*, 1922, **53**, 292, 505) that the anti-rachitic factor is distinct from the fat-soluble growth factor is gaining ground. At the same time, there is a feeling that they are in some way related to one another. Thus Zucker (*Proc. Soc. Exper. Biol. and Med.*, 1923, **20**, 20) finds them both in the cholesterol-free, unsaponifiable matter of cod-liver oil. Further, Hume has shown that ultra-violet light retards the onset of the symptoms of deficiency in the growth factor, as it does in the case of the anti-rachitic factor. In the latter connection an amusing observation is reported by Hume and Smith (*Biochem. Jour.*, 1923, **17**, 364). They found that animals placed in jars the

inside of which had previously been irradiated with the mercury vapour lamp achieved a measure of protection against deficiency in vitamin A. No protection was afforded to animals kept in jars through which irradiated air was drawn. Confirmation and interpretation of this surprising result are awaited with interest. It seems probable that both light and the anti-rachitic factor function in the prevention of rickets ; probably by maintaining efficiently a normal metabolic process connected with the utilisation of calcium and phosphorus.

Bone Formation.—An interesting paper by Robison (*Biochem. Jour.*, 1923, **17**, 286) continues the study of the hexose monophosphoric ester recently isolated by him. Robison has followed the distribution in the tissues of an enzyme which hydrolyses the soluble calcium salt of this acid with the precipitation of calcium phosphate. All ossifying cartilage tested contained the enzyme, and the only other tissue to give appreciable results was the kidney. Again, whilst most tissues are capable of hydrolysing one phosphoric acid group from hexose diphosphoric acid, only ossifying cartilage and kidney were found capable of removing the second with ease. The significance of this enzyme in the process of ossification is discussed, and some interesting preliminary experiments in this connection are described. It is reasonable to wonder by what steps this study may become linked up with the problems of the ætiology of rickets discussed above.

Tissue Respiration.—Further publications from Prof. Gowland Hopkins' laboratory continue the elucidation of the function of glutathione in cellular respiration. The constitution of this dipeptide of glutamic acid and cystine has now been fully worked out (*Biochem. Jour.*, 1923, **17**, 586). A study of the kinetics of the reduction of methylene blue by glutathione yields the fact that the reaction is autocatalytic, and the authors are led to the conclusion that the oxidised form of the dipeptide catalyses the reduction of methylene blue by the reduced form. Further, the oxidation of the reduced form by atmospheric oxygen is catalysed by the oxidised form (*Proc. Roy. Soc.*, B, [illegible], 1923, 266). This paradoxical behaviour is probably to be explained by the formation of a compound of the reduced form (R-SH) with the oxidised form (R-S-S-R), which is more active than the free molecule RSH.

MINERALOGY. By A. Scott, M.A., D.Sc.

Some interesting results have been obtained by G. W. Morey and N. L. Bowen (*Amer. Jour. Sci.*, **4**, 1, 1922) in an investigation of the melting phenomena of potash felspar. When specimens of this mineral, either natural or synthetic, are heated to

temperatures above 1,200° C., a glass is apparently formed. On careful examination, however, lines suggestive of some crystal structure can be detected in this glass and, by heating to higher temperatures, skeletal crystals develop, which in turn give rise to spherical grains of leucite. Orthoclase, therefore, is a compound with an incongruent melting-point, and in this respect differs from the corresponding soda-felspar, albite. At a temperature below 1,200° C. the former breaks up to give a mixture of solid leucite and a glass. The importance of these facts in connection with the origin of certain types of igneous rocks, such as nephelite-syenite, is discussed by Bowen. It is possible that leucite and quartz, the former as phenocrysts, the latter as groundmass, may coexist in certain rocks ; thus the pseudoleucite of borolanite may have originated. It may also be remarked that the physico-chemical possibility of the co-occurrence of leucite and quartz is not in favour of the use of the so-called " saturation principle " in rock classification.

In a paper on the structure and polymorphism of silica, R. B. Sosman (*Jour. Franklin Inst.*, **194**, 741, 1922) endeavours to find a series of hypotheses which will explain the properties of the various forms of this oxide. At the present time the only one of the seven crystalline modifications of silica, which has been investigated by means of X-rays, is the low temperature form of quartz. The author rejects the structure advocated by Bragg (*X-rays and Crystal Structure*, 160, 1915) in favour of that advanced by Beckenkamp (*Zeit. anorg. Chem.*, **110**, 290, 1920). According to the last-named the atoms of a crystal of quartz are arranged in the form of a helix, there being nine atoms (three SiO_2 triplets) in one complete rotation, the dextro or lævo nature of the crystal depending on the type of rotation, whether right- or left-handed. This SiO_2 triplet is supposed by Sosman to maintain its individuality in the various crystalline forms of silica as well as in the amorphous forms. The differences between quartz, cristobalite, and tridymite are presumed to depend on different arrangements of the triplets, while the reversible transformations are considered to develop as the result of internal changes in the triplet whereby the relative positions of the constituent atoms undergo alteration.

The systems silica–strontium oxide and silica–barium oxide have been investigated by P. Eskola (*Amer. Jour. Sci.*, **4**, 331, 1922), who compares the results obtained with those for the corresponding system, lime–silica. Strontia forms two compounds with silica, the orthosilicate Sr_2SiO_4 and the metasilicate $SrSiO_3$. Each of these exists in only one modification, the metasilicate being isomorphous with α-calcium metasilicate. It is therefore probably monoclinic, but in its properties it appears to be dihexagonal pyramidal. Barium oxide forms four

compounds with silica—barium orthosilicate (Ba_2SiO_4), barium metasilicate ($BaSiO_3$), dibarium trisilicate ($Ba_2Si_3O_8$), and barium disilicate ($BaSi_2O_5$). While in general the binary systems of these compounds in order show simple eutectics, in one case, $Ba_2Si_3O_8$ and $BaSi_2O_5$, there is complete isomorphism, the diagram corresponding to Roozeboom's type I.

Strontium metasilicate is found to give a complete series of solid solutions with calcium metasilicate, the system corresponding to Roozeboom's type III and showing a minimum on liquidus and solidus. On the other hand, there is no isomorphism between barium metasilicate and the corresponding strontium and calcium compounds, but in the latter instance a double compound, dicalcium barium trisilicate ($Ca_2BaSi_3O_9$), is formed. No trace of compounds analogous to diopside could be found. The barium analogue of anorthite was also prepared, and it was demonstrated that the apparently orthorhombic nature of the material described by Fouqué and Michel Lévy (*Bull. Soc. Min. Fr.*, **3**, 125, 1880) was due to Carlsbad twinning. The corresponding strontium compound is very similar to anorthite.

In a paper on "The Reaction Principle in Petrogenesis," N. L. Bowen (*Jour. Geol.*, **30**, 177, 1922) concludes that eutectic relationships are relatively unimportant in igneous rocks, and that many of the structures which have hitherto been regarded as indicative of the existence of eutectics can be explained on the basis of reaction between liquid and crystal phases. These reactions are characterised as continuous in those cases where a regular variation in composition occurs during the formation of a mineral by the interaction of already formed crystals and the residual magma, as in the case of the plagioclases. Where the change in composition is not continuous, as in the ferro-magnesian series, olivine–pyroxene–hornblende–mica, the reaction is classed as discontinuous. For example, graphic intergrowths of quartz and felspar are supposed, in some cases, to arise from the reaction between already formed quartz and the remaining molten material, while an analogous explanation of intergrowths of spinel and pyroxene is suggested.

In a paper on the behaviour of inclusions in igneous magmas, N. L. Bowen (*Jour. Geol.*, **30**, 513, 1923) rejects the theory of magmatic superheating as a factor in the solution of inclusions. Instead, he regards the phenomena as explicable on the basis of the reaction principle mentioned above. The products formed during the interaction of the magmas and the inclusions depend on the position of the minerals in the latter in the reaction series with respect to the magma. Thus olivine and pyroxene in a granitic magma saturated with mica may give rise to the subsequent member of the series, biotite. This principle is applied to basic magmas as well as to acid.

Among recent papers on mineralogy the following may be noted. R. Seeliger (*Phys. Zeit.*, **22**, 563, 1921) has investigated the absorption of gases by chabasite. The formation of aluminium phosphates by the interaction of phosphate solutions and felspar is described by H. Leitmeier and H. Hellwig (*Festschrift C. Doelter*, 41, 1920), while E. Dittler (*ibid.*, 15, 1920) has described some experiments on the formation of nickel silicates. A copper aluminate allied to the spinels is described by J. A. Hedvall and J. Heuberger (*Zeit. anorg. Chem.*, **116**, 137, 1921). In a paper on the formation of smirgel, K. Walther (*Zeit. deut. geol. Ges.*, **73**, 316, 1921) mentions the occurrence of a monoclinic mineral of the composition $Al_2O_3 . H_2O$, indicating the dimorphism of this compound. W. Eitel (*Neues Jahrb. Min.*, B.B, **47**, 201, 1922) has undertaken a statistical study of the published analyses of alkaline and aluminous hornblendes, and has discussed the relationships between the amphiboles and pyroxenes.

In a further paper on the origin and constitution of the natural sulphates of iron, R. Scharizer (*Zeit. Kryst.*, **58**, 420, 1923) considers the double salt, metavoltite, to be a double sulphate of potassium and iron. The nature of the inclusions in fluorspar and their relation to the colour of the mineral is discussed by H. Steinmetz (*ibid.*, **58**, 330, 1923). C. Weigel (*ibid.*, **58**, 183, 1923) has investigated, as a sequel to certain conclusions from an earlier paper on the water in zeolites (Abs. in *Cent. Min.*, **164**, 201, 1922), the conduction of electricity in minerals of this group.

In a study of the weathering of certain rock types in South-West Africa, E. Kaiser (*Zeit. Kryst.*, **58**, 125, 1923) finds that kaolinisation is the most important process, accompanied often by local silicification through the action of the silica which is set free. There is no hydrolytic break-down of silicate as in more humid climates.

Data for the optical dispersion of three intermediate plagioclases are given by S. Tsuboi (*Miner. Mag.*, **20**, 93, 1923), who utilises the results as a method of discriminating the plagioclases (*ibid.*, **20**, 108, 1923). The optical properties of andesine have been determined by F. Becke (*Tscher. Min. Mitt.*, **35**, 31, 1921). A micrometric study of the proportions of plagioclase in a number of perthites has been carried out by P. Tschirvinsky (*Zeit. Kryst.*, **57**, 359, 1923). By combining the results with a critical examination of the chemical data relating to orthoclase and plagioclase in granitic rocks, it is concluded that the relative amounts are approximately equimolecular, thus agreeing with Vogt's diagram (*cf.* P. Niggli, *ibid.*, **57**, 178, 1923). The xenocrystic origin of epidote in certain granitic rocks is described by L. Duparc (*Bull. Soc. fr. Min.*, **46**, 21,

1922). C. S. Garnett (*Miner. Mag.*, 20, 54, 1923) finds that under laboratory conditions dolomite does not dissociate in two distinct stages, but appears to lose its carbon dioxide as a single substance.

GEOLOGY. By G. W. Tyrrell, F.G.S., A.R.C.Sc., University, Glasgow.

Earth Structure and Orogeny.—In L. Kober's book *Der Bau der Erde* (Berlin, 1921, pp. 324) we have what is probably the most significant essay on general earth structure and orogeny since Suess's *Antlitz der Erde*. It is impossible to deal adequately with Kober's great conception within the limits of one or two paragraphs. The reader is therefore referred both to the original and to two short but excellent summaries (C. R. Longwell, "Kober's Theory of Orogeny," *Bull. Geol. Soc. Amer.*, 34, 1923, pp. 231–42; O. H. T. Rishbeth, "The Structure of the Earth, A New Theory," *Discovery*, July 1923, pp. 171–5), of which Longwell's is the more technical and Rishbeth's the more popular exposition.

Stated very briefly, Kober's theory is that the history of the earth is one of continuous reaction between relatively solid continental nuclei, and narrow, relatively weak, intervening zones, which completely ring them round. The latter form the orogenetic zones, which begin as oceanic geosynclines, are later pressed up into fold-mountain systems, and by final collapse and subsidence, revert again into geosynclinal depressions, dragging down with them strips of the marginal forelands. Kober shows that the majority of the great tectonic lines are ranged concentrically round the continental blocks, the centres of which are permanent units of the earth's crust. The sites of the geosynclines and fold-mountain ranges vary from time to time, but are always marginal to the nuclei. Occasionally two or more of the blocks are welded together, as are Europe and Asia together possibly with Africa, at the present time.

Kober's theory of orogeny is expounded in the book under consideration with particular reference to the Alpine system; it is applied in great detail to the Alps in his later work, *Bau und Entstehung der Alpen* (Berlin, 1923, pp. 283). In opposition to the prevalent view of the asymmetric structure of fold-mountain ranges (following Suess), Kober advocates a bilateral symmetry. By the pressing together of the continental blocks the material of the plastic intervening orogenetic zones is squeezed out over the edges of the opposing blocks, giving rise to more or less parallel fold-mountain ranges, situated back to back, and advancing against their respective forelands. Thus the Alpine *orogen* is believed to be built of a northern member, consisting of the Betic Cordillera, Pyrenees, Alps, Carpathians,

Balkans, and Caucasus, in which the movements were directed towards a northern foreland; and a southern member, consisting of the Atlas, Apennines, Dinarides, Hellenides, Taurides, and the Iranides, in which the mountain arcs advance against the southern foreland of Africa and Arabia. Where the bilateral symmetry is most pronounced there may be a broad intermontane region (*Zwischengebirge*) between the marginal ranges, *e.g.* the Hungarian Plain between the Carpathians and the Dinarides.

Kober applies the same structural conception with considerable success to other fold-mountain belts; but in one at least, the Rocky Mountains and Sierra Nevada of North America, he has made serious errors in interpretation, as is pointed out by some of the American commentators on his theory (see Longwell, *supra cit.*).

The necessary energy for these enormous movements is derived by Kober from the contraction of the earth, which brings the relatively solid earth-nuclei crowding towards one another at intervals.

Students of American mountain structures have recently taken part in a very interesting symposium on the structure and history of mountains, and the causes of their development, which has been centred about Kober's book. (*Bull. Geol. Soc. Amer.*, **34**, No. 2, 1923.) The first paper of the symposium is C. Schuchert's Presidential Address on "Sites and Nature of the North American Geosynclines" (*ibid.*, pp. 151–230). According to Schuchert, geosynclines are more or less long, very mobile, sinking areas which originate *within* a continental block, not on its margins, but between a rising borderland and the practically neutral nucleus of the continent. Mediterraneans, on the other hand, are vastly greater fields of diastrophism with long and intricate geological histories (*e.g.* the ancient Tethys). They appear to be equivalent to the orogens of Kober. This important paper is illustrated by seventeen most instructive palæogeographic maps of North America.

Prof. W. H. Hobbs will have none of Kober's bilateral symmetry theory of mountain structure, which he describes as a reversion to the earlier ideas of Escher and Heim ("The Asiatic Arcs," *ibid.*, pp. 243–52). In this paper Prof. Hobbs further explains his views, developed recently in his book *Earth Evolution and its Facial Expression*, that folded mountain arcs are due to compressive stresses coming from the front and below the convex side of the arc, not from the rear and above, according to the current idea (see SCIENCE PROGRESS, January 1923, p. 492). In the penultimate paragraph of this review the word "accurate" should be "arcuate." (See also a review of Hobbs's book by E. Blackwelder, *Journ. Geol.*, **31**, 1923, p. 434.)

Appalachian structures are dealt with by J. B. Woodworth ("Cross-section of the Appalachians in Southern New England," *ibid.*, 253–62) and by A. Keith ("Outlines of Appalachian Structure," *ibid.*, pp. 309–80). The latter author reviews current orogenic theories, such as contraction, sub-oceanic spreading, isostasy, geosynclines, and continental creep, in a very bold and trenchant manner. Keith finally adopts the view that batholithic intrusion, actuated by gravity movements in the sub-oceanic crust, is the immediate cause of mountain building. "These [batholiths] furnished the heat and force required, were of adequate power and bulk, and accord remarkably with the varied phenomena of the [Appalachian] system, both in space and in kind [*sic*; time?]. . . . [The theory] adopts many of the great facts and processes on which other theories are based, notably those of sub-oceanic spread and isostasy, and forms a consistent and reasonable whole. . . . Its results are related to the heavy masses of the earth, and not to meridians and parallels, so that no upsetting of earth constants is required."

Rocky Mountain structures are discussed by G. R. Mansfield ("Structure of the Rocky Mountains in Idaho and Montana," *ibid.*, pp. 263–84), and by W. T. Lee ("Building of the Southern Rocky Mountains, with Notes on Isostasy by C. E. van Orstrand, and on Elastic Yielding of the Earth's Crust under a Load of Sedimentary Deposits by W. D. Lambert," *ibid.*, pp. 283–308). For the Northern Rockies Mansfield's conclusions are that "compressive stresses, originating in gravitative adjustments within the earth, and influenced to a greater or less degree by magmatic movements, were localised by the heavily loaded geosyncline, which became folded and overthrust, producing high mountains that were subsequently peneplained. Later compressive stresses, originating in a similar manner but not localised by sedimentary accumulations, caused broad upwarpings and gentle folds which gave the mountains their present elevations."

W. T. Lee, however, holds that for the Southern Rockies there is insufficient evidence of lateral thrust of magnitude great enough to account for the mountains by crustal shortening. Field evidence, especially peneplains at 12,000 feet and 10,000 feet, shows that they were formed chiefly by vertical uplift.

E. C. Andrews ("Contribution to the Hypothesis of Mountain Formation," *ibid.*, 381–400) believes that the continental masses have flowed or crept in undulations towards the larger ocean deeps, especially the Pacific and the Tethyan region. This movement has produced earth crests and troughs which tend to become mutually supporting through the

medium of a lower zone of rock flowage. The continents are becoming more stable by the welding together of the mountain ranges and the intervening troughs; the ocean basins, too, tend towards a greater permanence. The present mountain ranges are due to a general pull by shrinkage of the sub-oceanic segments of the earth's crust.

H. Stille's address entitled "Die Schrumpfung der Erde" has been summarised by C. R. Longwell (*Amer. Jour. Sci.*, **6**, 1923, pp. 168–72). He emphasises the rhythm of diastrophism, long regular periods of epeirogeny alternating with short spasmodic episodes of orogeny. Stille finds the energy for these processes in the shrinkage of the earth; and reaches a conception, very similar to Kober's, of rigid plates of the earth's crust adjusted to contraction by the buckling of intervening weak zones, but without sensible deformation of the plates themselves.

In a recent pamphlet Mordziol (*Die Gebirgsbildung der Erde*, Leipzig, 1922, pp. 30), according to a note in the *Geological Magazine* (August 1923, p. 373), adopts a theory of mountain building which appears to be akin to Keith's (*supra cit.*). He attributes orogeny largely to tangential thrusts produced by liquation and differentiation processes in magmas.

In a paper on "The Earth's Crust and its Stability," Prof. R. A. Daly (*Amer. Jour. Sci.*, **5**, 1923, 347–71) shows that the pressures down to a depth of 100 km. have a practically negligible effect on the m.p. of diabase; and as the temperature at 40 km. depth is probably more than 1,200° C., the basaltic sub-stratum cannot be holocrystalline. Hence he conceives of a holocrystalline crust of definite thickness (40 km. in Europe) resting on a sub-stratum of hot basaltic glass. This view incidentally lessens the difficulties attending the idea of isostatic underflow in response to crustal loading. It also leads Prof. Daly to favour the Taylor-Wegener hypothesis of continental drift, with certain modifications. Various causes are considered which may produce de-levelling and large-scale distortion of the earth's crust, whereby sliding slopes may be generated for the lateral movement of thin crustal plates over the elastico-viscous sub-crust.

In his brilliant study of the causes of the surface movements of the earth's crust (*Phil. Mag.*, June 1923, pp. 1167–88; *Nature*, May 5, 1923, pp. 603–6) Prof. J. Joly has made a most outstanding contribution to the theory of epeirogenic and orogenic movements. He accepts a basaltic sub-crust, which he believes to be periodically remelted by the accumulation of heat generated by the radio-active substances always present in basalt. This heat becomes sufficient to melt solid basalt already near its m.p. in about 25,000,000 years. At the climax of this energisation of the basaltic sub-stratum the lighter

continents will tend to sink deeper, with resulting oceanic transgressions over their borders. On the ensuing dissipation of heat and the re-solidification of the sub-crust the continents will again rise. Thus is the major periodic epeirogeny of the earth explained.

Orogeny is ascribed to intensified tidal phenomena in the basaltic sub-crust at energised periods. The tidal retardation of the thin solid crust upon the sub-stratum results in a westward lag of the continents, which tend then to yield along geosynclinal zones of weakness. The crushed geosynclinal sediments are forced both up and down in the crust ; the rising parts constitute the mountain chains, and the subsiding parts provide the isostatic compensation for the mountain uplift. East and west orogens like the Alpine-Himalayan system are explained as due to lateral movements of the continental blocks under the influence of tidal retardation ; in this case, a pivotal movement of Africa against Euro-Asia.

In a suggestive paper on " The Existence and Configuration of Pre-Cambrian Continents " (*Bull. N.Y. State Mus.*, **239, 240**, 1922, pp. 67–152) R. Ruedemann has endeavoured to show " that the trend-lines of the folds of the Pre-Cambrian rocks, together with their foliation, schistosity, and the longitudinal direction of the batholiths, form a complex of phenomena that are causally connected, and that exhibit uniform directions over immense tracts of the earth. There are three such tracts of super-continental size which are termed Archi-America, Archi-Eurasia, and Archi-Gondwana. The same tracts appear as continents in early Cambrian times, and persist, more or less fractured, as fundamental units of the surface of the earth throughout geological time."

In a later paper on " Fundamental Lines of North American Geological Structure " (*Amer. Jour. Sci.*, **6**, 1923, 1–10) the same author attempts to show " that the original grain of the Pre-Cambrian foundation of the continent reappears in the main direction of the epicontinental seas, principally of the Palæozoic Era, in the present general strike of the rock formations, and in the major physiographic features of the continent, notably its general outline, the mountain systems, the principal river courses, and the major axes of some of the Great Lakes." These lines are arranged more or less concentrically around the North American continent (*cf.* Kober).

In a paper to the Royal Geographical Society (*Geogr. Jour.*, **[illegible]**, 1923, pp. 20–32) Prof. J. W. Gregory marshals the evidence to show that the geographical lines of the Banda arc are not due to folding in the continuation of the Alpine-Himalayan orogen, but to arcuate subsidences, and to horsts whose

boundaries cut across the main E.-W. fold lines. The geological affinities of the islands are with New Guinea.

F E. Suess gives a brief comparison between Alpine and the earlier Variscian structures (*Geol. Rundschau*, **14**, 1922, p. 2).

Stratigraphy and Regional Geology.—Regional geological study in this country is furthered by the publication of several new Geological Survey memoirs (Geol. of Corrour and the Moor of Rannoch, Expl. of Sh. 54, *Mem. Geol. Surv. Scotland*, 1923, pp. 96; Geol. of Lower Findhorn and Lower Strathnairn, Expl. of Sh. 84 and part of 94, *Mem. Geol. Surv. Scotland*, 1923, pp. 128; Geology of Liverpool, Expl. of Sh. 96, *Mem. Geol. Surv. England and Wales*, 1923, pp. 178; The Concealed Mesozoic Rocks in Kent, by G. W. Lamplugh, F. L. Kitchin, and J. Pringle, *Mem. Geol. Surv. England and Wales*, 1923, pp. 244), and by the extremely useful long excursion guides which are published from time to time by the London Geological Association. Amongst the latter we notice two papers in which the geology of East Anglian centres is summarised (P. G. H. Boswell and I. S. Double, "The Geology of the Country around Felixstowe and Ipswich," *Proc. Geol. Assoc.*, **33**, 1922, pp. 285–305; P. G. H. Boswell, "The Geology of the Country around Cromer and Norwich," *ibid.*, **34**, 1923, pp. 207–22). In the same series Dr. J. W. Evans describes the geological structure of the country around Combe Martin, North Devon (*ibid.*, **33**, 1922, 201–27); and Dr. R. L. Sherlock deals with the geology of Lydford and Brent Tor, Devon (*ibid.*, **34**, 1923, pp. 21–32).

In an important paper on "The Base of the Devonian" (*Geol. Mag.*, **60**, 1923, 276–82; 331–6; 367–72; 385–410) Dr. L. D. Stamp puts forward the thesis that the Ludlow Bone Bed is the true base of that formation. It marks a considerable change in physical conditions, which is noticeable elsewhere as an unconformity, and there is also a marked faunal change at this horizon. Physical breaks or unconformities occur in Scotland, Belgium, Brittany, Scandinavia, and Spitsbergen, on or near the same horizon.

In a paper on the Liassic Rocks of Glamorgan, Dr. A. E. Trueman (*Proc. Geol. Assoc.*, **33**, 1922, pp. 245–84) shows that they have been laid down on an irregular floor of Carboniferous Limestone and Old Red Sandstone. The Pre-Liassic landscape is being gradually exposed in South Wales by the removal of the softer Mesozoic rocks. Interesting littoral deposits occur within the formation just off the old shore-lines.

Another Mesozoic section is described by L. Richardson in his paper on "Certain Jurassic (Aalenian-Vesulian) Strata of Southern Northamptonshire" (*Proc. Geol. Assoc.*, **34**, 1923, pp. 97-113).

In discussing the minor structures of the London Basin, S. W. Wooldridge (*Proc. Geol. Assoc.*, **34**, 1923, 175–92) shows that they present a well-marked plan governed by N.E.–S.W., and N.W.–S.E. fold and fault lines, and by a few N.–S. features. This plan is ascribed to the wrinkling of the Cretaceo-Tertiary cover resting on the London Palæozoic platform, due to renewed movement along the dominant tectonic lines known to affect the Palæozoic rocks in their nearest exposures.

Lauge Koch has now published his new geological data upon ice-free Peary Land, in the extreme north of Greenland (*Amer. Journ. Sci.*, **5**, 1923, 189–99); and in another paper has given a general account of new features in the physiography and geology of Greenland (*Journ. Geol.*, **31**, 1923, 42–65). The whole of Peary Land and the adjacent Mylius Erichsen's Land belong geologically to a great area of Palæozoic transgression comprising almost the whole of North-west Greenland and have been affected by folding which is a continuation of the great Caledonian system of Spitsbergen, Scandinavia, and Scotland. From his physiographic studies Koch shows that a great depression crosses Greenland from Disco Bay to Scoresby Sound. The extremely important observation is made that the plateau basalt regions of Greenland are situated at the western and eastern ends of this depression, which is directly in line with the extension of the submarine basaltic plateau which connects Greenland through Iceland and the Faröes to the Inner Hebrides and Antrim. The theory that the depression is simply a direct continuation of the great Tertiary fracture-zone of the North Atlantic is adopted.

Sedimentary Rocks and Structures.—In his Presidential Address to the Liverpool Geological Society Prof. P. G. H. Boswell (*Proc. Liverpool Geol. Soc.*, **13**, pt. 4, 1923, pp. 231–303) gives a most valuable summary of the history of the petrological examination of sedimentary rocks from De Réaumur (1718) to the present time. He deals also with such topics as the mineral assemblages in sediments, and their aid in elucidating stratigraphical and economic problems; the stability of minerals, and the prevalence of the stress-minerals (Harker) in sediments. A very full bibliography is appended, and there is an appreciation, to be expanded in a later work, of some little-known continental work on sediments, especially Italian.

In his paper on the petrography of the Cretaceous and Tertiary outliers of the West of England, Prof. Boswell (*Quart. Journ. Geol. Soc.*, **79**, pt. 2, 1923, pp. 205–30) shows that the materials of these formations are, on the whole, of similar character. Their mineral assemblages differ from those of the Permian, Trias, and Jurassic of the West of England in that they contain more material derived from British and less from

Armorican rocks. Tourmaline, of all shapes, sizes, and colours, is the most abundant mineral. It is obviously derived from the West-Country granites and their aureoles. For some unknown reason garnet is very rare or absent in these deposits, despite its abundance in the igneous and metamorphic rocks of Devon, Cornwall, and Brittany, and its abundance in the Permian and later sediments of the West of England.

H. G. Turner and H. R. Randall, in investigating the microscopy of anthracite, have used a method of preparing specimens which, judged by the beautiful microphotographs they publish, has been extraordinarily successful (*Journ. Geol.*, **31**, 1923, 306–13). Small surfaces of anthracite are highly polished, heated in a drying oven at 220° C. to obviate splitting, and are then etched by momentarily applying the oxidising blowpipe flame. This results in differential oxidation of the various laminæ and structures. The results show that anthracite possesses all the essential characters and the same types of organisms as bituminous coals.

Messrs. J. and J. R. Lomax have now fully explained the technique of the methods they employ to prepare thin transparent sections of coal 8 in. by 4 in. in area, and also relatively large sections of fragile coals (*Bull.* XIV, Lancs. and Cheshire Coal Research Assoc., 1923, pp. 24).

In a study of stalactites and stalagmites carried on at the Experimental Mine of the U.S. Bureau of Mines, where these growths are forming under known conditions, V. C. Allison (*Journ. Geol.*, **31**, 1923, 106–25) has developed an approximation method for determining their ages. This is expected to prove useful for evaluating recent geological and archæological periods.

E. M. Kindle continues his well-known line of research in writing on the nature of mudcracks and ripple-marks of the recent calcareous sediments of the Bahama-Florida region (*Journ. Geol.*, **31**, 1923, 138–45).

PLANT PHYSIOLOGY. By Cyril West, B.A., D.Sc., Low Temperature Research Station, Cambridge (Plant Physiology Committee).

The available data on the behaviour of plants and plant tissues when exposed to frosts or low temperatures have for the most part been obtained by empirical methods, with the result that our knowledge of the actual physiological processes involved is extremely meagre. From the following brief summary of some recently published work it will be seen that in this field of research many interesting problems await solution by the plant physiologist.

The earlier literature bearing upon this subject is summarised by R. B, Harvey (" Hardening Process in Plants and Develop-

ments from Frost Injury," *Ph. D. Diss. Univ. Chicago*, October 1918), who points out that in the resistance of plant tissues to frosts or low temperatures the relative importance of such factors as undercooling of the tissues, mechanical injury of the cell by ice-formation, the actual freezing-point of the cell sap, and the precipitation of proteins through salting-out, has been in the past the subject of much dispute. Harvey describes in detail the results of frost injury to various succulent plants. One of the first indications of frost injury is the appearance of injected areas on the leaves due to the withdrawal of water from the cells, and the displacement by this water of the air in the intercellular spaces. In some plants the frozen cells are stimulated to abnormal growth, tumour-like outgrowths being produced similar in many respects to those produced in the plant by pathogenic organisms. In the case of the cabbage (*Brassica oleracea*) and tomato (*Lycopersicum esculentum*) he compares the resistance to low temperatures and to freezing of untreated plants and plants which have been " hardened " by exposure for a week or so to temperatures somewhat above the freezing-point. Cabbage plants which had been kept at 3° C. for five days withstood exposure for half an hour to — 3° C., whereas the control plants were killed. Harvey believes that the main effect of the hardening process is to be attributed to a change in the proteins of the protoplasm, which prevents their precipitation as a result of the physical changes incident upon freezing. An increase in the amino-acid content of the hardened plants takes place, whereas the carbohydrate changes are slight. In this connection changes in the sugar content causing increased depression of the freezing-point, or the nature of the surface of the epidermal cells, which may affect undercooling of the tissue by the prevention of inoculation from ice formed on the surface (R. B. Harvey, " Importance of Epidermal Coverings," *Bot. Gaz.*, **67**, 1919, 441), appear to be relatively unimportant. In a later communication (R. B. Harvey, " Varietal Differences in the Resistance of Cabbage and Lettuce to Low Temperatures," *Ecology*, **3**, 1922, 134) similar results are given for lettuce.

G. M. Tuttle (" Induced Changes in Reserve Materials in Evergreen Herbaceous Leaves," *Ann. Bot.*, **33**, 1919, 201) and F. J. Lewis and G. M. Tuttle (" Osmotic Properties of Some Plant Cells at Low Temperatures," *Ann. Bot.*, **34**, 1920, 405) measured periodically from autumn to summer the osmotic pressures, electrical conductivities, proportions of electrolytes and non-electrolytes, and the amounts of sucrose, maltose, and glucose in the leaf tissues of *Picea canadensis*, *Linnæa borealis*, and other plants, and in the cortical tissue of *Populus tremuloides*. They found no definite correlation between these values and the

daily or weekly fluctuations of air temperature; the sugars, however, showed a decided concentration during the winter months and a progressive decrease from the winter maximum towards the summer.

In addition they examined the microscopic structure of the mesophyll cells of *Picea* when subjected to the severe winter conditions obtaining in North-west Canada. The identity of the individual chloroplasts, which appeared as diffused masses, bright yellowish-green in colour, closely associated with the nucleus, was completely lost. A large vacuole filled with oil or fat occupied the greater part of the cell. All traces of starch had disappeared during the autumn. The replacement by oil of the starch in the cell contents during the autumn months seems to be characteristic of most of the evergreen plants which occur in North-west Canada. The authors make the interesting statement that ice does not begin to form in living cells of *Pyrola* until a temperature below − 31° C. is reached.

During the last few years the problem of hardiness in fruit trees, as indicated by their ability to withstand low temperatures, has received much attention. As a convenient physical measurement of hardiness of fruit buds, E. S. Johnston (" An Index of Hardiness in Fruit Buds," *Amer. Jour. Bot.*, **6**, 1919, 373; also " Moisture Relations of Peach Buds during Winter and Spring," *Univ. of Maryland Agric. Expt. Stat. Bull.*, **255**, June 1923) has suggested that the ratio of water content to dry-weight might serve as a possible index. The same author (" Undercooling of Peach Buds," *Amer. Jour. Bot.*, **9**, 1922, 93), using a thermo-electrical method, has obtained data during a period of several weeks (January to March) showing the average temperature at which crystallisation began (= undercooling) and the average temperature immediately after crystal formation (= freezing-point), together with the ratio of water content to dry-weight of fruit buds of two varieties of peach grown in the same orchard. The results obtained indicate a decrease in the hardiness of the fruit buds with the approach of spring. Johnston also presents data which show that wet buds freeze at a considerably higher temperature than dry buds, thus confirming the results previously obtained by West and Edlefsen (" Orchard Heating," *Utah Agr. Coll. Expt. Sta. Bull.*, **161**, 1917). This result is of practical significance in that it indicates that a cold period following rain is especially dangerous to fruit trees.

F. L. West and N. E. Edlefsen (" Freezing of Fruit Buds," *Jour. Agric. Res.*, **20**, 1921, 255) present the results of freezing experiments with fruit buds of the apple, peach, cherry, and apricot extending over a period of seven years, and also a record of the frosts occurring in orchards near Logan, Utah,

during the same period. They find that the same buds show different degrees of hardiness at different stages of development, these differences being attributed to changes in the quality and concentration of the cell sap. The safety limit for the buds when in full bloom appears to be about 29° F.

As the result of an extensive study of hardiness of plum trees, M. J. Dorsey and P. D. Strausbaugh (" Plum Investigations : I, Winter Injury to Plum during Dormancy," *Bot. Gaz.*, **86**, 1923, 113) conclude that many factors are operating, and that different factors may be involved in different localities and with different species. Even in the same plant different tissues respond differently. According to these botanists a sharp distinction can be drawn between the fruit buds and the flower buds, as indicated by micro-chemical tests. When fruit buds were dissected and examined microscopically at a temperature of −14° F. it was found that, whereas the spaces between the bud scales were packed with ice crystals, causing the bud as a whole to appear swollen and distended, ice crystals were entirely absent from the bud space enclosed by the scales above and around the flower buds.

H. D. Hooker (" Pentosan Content in Relation to Winter Hardiness," *Proc. Amer. Soc. Hort. Sci.*, 1920, p. 204) and J. T. Rosa (" Pentosan Content in Relation to Hardiness of Vegetable Plants," *Proc. Amer. Soc. Hort. Sci.*, 1920, p. 207), working respectively with fruit trees and vegetables (*e.g.* cabbage, lettuce, celery, tomato, etc.), set out to discover whether any correlation existed in these plants between winter hardiness and pentosan content. Their data show a marked positive correlation between the pentosan content of all the plants investigated and their resistance to low temperatures. The hypothesis is put forward that some specific pentosan functions in the plant tissue by holding water in the form of "absorbed" or colloidal water, and that such " absorbed " water is not frozen upon exposure to moderate freezing temperatures.

No doubt owing to the fact that the potato crop often suffers severe loss on account of frost injury, the behaviour of the potato in relation to low temperatures has recently received the attention of several agriculturists in the United States. L. R. Jones, M. Miller, and E. Bailey (" Frost Necros of Potato Tubers," *University of Wisconsin Agric. Expt. Sta. Research Bull.*, **46**, October 1919) state that when potato tubers are frozen solid they are killed and collapse on thawing ; if the treatment is less severe, the tubers may appear perfectly sound externally, although when cut open they exhibit signs of internal frost necrosis. The vascular tissues are more sensitive to frost injury than the parenchymatous ground tissue, discoloration of the vascular network being usually the most

obvious change in the appearance of the tuber. The conditions necessary to produce frost necrosis are shown to be exposure to $-10°$ C. for one hour, to $-5°$ C. for two hours, or to $-3°$ C. for several hours. The actual freezing-point of the potato lies in the region of $-1°$ C., but owing to the phenomenon of supercooling the living tuber will often withstand long exposure to $-3°$ C. without injury. The supercooling range appears to be dependent upon the air temperature and also upon the rate at which this temperature is dropped. Thus, at $-3{\cdot}5°$ C. the supercooling point approaches the air temperature. But if the air temperature is dropped *slowly* to $-5°$ C. or below, it will approach $-5°$ C., whilst if dropped *rapidly* to the same point, it will be much higher, namely, about $-3°$ C. The authors make the additional interesting point, that the sprouts arising from the tuber are more resistant to low temperatures than the tubers themselves.

This question has been pursued farther by R. C. Wright and R. B. Harvey ("The Freezing-point of Potatoes as determined by the Thermo-electric Method," *Bull. No.* 895, *U.S. Dept. Agric.*, May 1921) and by R. C. Wright and C. F. Taylor ("Freezing Injury of Potatoes when Undercooled," *Bull. No.* 916, *U.S. Dept. Agric.*, June 1921), who by means of a thermo-electric method have determined the freezing-point of eighteen standard varieties of potato. They have also shown (1) that the freezing-point tends to rise as the season advances; (2) that the freezing-point varies with the variety of potato tested; (3) that in general early and mid-season varieties have a higher freezing-point than late varieties; (4) that undercooling can be terminated at any point by inoculation; (5) that the tubers freeze more quickly when exposed to a rapidly falling temperature than when the temperature falls slowly, thus corroborating the findings of Jones and his co-workers; and (6) that potatoes can be undercooled several degrees below their true freezing-point and then warmed again above the freezing-point without injury, provided no ice-formation takes place within the tissue. Wright and Taylor have extended their studies of the freezing-point to various fruits, vegetables, and cut flowers; their results are presented in tabular form in *Bull. No.* 1133 *of the U.S. Dept. Agric.*, published in February 1923.

In the course of an address delivered before the National Academy of Sciences and printed in vol. 20 of the *Journal of Agricultural Research* for 1920, 9, 151, F. V. Colville endeavours to disprove the generally accepted view that the dormant condition exhibited by most trees and shrubs in regions with a cold winter is brought about by low temperatures. He points out that trees and shrubs in cold climates may

become dormant at the end of the growing season without exposure to cold weather, and when maintained at a moderately high temperature during the winter months, resume growth in the spring much later than those which have been subjected for a given period to low temperatures. Colville advances the theory that the stimulating effect produced on dormant plants by low temperature is intimately associated with the transformation of stored starch into sugar, and that this change is brought about by the weakening of the cell-membranes, which become permeable to amylolytic enzymes, which can then act upon the starch grains stored in the cell. In support of this hypothesis experimental data are presented to show that growth may be stimulated in the absence of chilling by any process which will tend to cause local injury of the tissues. Pruning, girdling, and notching may provide the necessary traumatic stimulus.

The recent appearance in the pages of this Journal of a series of articles by I. Jørgensen and W. Stiles dealing with the scientific aspects of the preservation of fruit by cold storage (" Some Scientific Aspects of Cold Storage," Science Progress, **13**, 1919, 514 ; also **14**, 1919, 98) renders it unnecessary to deal here in detail with this subject.

In order to ascertain what are the factors underlying the retardation of ripening and the temporary delay of autolytic decomposition of plant tissues at low temperatures, physiological work on cold-stored fruit and vegetables has consisted mainly of periodic chemical analysis of the cell contents ; and since the disintegration of carbohydrates and acids as a result of respiration and other metabolic activities involves the production of carbon dioxide, determinations of the carbon-dioxide output and oxygen uptake, and of the internal atmosphere of the plant tissues at low temperatures, have been made. The results obtained have generally been compared with those found at ordinary temperatures.

It should be borne in mind, however, that the success or failure of fruit and vegetable cold storage depends not only on the lengthening of their storage life, but also upon the maintenance of such conditions as will allow the various physical and chemical changes which are usually spoken of as " life processes " to proceed without interruption. The available literature provides many indications that at low temperatures the normal metabolic or enzymatic processes of plant tissues may be profoundly altered or easily thrown out of gear.

Under the first category one may quote the remarkable fact brought out by Gassner's work on several winter varieties of cereals (" Beiträge zur physiologischen Charakteristik sommer- und winter-annueller Gewächse, inbesondere der Getreidepflanzen," *Zeitschr. für Bot.*, **10**, 1918, 417), in which

it is shown that the time of culm-formation may be determined by the conditions to which the seed grain has been subjected during or immediately following germination. By exposing seeds of winter rye sown in the spring to a temperature of 0° to −5° C. for twenty-five days, Gassner succeeded in obtaining plants with fully developed flowering stems the same year after as brief a growing-period as sixty-eight days. Plants developed from similar seeds not treated in this way produced flowering stems the following year.

Under the second category mention may be made of the marked deleterious effect upon such seeds as beans and peas of soaking them in water at low temperatures (F. Kidd and C. West, " The Influence of Temperature on the Soaking of Seeds," *New Phytologist*, **17**, 1919, 35), and of the so-called physiological or functional disease of cold stored apples which is known as " internal browning " (W. S. Ballard, J. R. Magness, and L. A. Hawkins, " Internal Browning of the Yellow Newtown Apple," *Bull. No.* 1104, *U.S. Dept. Agric.*, October 1922). Again, at low temperatures apples have been shown to be more susceptible to brown heart, another functional disease, than at ordinary temperatures (F. Kidd and C. West, " Brown Heart—a Functional Disease of Apples and Pears," *Special Rpt. No.* 12, *Food Invest. Board*, 1923).

For an account of other recent work dealing with the changes occurring in various fruits and vegetables when kept at low temperatures, reference should be made to the following articles : L. A. Hawkins and J. R. Magness, " Some Changes in Florida Grapefruit in Storage," *Jour. Agric. Res.*, **20**, 1920, 357 ; J. R. Magness, " Investigations in the Ripening and Storage of Bartlett Pears," *Jour. Agric. Res.*, **19**, 1920, 473 ; L. A. Hawkins, " A Physiological Study of Grapefruit Ripening and Storage," *Jour. Agric. Res.*, **22**, 1921, 263 ; L. A. Hawkins, " The Effect of Low-temperature Storage and Freezing on Fruits and Vegetables," *Amer. Jour. Bot.*, **9**, 1922, 551 ; and E. E. Thomas, H. D. Young, and C. O. Smith, " A Study of the Effects of Freezes on *Citrus* in California : II, Changes that take place in Frozen Oranges and Lemons," *Univ. California Agric. Expt. Stat., Bull.*, **304**, 1919, 299.

ENTOMOLOGY.

By A. D. Imms, D.Sc., Institute of Plant Pathology, Rothamsted Experimental Station, Harpenden.

Owing to the great number of entomological works that have appeared during the last nine months, it has only been possible to include a selection of those which have come to hand.

General Entomology.—H. M. Lefroy has written a textbook entitled *Manual of Entomology* (Edward Arnold & Co., 35*s*. net),

which is a systematic treatment of the various groups of insects, with special reference to the needs of those interested in the applied aspect of the subject. We welcome the appearance of several new parts of Schröder's *Handbuch der Entomologie*, which fully maintain the excellence of their predecessors. G. C. Crampton continues his morphological studies with unabated enthusiasm. In *Can. Ent.*, **55**, 126–32, he deals with the sclerites and other parts of the insect-leg and discusses their terminology; in *Jour. New York Ent. Soc.*, **31**, 77–107, he has a larger contribution on the maxillæ throughout the Insecta, and has figured a very large number of representative types. The rôle of the taxonomist in entomology is the subject of an interesting address by A. B. Gahan (*Proc. Ent. Soc. Washington*, **25**, 69–75), who concludes with a quotation from Pearl as follows: "It is the systematist who has furnished the bricks with which the whole structure of biological knowledge has been reared. Without his labours the fact of organic evolution could scarcely have been perceived, and it is he who to-day really sets the basic problems for the geneticist and the student of experimental evolution." A. D. MacGillivray, in an article entitled "Landmarks in Insect Morphology" (*Ann. Ent. Soc. Am.*, **16**, 77–84), makes some suggestive remarks as to the utilisation of definite morphological features when attempting to identify the various sclerites. The article is a pertinent one, for it is only by exact methods such as he advocates that progress is likely to ensue. We regret, however, that he sees fit to introduce so many new terms, and rather doubt whether all of them are necessary. A translation of an important paper on the wing-venation of insects, by A. Lameere, appears in *Psyche* (**30**, 123–30). R. A. Wardle and P. Buckle have recently brought out a textbook on *The Principles of Insect Control* (Manchester University Press), which is evidently a very clear and thorough exposition of the subject.

Orthoptera.—N. B. Tindale (*Rec. S. Australian Mus.*, **2**, 425–55) deals with the Mantidæ, or praying insects, of Australia, a family which is evidently abundantly represented in that country. The known Australian species now number 76, including 4 genera and 16 species added by Mr. Tindale. One of the most interesting forms is *Bolbe maia* sp. nov., which is the smallest known mantis and attains a length of only 8 mm. Tindale mentions that it came freely to light in a camp and was so active that it was very difficult to capture; it often seized flies and other insects which had been likewise attracted. Another new mantid, *Parhierodula majuscula*, is probably the largest Australian member of the family, and the female measures 95 mm. long, while the outspread tegmina have an expanse of 113 mm. A third species, *Orthodera ministralis*

Fab., seems to occur in all parts of the continent as well as in Tasmania. It exhibits a wide range of variation which has resulted in an extensive synonymy; notwithstanding its wide distribution, it does not appear to have developed any clearly defined local races, unless the Tasmanian form is to be regarded as coming under that category. In *Bull. Entom. Res.*, **14**, 31–9, B. P. Uvarov discusses the habits of the swarming locust *Schistocera gregaria* (*peregrina*), which is the only Old World representative of the genus. The locust *Acridium flaviventre* Burm. is regarded as no longer a distinct species, but as the solitary phase of the dimorphic species *S. gregaria*. Uvarov agrees with the conclusions of Vosseler that the migration of *S. gregaria*, either as nymphs or adults, has nothing to do with need for food or with the search for new breeding-grounds, and a solution of the phenomenon is not yet forthcoming. Kunckel d'Herculais has observed, and Vosseler has studied more thoroughly, the extremely interesting colour changes in the individuals forming migratory swarms. These changes in *S. gregaria* are very pronounced, and Uvarov believes that they are in direct physiological connection with the maturation of the sexual products, and of the development and reduction of the fat-body. The life-cycle of this species is very poorly known, and its permanent breeding-grounds and the conditions under which breeding takes place are greatly in need of study. In *Trans. Ent. Soc.*, 110–69, M. Burr, B. P. Campbell, and B. P. Uvarov have a paper on the Orthoptera of Macedonia, enumerating 108 species which they consider as representing the greater number of those inhabiting that country. Their paper includes 3 Forficulids, 8 Blattids, 5 Mantids, 14 Gryllids, 39 Tettigoniids, and 39 Acridids. In their analysis of the Orthoptera they conclude that the fáuna of these insects differs profoundly from that of the rest of the Balkan peninsula, and closely resembles that of Western Anatolia. A record of considerable interest is the finding of *Grylloblatta* in California (A. N. Caudell, *Can. Ent.*, **55**, 148–59), this remarkable insect only being previously known from Alberta. E. Bugnion (*Mem. Soc. Vaud.*, **5**, 177–243) has an illustrated paper on the ootheca, method of eclosion of the nymph, and the structure of the ovipositor in *Mantis*, *Empusa*, and *Gongylus*. E. B. Fulton (*Jour. Econ. Ent.*, **16**, 369–76) has conducted experiments in order to discover a good poison-bait for the common European earwig. The most effective poison is sodium fluoride added to a suitable bait such as wheat bran sweetened with molasses.

Coleoptera.—In *Records Australian Mus.*, **14**, 353–96, A. M. Lea treats of the dung beetles of the sub-family Coprides, but in comparison with other parts of the world Australia is poor in indigenous species of these insects. This, however, is scarcely

surprising considering the dearth of large indigenous mammals. Dung beetles of several kinds have multiplied with the distribution of domestic animals, and many European species have been introduced. Several of the genera are of exceptional interest, especially *Macropocopris,* species of which live in fur about the anal region of Marsupials, and have developed very powerful claws: one species, *M. symbioticus,* has been found in the cloaca of a wallaby. T. G. Sloane (*Trans. Ent. Soc.*, 234–50) has a new classification of the great family Carabidæ, which he separates primarily into six main divisions based upon characters afforded by the coxal cavities. The metamorphoses of the Staphylinid *Aleochara algarum,* which parasitises puparia of *Fucellia maritima* and other shore-inhabiting Diptera, are described by P. Lesne and L. Mercier (*Ann. Soc. Ent. Fr.*, **91**, 351–7). The dispersion of the Japanese beetle *Popilla japonica* in America is discussed by C. H. Hadley and L. B. Smith (*Jour. Econ. Ent.*, **16**, 349–53). It appears that it occurred over less than one square mile in 1916, when it was first discovered in New Jersey. At the end of 1922 it extended over 773 square miles of New Jersey and Pennsylvania.

Lepidoptera.—C. B. Williams (*Trans. Ent. Soc.*, 207–33) discusses the subject of insect migration very fully, and points out how little we know concerning the factors which determine a species to migrate and the course taken during migration. It appears probable that some innate tendency to migrate must be present in the species concerned, but this will require a suitable concurrence of external conditions in order to develop Many species exhibit a tendency to go in a certain direction, but it is impossible to say at present what determines it. R. J. Tillyard (*Trans. Ent. Soc.*, 181–306) has an important paper on the mouth-parts of the Micropterygoidea, which include the most primitive of all moths, and concludes that the Lepidoptera are not derived from the Trichoptera, but both orders are descended from a common ancestral stem. G. Grandi (*Boll. Lab. Zool. Portici*, **17**, 3–40) continues his elaborate study of the post-embryonic development of the silkworm. The external morphology of the larval instars of the race " Treotti " is fully described by him and a key to them is provided.

Diptera.—T. D. A. Cockerell (*Ent. News*, **34**, 29) records the Leptid *Symphoromyia* being present in large numbers in Colorado and inflicting painful punctures with their mouth-parts. R. C. Shannon (*Proc. Ent. Soc. Washington*, **25**, 103–4) contributes a note on rearing certain larval Diptera on nutrient agar. W. R. Thompson (*ibid.*, 33–44) writes on *Masicera senilis,* a parasite of the European corn-borer. The larva is one of those Tachinids which attach themselves to a trachea of the host by its posterior extremity. It becomes enclosed by a sheath

derived from the tracheal wall, within which it remains until the third instar. When it has arrived at the latter condition, it leaves the tracheal sheath and devours the viscera of its host, until only the empty skin remains. After it has finished feeding, it usually emerges from the caterpillar's skin, and pupates. The life-cycle of *Miastor* is discussed by R. G. Harris (*Psyche*, **30**, 95–101), who points out that the non-pædogenetic larvæ (*i.e.* those which pupate) are distinguished from pædogenetic larvæ by the presence of imaginal buds, the sternal spatula, and in separation of the eyes. Among the pædogenetic larvæ two types are recognised—the white and the yellow, which exhibit also certain correlated differences.

Hymenoptera.—A. S. Buckhurst, L. N. Staniland, and E. B. Watson have written a short handbook on British Hymenoptera (Edward Arnold & Co., 9*s.* net), illustrated with text-figures and photographic plates. A. D. Betts (*Practical Bee Anatomy*, The Apis Club, Benson, Oxon) has produced an admirable up-to-date guide to the anatomy of the bee which will commend itself to all interested in the structure of insects. C. Morley (*Ent. Month. Mag.*, **59**, October, 228–32) has a further contribution of his synopsis of the British Proctotrypidæ. The structure of the head and mouth-parts of the hornet, along with their musculature, form the subject of a detailed paper by Brocher (*Bull. Inst. Nat. Genevois*, **45**, 36 pp.). The life-history of the Braconid, *Aphidius avenæ*, which parasitises the nettle aphid, is described by E. I. MacGill (*Proc. Roy. Soc. Edin.*, **43**, 51–71). Its primary larva is provided with a caudal prolongation which is also visible in the second instar; the tracheal system is closed until the last instar, and pupation takes place within the skin of the host. This parasite appears to be most active in the early part of the year and is an important check on the increase of the aphid at that period, *i.e.* when the plants are young. As many as 83 per cent. of the aphids examined in April were found to support this Braconid: the parasitism went down to 44 per cent. in May, to 37 per cent. in June, while in July it was also very low, and from August to January it did not exceed 33 per cent. in any one month. A more extended study of Braconids parasitic on Aphididæ will be found in *Ann. Ent. Soc. Am.*, **16**, 1–29, by E. W. Wheeler. H. Yuasa has brought out in *Illinois Biol., Monograph* 7, No. 4, the most important contribution yet made on the larvæ or saw-flies and their allies. More than 2,500 specimens of larvæ, representing at least 400 species, have been examined and a good many species have been bred. The memoir contains a wealth of morphological and other information, and concludes with a classification of the Tenthredinoidea, based largely upon features afforded by the larval legs. The poison of ants forms the subject of an investi-

gation by R. Stumper (*Ann. Sci. Nat. Zool.*, **10**, 1922, 105–12). Formic acid appears to be constantly present in the Camponotinæ, while in the Myrmecinæ and Dolichoderniæ it is either wanting or negligible. The poison in the two latter groups is undetermined, but may be of an albuminoid nature. The biology of the Scelionid *Rielia manticida* Kieff is discussed in an interesting paper by L. Chopard (*Ann. Soc. Ent. Fr.*, **91**, 249–72). This species, along with the Chalcid *Podagrion pachymerum*, which is also discussed, are parasites of the common mantis in France.

Hemiptera.—F. Muir (*Proc. Hawaiian Ent. Soc.*, **5**, 205–47) has an important paper on the classification of the superfamily Fulgoroidea, among which he recognises 15 families. The author also briefly discusses the characters separating the Heteroptera from the Homoptera, and points out that the usual statement that the " beak " arises from the front or back of the head is incorrect ; similarly the tegminal characters do not always hold good. In the Heteroptera there is a well-marked gula, the head projects forward, and the proboscis is bent at its base and lies under the head when at rest. In the Homoptera the gula is absent or membranous, the head is deflexed and inflexed so that the base of the labium is in intimate connection with the prosternum. The Australian water-bugs of the family Notonectidæ form the subject of a contribution by H. M. Hame (*Rec. S. Australian Mus.*, **2**, 397). The predominant genus is *Anisops*, which has eight species, and nothing previously appears to have been known concerning its life-history. This writer has been able to fill this gap to some extent in describing the biology and metamorphoses of the commonest species, *A. hyperion*, which occurs in both running and stagnant water. It was reared upon mosquito larvæ and pupæ, which were eagerly devoured, an average of 209 being consumed by each isolated nymph in less than four weeks. S. Rostrup and M. Thomsen (*Tides for Planteavl.*, **29**, 395–461) write on the biology and control of Capsids upon apple trees. The species observed as pests are *Pleisiocoris rugicollis* and *Lygus pabulinus*. The latter insect has two generations in the year, and the eggs are laid on the bark of fruit bushes and fruit trees. The nymphs feed for a while on the sap of these hosts, but from about June they seek herbaceous plants, chiefly potatoes. The eggs of the summer generation are laid on the shoots of the potato, bean, etc., and when the adults are mature they seek the bark of fruit trees and bushes for oviposition, the winter being passed as eggs. *P. rugicollis* has only one generation and does not utilise in the same way other plant hosts. With regard to control measures against these Capsids, a single spraying a short time prior to blossoming suffices for all practical purposes. An

elaborate publication of 260 pages by E. A. Andrews on the factors affecting the control of the tea-bug, *Helopeltis theivora*, has been issued by the Indian Tea Association. One of the most important conclusions arrived at is that comparative immunity from attack accompanies an increase in the proportion of potash, as compared with phosphoric acid, in the leaf. It appears, however, that the potash in the leaf is not controlled by the intrinsic chemical composition of the soil, but by some unknown factor. The composition of the leaf may be different when grown on the same soil, under different methods of treatment, with a resulting difference in the degree of liability to attack. H. B. Hungerford (*Can. Ent.*, **54**, 1922, 262–3) records the presence of oxyhæmoglobin in a Notonectid, *Buenoa margaritacea*. It occurs in cells associated with certain of the abdominal tracheæ, and tests conducted by Sherwood for the author appear to leave no doubt as to the nature of the bright red pigment present. The record is of great interest, as hæmoglobin has only been known hitherto in larvæ of *Chironomus* and *Gastrophilus* among insects. The peculiarities of the digestive system of the Periodical Cicada form the subject of papers by C. W. Hargitt and L. M. Hickernell (*Biol. Bull.*, **45**, 200–12, 213–22). E. R. Speyer (*Phil. Trans.*, B, **295**, 111–46) has a lengthy paper on the biology of the Larch Chermes.

Other Orders.—R. T. Tillyard (*Trans. N. Z. Inst.*, **54**, 170–96) has monographed the Psocoptera of New Zealand, of which six families are represented in that country. Included in the paper is a discussion of the nymphal wing-tracheation to the adult venation. In *Jour. Linn. Soc. Zool.*, **35**, 143–216, the same worker discusses the wing-venation in Ephemeroptera, and brings forward a new system of homologies different from those of Comstock and Needham and of Morgan. In *Jour. N. Y. Ent. Soc.*, **31**, 31–52, J. Krafka discusses the morphology of the head among larvæ of Trichoptera, and concludes that the stem form is to be found in the cruciform larva, the campodeiform type being a specialised condition in that order. A new species of the small and comparatively new order Zoraptera is recorded by Caudell from Bolivia (*Proc. Ent. Soc. Washington*, 60–2). C. L. Withycombe (*Trans. Ent. Soc.*, 1922, 501–94) has an important contribution on the biology of the British Neuroptera-Planipennia, accompanied by a wealth of illustrations of the immature instars. The biology of the family Chrysopidæ forms Memoir 58 of the *Cornell Univ. Agric. Exp. Station*, by R. C. Smith, which is an equally careful and well-illustrated paper. The above two memoirs are to be specially commended to all who are interested in Neuropterous insects.

ANTHROPOLOGY. By A. G. Thacker, A.R.C.S., Zoological Laboratory, Cambridge.

In recent anthropological literature, the first place must be given to the *Journal of the Royal Anthropological Institute*, vol. liii, pt. 1 (January to June 1923), which contains an unusually large number of exceptionally interesting papers. I would call attention especially to Dr. R. Broom's article entitled, "A Contribution to the Craniology of the Yellow-skinned Races of South Africa." There is no section of ethnological science more interesting than the study of the primitive peoples who inhabited the vast sub-continent of South Africa in the days before the very recent southward movement of the Bantu. Dr. Broom deals with both the Hottentots and the Bushmen, and, unlike some writers, he believes that there are still hundreds of Bushmen, and thousands of Hottentots, surviving to this day who are pure, or practically pure, in blood. In regard to the shore-living savages who were described by the early writers under the name of Strandloopers and have left numerous "kitchen-middens," he thinks that these were not a separate race at all, but were in some places Bushmen, in other places Hottentots, and elsewhere, again, were probably a mixture of the two races. The author gives tables of the cranial measurements of a number of Bushmen skulls of both sexes. One of the most striking characteristics brought out by the tables is that the height is considerably less than the breadth, the breadth-height index varying around the figure 90. An interesting feature which appears to come out of the study of living Bushmen found in different environments is that the dwarfish stature of the inhabitants of the Kalahari Desert is apparently due to their inadequate diet and unfavourable surroundings, since Bushmen living under better conditions are fairly well developed. This fact is interesting, and suggests comparison with what is stated to occur in the so-called "misery-areas" of Europe.

Dr. Broom then proceeds to describe the Hottentots, and sets out data relating to their cranial characteristics. Their skulls are much more dolichocephalic than are those of the Bushmen, and the height is much greater compared with the breadth, the breadth-height index varying around 103. After dealing with the typical Hottentots, the writer passes on to the tribe of Koranas, who were fairly numerous in the Orange River and Vaal River valleys a century ago, though they are now reduced to a small remnant. These Koranas were different from other Hottentots, and spoke a different dialect of the language. They appear to have had a slight infusion of Bantu blood. But Dr. Broom relates a more interesting point regarding them, as follows: "In addition, however, to the Bantu

strain, there is some evidence of other and much more remarkable blood. In a number of Koranas who regard themselves as of perfectly pure blood we find clear traces of an Australoid race, or at least of a race with well-developed supra-orbital ridges. Large supra-orbital ridges are not a characteristic of either Bushmen, Hottentots, or Bantus, but, as will be shown presently, there is evidence of a race having lived in South Africa which had large, almost Neandertal-like supra-orbital ridges. Again, however, the evidence is not clear whether the Koranas met this Australoid race in South Africa or farther north, but as with the Bantu blood, the probabilities seem rather more in favour of the Australoid race having been encountered in the north." Dr. Broom describes a Korana skull found at Bayville, near Port Elizabeth, which has large brow-ridges. And he then briefly mentions the fossilised Boskop skull, and the now famous Broken Hill skull, which latter is supposed to represent of course a distinct species, *H. rhodesiensis.*

Dr. Broom's conclusion is that there were probably two ancient races in South Africa. One was the Boskop race, of which the Bushmen are the degenerate descendants. The other was the Rhodesian race, from which the Korana Hottentots derived their Australoid characteristics. He supposes that the Hottentots themselves are the result of an admixture of a northern dolichocephalic race with the Boskop race. The whole article is extraordinarily interesting.

Whilst discussing the natives of South Africa, I may take the opportunity to refer to some interesting remarks made by Dr. H. F. Sheldon in SCIENCE PROGRESS for October 1922, page 303. Dr. Sheldon comments on a statement of mine that "the Bantu reached South Africa after the white man," and makes the just criticism that I do not define what I mean by South Africa. Of South Africa in the political sense—Africa south of the Limpopo—the statement would be incorrect. The Bantus were certainly already south of the Limpopo at the time of the earliest Portuguese voyages. But it was not really this point that I had in mind. What I meant was that the Europeans, having arrived at the Cape of Good Hope and having moved four or five hundred miles north-eastward, met the black invasion and checked it in full career; so that the Bantus never conquered and took possession of the southern end of Africa, as they certainly would have done if the white man had not appeared on the scene. The fact that there were no negroes in Cape Colony at the time of the first Dutch settlements is no doubt familiar to everybody in South Africa, but many English readers might be unaware of this. As Dr. Sheldon raised this interesting point, I have endeavoured to ascertain the actual position of the Bantus in early historic times. The black wave

continued to advance southwards for three hundred years after the northward movement of the sparse white population began, and it was not until the middle of the eighteenth century that the two conquering races were fully in contact. As already stated, the Bantus were south of the Limpopo—were in " South Africa "—in the fifteenth century, and possibly long before this. At the end of the sixteenth century, as we gather from the accounts of the wreck of the *Santo Alberto* (in the year 1593), they had penetrated as far as the river Umzimvubo. Another hundred years later they were at the Great Kei River, and in the middle of the eighteenth century they reached the Great Fish River, which became a fairly definite border in 1778. It is fortunate for the modern anthropologist that the brilliant Portuguese navigators made their discoveries in time to save the primitive yellow races from being entirely blotted out by the powerful, numerous, and warlike swarms of the Bantu nations.

In the *American Journal of Physical Anthropology*, vol. v, No. 4 (October–December 1922), there is yet another paper on the Piltdown Jaw, this time by Dr. A. Hrdlicka. It will be remembered that many American palæontologists refused to accept Dr. Smith Woodward's association of the skull and jaw, and described the latter as that of a new species of chimpanzee. Having seen the actual specimen (not mere casts), Dr. Hrdlicka adopts an intermediate position ; he abandons the story of the chimpanzee, and admits that the jaw is primitive human ; but he thinks it cannot have belonged to the same race as the skull found near it.

The following papers may also be noted :

In *J.R.A.I.*, vol. liii (January–June 1923) : " The Pleistocene Deposits and their contained Palæolithic Flint Implements at Foxhall Road, Ipswich," by P. G. H. Boswell and J. Reid Moir ; " Maya and Christian Chronology," by R. C. E. Long ; and " Man's Nasal Index in Relation to Certain Climatic Conditions," by A. Thomson and L. H. D. Buxton. And in *Am. J. Phys. Anth.*, vol. v, no. 4 : " The Evolution of the Human Foot," by D. J. Morton. And in recent numbers of *Man* : " Further Evidence of Maglemose Culture in East Yorkshire," by A. L. Armstrong (September) ; " The Respective Sex-ratios of White and Coloured Races," by A. S. Parkes (October) ; and " The Sex-ratio and Race," by N. W. Thomas (November).

EDUCATION. By A. E. Heath, M.A., University, Liverpool.

To those of us who are engaged in the business of education, the highly technical literature on Formal Training is a real barrier to thought on the subject. It induces the feeling that the specialist equipment and vocabulary required are beyond the ordinary mortal. Nevertheless it is urgently necessary that all who wish to build upon scientific foundations should try to translate the broad lines of these investigations into everyday

language, and render them available in practical affairs. The urgency becomes clear once we realise that tacit assumptions about Formal Training underlie most educational schemes.

In general terms development in this field has been as follows. The doctrine of Formal Training, vaguely stated in the form that "mental power, however gained, is applicable to any department of human activity," formed the basis for the traditional disciplinary view of a subject's value. It does not matter upon what the mind is exercised if the exercise be vigorous and long-continued. This view was attacked as a matter of principle by the Herbartians, who held it to be built upon a false "faculty psychology." With the rise, in our own day, of a belief in educational experiment as the arbiter of methods, the type of discussion changed. First of all, the vagueness of the terms used became obvious as soon as the attempt was made at statistical measurement of the effects of transference; and then, when the problems involved were more precisely and more concretely stated, the mechanism of any possible transference became the centre of inquiry. It is impossible here to give adequate references even to a selection of the more important papers; but a trustworthy guide to them will be found in the article on "Formal Discipline from the Standpoint of Experimental Psychology," by J. E. Coover (*Psychological Monographs*, 1916, **20**, 3, 1–307). Attention is specially directed to the papers of W. H. Winch and of W. G. Sleight. I believe, however, that the general reader would do better, before coming to grips with the investigations themselves, to approach the literature by a flanking movement. In two books which have recently come my way a certain amount of dissatisfaction is expressed at the broad conclusions to which most workers in this field have been led—that very little real transference takes place. In each case the authors assert that the disciplinary value of studies is an observable fact which should not be overlooked because excessive or mistaken claims were made for it in the past. I am not sure that this assertion is altogether inconsistent with the results of experiments on transference; but in any case I suggest that a preliminary reading of these two volumes would form a good introduction to, and illuminate the practical importance of, the whole subject.

The first of these is a little book which seems to be less known than it deserves—*The Passman: How are our Universities to Train Citizens?* by Prof. R. L. Archer (A. & C. Black, 1918). Prof. Archer sets out from the fact that the modern pass courses took their origin from the then prevailing theory that a separate subject was needed to train each power of the human mind, and that all-round capacity would be secured by combining the

suitable subjects in due proportion. The failure of pass courses of this " balanced " type throws doubt on the underlying theory. On the other hand, the honours course, which aims at completing intellectual training by intensive study of a group of subjects possessing a felt unity, is everywhere successful. Before arguing that the pass course should be designed on the principle of the honours course, Prof. Archer feels it necessary, however, to deal with a possible fundamental objection, that this reliance on the general value to life of a unified course depends for its validity upon the outworn dogma of transference of capacity from the particular domain covered in the course itself to other fields of human experience. He therefore sets himself, in chapter iii, to inquire into the whole question of transference. He admits at the outset that the old idea that each subject provides training in certain general " faculties " will not do. On the contrary, each subject trains its own particular habits within each " faculty." Science trains a *scientific* imagination, a *scientific* observation, and so on. But it does not follow that a smattering of all subjects is needed; nor that no " mental training " whatever is possible within the study of a single domain. He contends that the whole argument of those who deny all transference is too crude, and based on too stiff and isolated a view of habits. Thus, when Thorndike has shown that children trained to be scrupulously neat in their arithmetic lesson remain as untidy as ever in other lessons, he goes on to claim that " habits " trained in one subject-matter will not " function " in another—or if they do, it is because there were common elements in the two fields. Bagley, however, affirms that if Thorndike's neat arithmeticians had *thought* about their neatness, they might have seen the desirability of being neat in other subjects. Yet he still holds that there are no " generalised habits "; it is " ideals " that can be carried over. The " habit " has still to be formed separately in each subject, but the " ideal " would provide the necessary stimulus. In the important work of Sleight there seems to be, also, a drift in this direction, as Archer points out. Sleight distinguishes three ways in which the study of a subject may prove useful outside its own domain: it is possible to carry over (1) concepts of method; (2) quasi-ideals, such as tidiness; and (3) ideals proper. These factors in the carry-over are, in general, conscious; and the function of the teacher is to employ them deliberately. " Instead of trusting to the formal training power supposed to be inherent in a given subject, we shall direct all our efforts to the building-up of general ideas of method of a kind which will admit of wide application " (W. G. Sleight, *Educational Values and Methods*, 1915, p. 96). With this, I gather, Prof. Archer would have no quarrel; but he insists

that the real point at issue between the modern disciplinarians and their opponents is that, though both believe in consciously directed transference, the former think it can also arise unconsciously after intensive work in a unified field, and the latter deny that it can do so. In this connection the following statement of Sleight is very significant: that "a general notion of the method employed in performing a piece of mental work may arise in our minds and be used upon some other subject-matter without our being aware of the fact" (*op. cit.*, p. 94). Prof. Archer at once takes advantage of this admission to suggest that this "general notion unconsciously held" is akin to the vague "feel" of our procedure which accompanies a stroke at golf. In other words, it belongs to the domain of habit, and not of thought at all. Such subconscious recognition of similarities between situations is indeed, for many purposes, more useful than deliberate thought: it is the basis of tact in dealing with the ordinary affairs of life. "Just as too much thought," says Prof. Archer, "impedes the execution of a good stroke at golf, so there are situations where too much thought about the course we should pursue hinders our effective pursuance." Every kind of study, pursued long enough and with sufficient intensity, and given the requisite capacity on the part of the student, will produce this form of "tact": and transfer of experience from lecture-room to life is possible wherever in life situations are found which can be subconsciously recognised as similar to the situation presented in studies. Out of this discussion it seems to me that we can begin to form some idea of what is and is not implied in the results of experimental work on transference. The view of the mind as an instrument which you first sharpen and then use is gone. It is a living thing whose life is in its contact with specific portions of experience. So, as Whitehead has said, the subjects pursued for the sake of a general education are not "general," but special subjects specially studied; and yet, on the other hand, one of the ways of encouraging general mental activity is to foster a special devotion.

Before turning from Prof. Archer's work to the second of our two volumes, it should be added that Prof. Archer goes on to strike a balance between the conscious and the subconscious sides of training. In treating of the plasticity of human behaviour, by the modification of habit-systems determined either consciously or subconsciously, he lays stress on the fact that even subconscious modifications may be helped by the formation in the individual of a conscious and general idea of plasticity itself. From this he concludes that a belief in the importance of conscious aims in thought and conduct is not inconsistent with recognition of the need for training in

that immediate subconscious reaction he likens to tact—a conclusion remarkably similar to that of the second of our two authors, Dr. E. H. Hankin. In his stimulating and lively book, *The Mental Limitations of the Expert* (Butterworth, 1921), Dr. Hankin, like Prof. Archer, puts forward a modified defence of formal discipline. His line of argument, developed with a wealth of detail, is that the power of practical judgment wielded by the "business man" is rooted in the subconscious mind and is of greater value in ordinary human affairs than the deliberate and conscious exercise of reason characteristic of the "expert." Yet our educational system is designed almost entirely for the expert. The stimulation of his reasoning powers, and the fostering of his capacity to recall all the pros and cons of any question—these are what he asks and gets, with the consequent slowing of initiative in action. He does not bother about that, however, because he has little need for initiative in action. He has to avoid rapid decisions. But the man of affairs needs the opposite treatment. He wants the power to take in the details of an entirely unfamiliar subject, with effort, and then forget them. His conscious mind should merely retain a general idea of the matter, the forgotten details being relegated to the subconscious, where they are ready to aid him in a quick act of unreflective judgment. He will then be in the position of the juryman who has forgotten the bulk of the evidence, but who gives a common-sense verdict. Dr. Hankin claims that modern educational methods are destructive of these powers. That is why, in his view, certain well-known systems of extremely formalised training, such as the learning by rote of Chinese classics, form so successful a foundation for business ability. The function of formal discipline, he suggests, is *not to exercise the reason, but to provide a training in the power of forgetting.* The work should not be too interesting, because if it is the details will remain unduly present to consciousness. Intensified interests spread over many fields do harm, because they inhibit everywhere the mechanism of forgetting. In formal work, as in the study of highly inflected languages, there is mental labour at complex material not intrinsically interesting, and so easily forgotten or momentarily passed out of mind. Dr. Hankin suggests that such studies should be the main element in earlier school years. The boy's reason should not be prematurely stimulated; our aim should be, instead, to foster his "common sense." Later, when this practically valuable reaction to everyday life has been consolidated, he may study, without harm, subjects that stimulate his reason and create interests in the world about him! This is a return to disciplinarian views, albeit of a subtler kind, with a vengeance. Dr. Hankin often expresses himself in a vague way, and I feel

sure that an analytical examination of the terms used would show that his view is not so much in opposition to other work in this field as appears at first sight. For instance, the effort and drill necessary to pass over " plateaux of learning " is as definite a part of interesting studies as of others : and relegation to the subconscious is a factor in the acquirement of *all* skill. Still, his work forms a fresh and unacademic approach to the subject of Formal Training.

The following is a selection of references to recent work :

E. L. Thorndike, " The Correlation between Interests and Abilities in College Courses," *Psy. Review*, 1921, **28**, 374–6.

W. H. Winch, " Children's Reasonings," pt. 4, *The Forum of Education* (the new name of the *Journal of Experimental Pedagogy*), 1923, **1**, 2, 152–7.

W. H. Winch, " The Transfer of Improvement in Reasoning in School Children," *Brit. Jour. of Psy.*, 1923, **13**, 4, 370–81.

Helen M. Woodhouse, " The Training Value of Exact Studies," *Forum of Education*, 1923, **1**, 1, 5–13.

D. J. Saer, " The Effect of Bilingualism on Intelligence," *Brit. Jour. of Psy.*, 1923, **14**, 1, 25–38. A decidedly bad influence is discernible which seems to persist into later life.

Kimball Young, " The Integration of the Personality," *Ped. Seminary*, 1923, **30**, 3, 264–85.

Ernest Jones, " Some Problems of Adolescence," *Brit. Jour. of Psy.*, 1922, **13**, 1, 31–47.

ARTICLES

THE INTERVENTION OF CONSCIOUSNESS IN MECHANICS

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The physicist, as physicist, sees the world essentially as a determinate mechanism. As a human being, on the contrary, he cannot help feeling, at any rate as a primary, naïve, unanalysed intuition, that certain events are governed by caprice, by his will and that of others. A problem thus arises. What opportunity can there be, in a determinate world, for the exercise of such capricious agencies ?

Certain tentative answers that have been suggested or implied must be mentioned only to be forthwith rejected. So, for example, to some an opportunity for capricious interference with the course of events has seemed to reside in the freedom of action left open by the two fundamental laws of thermodynamics. W. Ostwald [1] observes that " the organism utilises, in manifold ways, the freedom of choice among reaction velocities, through the influence of catalytic substances, to satisfy advantageously its energy requirements." H. Guilleminot [2] sees the influence of life upon physical systems in the substitution of guidance by choice in place of fortuitous happenings, where Carnot's principle leaves the course of events indeterminate.

This point of view evidently cannot bear scrutiny. The two laws of energetics, indeed, are not competent to predicate unequivocally the course of events; but this is true whether living beings are present or not. Yet, in the absence of such beings (and therefore irrespective of their caprice) the course of events is determinate, as we commonly suppose, in obedience to other principles well known to the physicist, and more exacting than the broad but indecisive first and second laws.

[1] *Vorlesungen über Naturphilosophie*, 1902, p. 328.

[2] *La Matière et la Vie*, 1919, p. 121 *et passim* ; *Nouveaux Horizons de la Science*, 1916, p. 415 *et passim*. Compare also Sir Oliver Lodge, *Life and Matter*, 1906, pp. 133 *et seq.*, 140, 148.

No; if room there is, in dynamics, for capricious intervention, the point of attack of the disturbing agencies must be sought elsewhere; the joint in the armour must be nearer the heart of the matter.

Others have seen the opportunity for the interference of consciousness with the course of physical events, in certain indeterminate situations that arise in mechanical systems. Among the orbits of such a system some are unstable, passing through singular points at which "influences whose physical magnitude is too small to be taken account of by a finite being, may produce results of the greatest importance. . . . In the course of this our mortal life we more or less frequently find ourselves on a physical or moral watershed, where an imperceptible deviation is sufficient to determine into which of two valleys we shall descend."[1] This conception makes the intervention of conscious volition in mechanics an exceptional occurrence, reserved for special occasions, or at least for a special set of circumstances, and Clerk Maxwell's quotation, in this connection, of Shakespeare's famous lines regarding the "tide in the affairs of men" seems to lend colour to the supposition that he did conceive of this interference as in some measure an exceptional occurrence. In point of fact, while heroic efforts of the will are unquestionably rare incidents, volitional activity of all shades, down to the almost imperceptible, is a constant ingredient of human consciousness. It is, of course, conceivable that an exceptional set of dynamical circumstances is constantly present in the living organism. But, other things equal, one would feel better satisfied with a point of view that should give place to consciousness in the scheme of nature without resorting to the supposition of dynamically exceptional conditions. Such a point of view is now to be developed.

Consider the elementary equation of dynamics

$$f = ma \qquad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

This equation is evidently devoid of meaning, or at any rate has no useful meaning, unless we have some other means of measuring the force f than by observing the mass m and the acceleration a and forming the product ma. It may be, for example, that the force f, to which (1) refers, is the tension of a stretched spring, and that we can measure this tension by the increase l in the original length L of the spring, writing

$$f = \mathrm{K}\frac{l}{\mathrm{L}} \qquad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

[1] *Life of Clerk Maxwell*, by L. Campbell and W. Garnett, 1882, pp. 434–44; Henderson, L. J., *The Order of Nature*, 1917, pp. 222–3, 226, 227. Compare also Boussinesq, V. J., *Conciliation du déterminisme mécanique avec l'existence de la vie*, 1878, reprinted 1922.

where K is a characteristic constant of the spring. The equations (1) and (2) can then be united, *with elimination of f*, into the relation

$$ma = K\frac{l}{L} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The process by which the relation (3) has been obtained reminds one of a method sometimes employed in throwing a bridge across a river : Two independent structures are reared, one on each bank, and these two structures are gradually extended, with the aid of suitable scaffolding or auxiliary stiffening members, until they meet at the centre of the water-course. They are then consolidated, and the auxiliary members, the falsework, having served their purpose, may be dismantled. So in forming the equation (3), in throwing the equality sign across from the member ma to the member $K\frac{l}{L}$, the symbol f has functioned merely as a temporary truss, as a falsework to be discarded as soon as the relation is established.[1]

In point of fact this is the only function that an abstract force ever serves in the relations of classical dynamics. It never appears explicitly in the equations of physics or engineering practice, as applied to any concrete case. The force is always eliminated in the process of forming the practical working equations. It never appears explicitly in these equations. Indeed, it cannot do so, *for we have no means whatever of directly measuring an abstract force f*. We can measure it only in terms of those very quantities which we substitute in place of f, thus eliminating f, whenever we form a working equation.

Does it follow that the abstract force f is of no practical significance whatsoever, that it is a mere figment, an artifice, possessing no counterpart in objective nature, and incompetent, therefore, to exert any influence upon the course of events ?

This is not a question to be hastily answered. Some may be disposed to assert, upon grounds known to themselves and not to be drawn into this discussion, that Newtonian abstract forces are indeed merely figments, perhaps not even useful figments. But this is not the question here raised. The question here is, Does the unreality of such forces, and, in consequence, their incompetence to affect the world's events, follow as a necessary inference from the fact that in the practical working equations these forces f never appear explicitly ? And the question may at once be made more general in scope.

[1] Compare Whitehead, A. N., *Principles of Natural Knowledge*, 1919, p. 19.

If a certain quantity F is invariably eliminated in the formation of the working equations of the law of operation of a real material system, so that F never occurs explicitly in these equations, does it follow that F cannot correspond to any objective entity having an influence upon the course of events in that system?

The answer, perhaps, is immediately plain on the grounds of general philosophical principles. But if not, an illustrative example may serve to resolve any doubts.

A mathematical theory of wealth, covering at any rate certain aspects of economics, can be built up in terms of prices and sales alone, without pushing the analysis of fundamentals beyond this point; that is to say, without examining the human motives that, presumably, find their numerical expression (not to say measure) in prices. On such a basis as this, for example, Cournot founded his admirable *Researches into the Mathematical Theory of Wealth.*

But it is also possible, if one is so disposed, to carry analysis farther, to inquire into the psychological phenomena that appear at least as contributing factors in the processes of price determination. One is thus led to a theory of value such as that developed by Jevons. In a theory of value of this type human *desires* occupy a central position, and in the analytical symbolism of the theory appear prominently certain quantities ϕ, variously termed, by different writers, "desirability," "utility," "ophelimity," etc. Now it is conceded by the exponents of theories of this type that these quantities ϕ admit, at best, only of indirect measurement, through prices. And, in point of fact, in all concrete applications these quantities ϕ are eliminated from the working equations. It is taken as quantitatively symptomatic of equality of desire for two commodities, if I am willing to purchase either the one or the other, indifferently, at the same price, *in maximo*. Now, desires that do not lead to purchases and sales evidently never enter the arena of economic contest, and therefore, it would seem, can be of no interest in the study of practical, commercial economics.[1] Pareto, who certainly cannot be regarded as adversely disposed toward economic theory of the type developed by Jevons, remarks[2]:

"Les notions de valeur d'usage, d'utilité, d'ophélimité, d'indices d'ophélimité, etc., facilitent beaucoup l'exposé de la théorie de l'équilibre économique, mais elles ne sont pas nécessaires pour construire cette théorie."

It may thus appear that the ophelimities ϕ are wholly

[1] Cournot, A., *loc. cit.*, p. 46: "We do not see for what reason theory need take account of any demand which does not result in a sale."

[2] *Manuel d'Économie Politique*, 1909, p. 160.

gratuitous importations into the theory of economics, introduced into the discourse only to be forthwith eliminated as soon as concrete application is to be made.

Are we therefore to conclude that *desires* are mere figments? Or even, granting their existence (since this will hardly be denied), are we to contend that they are without influence upon economic events? Surely not. And yet, to an onlooker unable to peer behind the scenes, unable to taste directly of these desires that we observe by introspection as operating in our own selves, the matter might not thus appear. For, however erratic human desires may be in detail, in the gross they have an appearance of uniformity, of law, of constancy —a fact recognised long ago by Adam Smith, who "considered a science of economics possible because of a few outstanding traits of man which guaranteed self-preservation, while also promoting the welfare of society at large."[1] The same constancy in the *average* performances of men was again noted, from perhaps a somewhat different angle, by Quetelet, and once again by Spencer, who was the first to recognise the full significance, in the evolution of the race, of that "adjustment of feelings to actions" which has made "pains . . . the correlatives of actions injurious to the organism," and "pleasures . . . the correlatives of actions conducive to its welfare."[2]

To our extra-mundane observer, innocent of human emotions, and observing only in the gross the physical facts of our comings and goings, our doings and the action of environment upon us—to him a system of laws, of correlations, might well become apparent, by which the past, present, and future course of events could be computed according to formula. His fundamental ignorance of our *motives* would in no wise disturb him in his calculations—so long as he were dealing only with human actions in the gross, and so long as his observations were not carried to an over-nice degree of precision, such as might reveal an occasional perplexing departure from the norm. He would be dealing solely with correlations—underlying motives would not concern him; he might affect disdainfully to ignore them as "metaphysical" speculations incapable of proof or disproof.

Yet we, who are in the secret, would know beyond peradventure that his picture of our world and its happenings was deficient in a most essential particular. The accord between his formulæ and facts might be never so good—in eliminating the ϕ s from his working equations he would have banished from his representation of nature what must appear to us, "who know," the very core and essence of our being.

[1] Boucke, O. F., *American Economic Review*, 1922, p. 599.
[2] *Principles of Psychology*, § 124.

Our immediate interest in the example furnished by the operation of "desires" in economics resides, not in such analogy [1] as these may or may not present to physical forces, but in the demonstration which the example has supplied, that a quantity eliminated in the process of forming the fundamental equations representing the law of operation of a system may nevertheless play a highly significant rôle in the history of that system. And another point also is well illustrated. To the extra-mundane beholder of the drama of human society, a system of correlation formulæ might suffice to represent the laws of operation in that society so long as observation were comparatively gross; yet these formulæ might fail when scrutiny were pushed to such refinement as to reveal departures from the general average, which average alone was the object of the grosser observation. Or, without increased precision of observation, instances might occur, special cases present themselves, in which individual salient departures from the norm forced themselves upon the notice of even the gross observer.

It might so be that the elimination of the ϕs were feasible only with regard to the average phenomena observed, but could not be carried out in a perfectly general way. In that case an *approximate* description of the laws of the system could indeed be given in terms not containing ϕs, but for an exact description the ϕs must enter explicitly.

This reflection is suggestive, because it reminds us that we must be prepared for this eventuality: the established working formulæ of dynamics, in which abstract forces do not enter explicitly, may be true only in the sense of statistical averages, and only in approximation, however close. It may be that in all cases they depart in some degree from the truth, and that in some special cases—perhaps where conscious organisms enter—they do so very materially, through the absence of certain quantities whose elimination is justifiable only with reservations, whereas they have been eliminated outright.[2]

It is thus seen how the equations of dynamics, from their very nature, open the door for possible "capricious" interference with the course of the world's events, through such

[1] That such analogy exists has been noted, from different points of view, by various authors. See, for example, Zöllner, *Über die Natur der Kometen*, 1872, pp. 201–3; 211–19; 325–7; 361–4; Petzolt, J., *Maxima und Minima und Ökonomie* (Göttingen Dissertation), p. 22; Edgworth, F. Y., *Mathematical Psychics*, 1881, pp. 88–91; Winiarski, *Revue Philosophique*, 1900, vol. xlix, p. 113.

[2] This conception is reminiscent of the *tychism* of Charles Peirce. (*The Monist.*, vol. ii, 1892, pp. 333–4, 533. See also Henderson, L. J., *The Order of Nature*, 1917, p. 100.)

agencies, for example, as that which we observe in ourselves and denote by the word " will."

But given the formal possibility of such interference, a new direction may now be given to our inquiry if we ask what may be the *function* of such interference ; how, in the evolution of this world, conscious volition, such as ours, entered the scheme of nature, and how it gained for itself its present dominating position. Parallel with the problem of the origin and evolution of life runs this problem of the origin and evolution of consciousness, of will in general, and of those particular types of will, those behaviour-patterns, which we observe in ourselves, and which are revealed to us, more objectively, in the collective actions of bodies of men viewed in the mass, through the spectacles of statistical analysis. Now, of the two problems—that of the origin and that of the evolution of such will-types—the latter is the less formidable, it seems. Since Herbert Spencer the scope of *free will* has been recognised as in any event circumscribed. We may, perhaps, *do* what we please, but we cannot *please* what we please, for certain types of will-patterns, should they come into being, are incapable of prolonged survival, and cannot therefore be found in a race, such as ours, that boasts of a long line of ancestors.

More baffling is the inquiry as to the *origin* of consciousness in the organic world. If we contemplate any one single act of a conscious organism, any one single response to external stimulus, it will be found that this response, or something very like it, can usually, if not always, be duplicated by some mechanism of human construction. It should hardly be necessary to adduce instances. Perhaps the most telling examples are certain mechanical toys, such as a tin beetle which gravely walks across a table-top, seemingly courting destruction (as a going concern, at least) by making straight for the precipice at the edge of its universe. But such emergency is duly provided for : when it reaches the danger-line, the creature promptly turns and continues its perambulations hugging the edge of the table with its antenna. Now the question arises, if such *zweckmässig* actions and responses can be produced by purely mechanical means, why has nature resorted to consciousness, and resorted to it in increasing measure, in the evolution of the most exquisitely adapted organisms ?

An obvious answer—too obvious to be accepted without further examination—is that there is secured, by the introduction of consciousness, an economy of parts, a comparative simplicity of structure. This certainly is true, that if, instead of being satisfied with the mechanical duplication of some one feature of animal behaviour, we insisted on reproducing, by mechanical means, the entire behaviour-pattern of a dog, for

example, or even of any animal much lower in the scale of evolution, we should find ourselves inextricably embarrassed in an altogether unmanageable maze of mechanical parts. But how is this technical difficulty overcome by the introduction of consciousness? Are the ascertained facts regarding consciousness competent to throw any light whatsoever on this question? Let us briefly consider the matter.

I know consciousness, as a natural phenomenon, directly in myself. I know it also indirectly, in myself, in its outward manifestations as I observe them in my acting and being-acted-upon. In myself, also, I know the relation between the phenomenon of consciousness, as observed directly, and these outward manifestations. Observing similar acting and being-acted-upon in others, I infer and hypothecate for them also, indirectly, similar consciousness, similarly related to these outward manifestations.

So long as my inference relates to organisms closely resembling me in their outward characteristics, this process of inference perfectly satisfies the mind, and is thoroughly justified pragmatically in its consequences, that is, in the results of actions based upon my hypothesis of consciousness in those others. The process becomes more and more dubious as it is applied to organisms lower and lower in the scale of animal life, organisms differing more and more profoundly from me in their outward characteristics. Most of us will hesitate not at all to postulate for a dog a species of consciousness having much in common with our own. We should be much more hesitant in the case of an amœba. And an important point to note is that as we recede farther and farther from constitutions similar to our own, not only do we become more and more dubious as to the existence of consciousness, but it becomes increasingly difficult for us to assign any definite meaning to the term "consciousness" as applied to an organism or portion of matter so constituted. In our own selves consciousness is a phenomenon essentially changeful in intensity, complexion, and content. It changes with season, age, and circumstance. Moreover, it must be borne in mind that certain features of our consciousness, features which we are habituated to look upon as inseparable characteristics inherent in the nature of the phenomenon, may in truth be only incidental, and may be peculiar to our particular type, our particular species of consciousness. So, for example, the normal healthy individual must find it difficult or impossible to conceive of any consciousness save that which is spun into one continuous thread by one presiding Self or Ego. Yet we know from the study of "abnormal" individuals that continuity may be lacking, there may be no clearly appreciated Self, or there may be

several Egos related to one body; and if there is but a single Self, its expanse may vary within wide bounds, from the small, narrow Self of the immature or mentally and morally underdeveloped person, to the world-embracing Self that says to every object perceived: "Thou art part of me."

Our mind should, therefore, be in fair measure prepared to contemplate the possible existence of consciousness departing more or less radically from that general type to which ours conforms. But there are limits to this elasticity of our ready conception of consciousness. *We must be prepared for the eventuality that the varieties of the objective phenomenon of consciousness may exceed the elasticity of our powers of conceiving these varieties*, that is, our power of presenting them to our imagination in terms of consciousness as known to us directly by introspection.

Consciousness, such as we know it directly in ourselves, and such as it is known to us in the least doubtful examples outside ourselves, seems intimately connected, in some manner, with life. Serious interruptions of the normal life processes are commonly accompanied by corresponding irregularities and sometimes by definite interruptions of the stream of consciousness. This suggests that consciousness is a state of matter, or attends a state of matter, which requires to be continually "excited" by certain life processes, somewhat as the magnetism of the soft-iron armature of a dynamo is maintained by the passage of an electric current through a neighbouring conductor.[1] And since one of the most prominent characteristics of life is that it is constantly attended by chemical change (metabolism), the inference readily comes to mind that consciousness is, or attends, a state of matter in the process of chemical transformation.[2] And, further, since the distinction between chemical and physical changes is presumably arbitrary, our inference must be broadened, so that we find ourselves contemplating consciousness as a phenomenon associated with, or attendant upon, matter in a state of strain, or, perhaps, matter in the process of yielding to a strain.

[1] Compare Sir Oliver Lodge, *Life and Matter*, 1906, chapter viii, in particular p. 129.

[2] This thought has probably occurred to many others as well as the writer. He has found it expressed in a book *The Unity of Consciousness*, by W. E. Ritter (vol. ii, p. 291); and also in an article by Dr. Charles Steinmetz in *Harper's Magazine*, vol. cxliv, 1922, p. 300. The fugitive condition through which matter must pass on its way from one molecular arrangement to another still remains an almost wholly unexplored field, and Schoenbein's comment upon it (*Jl. f. prakt. Chemie*, vol. lv, p. 152) holds almost with its original force; except that Sir J. J. Thomson's positive-ray analysis, with its power to weigh bodies existing but for one ten-millionth of a second, has proved competent to register the mass of such decapitated molecules as CH_3.

We find ourselves now in this singular position: we have based our reflections expressly upon the fact that consciousness, as typically known to us, is characteristically associated with life. And, by a few short steps, covering but two or three lines of print, we have arrived at a point of view which contemplates consciousness as a phenomenon that may be common to all matter.

This is not as strange or as disconcerting as it may at first sight appear. It is notorious that all attempts to define life, to distinguish categorically between living and non-living matter, are dismal failures. More and more, as man's knowledge advances, lines of demarcation in his outlook upon nature, in his representation of her, are found to be of subjective origin, and devoid of correspondence with any parallel lines of demarcation in objective fact. It is true that our imagination is quite incompetent to form for us any conception, in terms of our habitual modes of consciousness, of such elementary forms of the phenomenon as may be associated with the course of events in so-called non-living matter. Certainly the element of continuity and of synthesis into a thread of "personal" experience must be lacking. But, as already remarked, these features may not be so fundamental traits of the phenomenon of consciousness as we ordinarily suppose. The finished product of highly developed consciousness, such as we know it in ourselves, must represent, not a typical example, but rather "the acme of accomplishment in the integration of the animal organism. . . . As such it has spelt biological success to its possessor."[1]

The problem of the origin of consciousness in living organisms now is seen in a new aspect. Its *ultimate* origin, of course, is one of the unsolvable riddles of the universe; but that is not the question here before us. Our curiosity relates not to this ultimate origin, but to the origin of consciousness *as one of the possessions of the animal organism.* Where and how, in the progress of evolution, did the organism acquire this remarkable and pre-eminently useful quality? If the reflections set forth above are to the point, then this riddle is answered: the organism did not acquire consciousness at any time; consciousness in some rude form belonged to it from the beginning, as it belongs to all matter. What was evolved was not consciousness itself, but a certain particular type of consciousness, a type of consciousness integrated into or around a more or less clearly defined Ego that presides over the behaviour of

[1] Sir C. S. Sherrington, "Some Aspects of Animal Mechanism," Presidential Address, British Association, 1922 (*Nature*, September 9, 1922, p. 350). I have taken the liberty of applying to function the words originally applied to the corresponding structures.

the organism, and imposes unity of behaviour. As to the origin, the mode of development of this particular type of consciousness, we are tolerably well supplied with at least strong circumstantial evidence. This type of consciousness is characteristic of organisms that seek their food *actively*. Plants, whose food seeps to them by a spontaneous process in which they remain essentially passive, are devoid of those structures which observation teaches us to associate with the integrated type of consciousness. Even we ourselves adopt essentially the vegetative habit in drawing that part of our supplies which surrounds us in homogeneous abundance, and which is replenished by the automatic process of gaseous diffusion as fast as we consume it in respiration. Undoubtedly the food quest has been the dominating factor in developing the psychophysical apparatus of those species of organisms whose food is scattered heterogeneously—piecemeal—over the earth and has to be *earned* before it can be consumed. It may be humiliating, but there can be little doubt that we owe our mentality to the necessity of providing food; our mind, whatever may be its functions now, has been developed primarily as a means to fill our stomachs.

We have thus found at least the suggestion of an answer to the question: How did living organisms enter into the possession of consciousness? They possessed it, according to this view, from the beginning, in common with all matter, in certain circumstances. Our *particular type* of consciousness was developed by the exigencies of the food quest.

But there still remains the question: Why was the highly integrated type of consciousness selected, in preference to ordinary mechanism, to produce certain effects which, singly at least, can be copied by what we are pleased to call purely mechanical means? It has been suggested above that the utilisation of consciousness for this purpose in some way secured economy of mechanical parts. Conflicting forces, and forces working together toward a result, are, in man-made machines, made to bear on one another, to produce their resultant, through material, mechanical members. What if they could be brought to a compact, to strike their balance directly, before they apply themselves to a piece of matter? Is this what is taking place in our psychophysical apparatus when we weigh alternative courses of action, and make a decision? Is this the secret of our neural integrating system? There is a certain plausibility about the conception. It would account, at least in part, for that simplicity of structure, that compactness of material apparatus, out of all proportion with the results achieved, which is so characteristic of the human brain. And it would accord with a deep-rooted article of

faith, the rock on which stands our concept of causation: we have an intuitive conviction of being the cause of the events which we successfully will.

It is undoubtedly from this conviction that we derive (by a species of anthropomorphism which *may* be justified) our concept of causation.[1] Perfect correlation may be a symptom of causation, but it is not itself what we *conceive* as causation. This is perfectly clear from the fact that we may have (practically) perfect correlation between two sets of circumstances, which, by common consent, are admitted to stand in no direct causal relation. Our subjective *concepts* of perfect correlation and of causation are therefore clearly distinct, whatever may be the corresponding objective situation. Those who would totally exclude the naïve concept of causation from their repertoire of scientific concepts forget, for the moment, that they observe causation in themselves every time they perform a voluntary act. Busying themselves constantly with the observation of systems that are accessible only to observation of correlations, they have become oblivious of the fact that there are other systems accessible to observation through an exceptional channel (owing to the special limitation of the nervous system within the confines of the body), and that in these systems, if nowhere else, they know causation by direct experience. Whether causation of this same kind occurs, as a natural phenomenon, also outside these systems thus exposed to our scrutiny by an exceptional method of observation, that is a question toward which the only scientifically sound attitude, in the present state of knowledge, is that of the open mind.

[1] To object that "the Ego is mistaken in supposing himself in any way the cause of the actual result" (to use Clerk Maxwell's phrase) is beside the point. The *conviction* exists as a phenomenal fact, irrespective of the *justification* of the conviction. If we define causation as a certain phenomenon of which we are directly conscious in volitional action (this we are entirely at liberty to do), the question of self-deception cannot arise at all; its place is then taken by the question whether causation of the kind described by our definition has any part in "inanimate" nature. The physicist does not ordinarily concern himself with this question; he is well satisfied, for the time being, to leave it aside, his interest being, to-day, solely in correlations. (Compare Boucke, O. F., *loc, cit.*, pp. 603–4; Pearson, K., *The Grammar of Science*, 1900, pp. 122, 128 *et passim*; Niles, H. E., *Genetics*, 1922, vol. vii, p. 259.) But whether this policy of deliberately ignoring a fundamental phenomenon of nature can be lastingly satisfactory must appear very doubtful; certainly, we cannot affect surprise at the fact that, adopting this policy, we should find ourselves in difficulties when we come to consider physical systems comprising conscious, living matter—a consideration which the physicist has hitherto avoided with discretion, or perhaps one should say with timidity, but for the attack of which sooner or later courage must be summoned. Blinders may be a justifiable part of the harness for a nervous horse, but they should not properly form part of the equipment of the searcher after truth.

The point of view that has here been developed conceives such causes as possibly at work in all physical events. What distinguishes us from so-called unconscious, inanimate matter is not, according to this view, the presence in us, and the absence in it, of causation of this kind; but the integration in us, the bending to one end, of influences which in it are more or less chaotic, dissipative, bearing no relation to a controlling unity.

ON THE FUNCTION OF SECONDARY SEXUAL CHARACTERS

By J. C. MOTTRAM, M.B. (Lond.)

Nature, starting with a unicellular organism, has gradually evolved the highly complex vertebrate animal. This has been brought about by the specialisation of cells. At first there were colonies of similar cells forming zoöglœa-like masses. An early stage in specialisation was into a two-layered animal (grastula) in which the outer cells were designed to give protection to an under layer, the chief function of which was to digest food. A three-layered animal (blastula) was the next stage, in which the middle layer gave the power for motion and was responsible for the transport of food to the different parts of the body: and from this basis, through further cellular specialisation, the vertebrate animal can be traced.

Nature has not been content to deal only with the cellular unit: directly animals began to aggregate themselves into herds, she began to work with individuals as she had done with cells. It is possible gradually to follow the increase in specialisation which separates an undifferentiated colony of animals from a modern first-class nation.

We can imagine an advantage in all this specialisation which has gradually evolved from this primitive unicellular form of living matter. We can demonstrate the advantage of the specialisation of cells to a multicellular individual: for instance, the protection of digestive cells by an external layer of cells especially differentiated to withstand injuries; the capacity of transport provided by muscle cells especially differentiated for this purpose.

So also, can be demonstrated the advantage of specialisation of the individual of a herd: for instance, in man, those who make the food are protected by soldiers, and all are transported from place to place by individuals trained in engineering, but who know nothing of soldiering or farming; so that a nation exhibiting advanced specialisation has the advantage over a collection of undifferentiated individuals.

It is thus clear that, no matter whether we are considering the character of an individual cell of a multicellular animal, or

the character of an individual of a social animal, in an endeavour to correlate character with function, it is essential to take into account the particular part played by the cell or the individual in the economy of the multicellular animal or of the herd of individuals.

If, for example, we find a character that is a disadvantage to the individual in the struggle for life, we must ask ourselves the question: Can it be that this character, though a disadvantage to the individual, may be an advantage to the society to which the individual belongs?—for instance, the inability of a soldier to grow corn or drive a motor may look like a serious disadvantage to him, yet these disadvantages disappear when, as members of a nation, these various wants are provided for.

We can now pass on to consider secondary sexual characters, and to ask ourselves whether or no this differentiation of individuals can be brought into line with the cellular differentiations and individual specialisations already considered. When considering the character of cells, it is necessary to examine closely the particular function in the economy of the body which they serve: for instance, digestion by cells lining the alimentary canal, excretion by the cells of the kidney, etc. So, when considering individuals, the particular part played by the individual in social economy is a prime consideration when endeavours are being made to account for characters which are not generally distributed among the individuals, but are present in one set of individuals and absent in another.

Now these are precisely the conditions found in secondary sexual differentiation. Firstly we observe an association of two animals to form a pair, male with female, and secondly we observe that certain characters are confined to one sex: thus we have to ask ourselves what function these characters serve in the economy of the pair, rather than in the economy of the individual.

We are not here concerned with an attempt to account for sexual differentiation; we do not require to know what advantage lies in the differentiation of the sexual cells into two kinds, sperms and ova, nor in the housing of these cells, in the majority of animals, in separate individuals; neither are we concerned with any characters which regulate the sexual function; on the contrary, secondary sexual or extra-sexual structures are characterised by having no obvious connection with the sexual function. Thus it is that we are forced to ask ourselves whether these characters are related to a function of the pair, rather than to the particular individual, male or female.

What, then, are the particular functions of males and females, in respect of the pair?

(1) *Males as Protectors of the Pair.*—Our own feelings tell us, as well as the ways of animals, that the male has a strong instinct to protect the female: here, then, we have a male secondary sexual character clearly related to the pair and not to the individual, and correlated with this instinct are many structures which give the male greater offensive power than the female, such as powerful muscles, horns, antlers, spurs, teeth, etc.

We have now to ask ourselves what advantage to the pair can be this willingness of the male, rather than the female, to risk an encounter with an enemy and thus sacrifice himself?

The answer is not far to seek: it is because the female of the pair is of much greater value to the species than the male, and, further, the advantage to the pair must be the advantage to the species. In nature, neither the individual nor the pair counts, only the species: once survival is assured, the individual may disappear.

(2) *Females as Rearers of the Young.*—Here also we have knowledge of an instinct often confined to the female, which provides for the protection and care of the young, and which is associated with a number of secondary sexual structures, such as pouches, mammæ, etc. The utility of these to the species is very clear, and does not here require special consideration.

(3) *Males as Defenders of Territory.*—It has long been known to field naturalists that pairs of animals preserve to themselves defined areas over which they hunt and find their food, and that the intrusion of another individual of the same species results in fierce combats. Howard's *British Warblers* has shown that among birds it is the male who is the defender of these territories, and that among migratory birds the males arrive at the breeding-ground before the female and defend it until she arrives. These combats often result in death, and the great muscular development of males and the various offensive weapons which they especially present are undoubtedly related to this function of the male to fight with other males of the same species in mortal combat. This sacrifice of males is an advantage to the species: it gives a proper feeding-ground for the rearing of a family, it promotes a wide distribution of the species rather than overcrowding at especially favourable situations. For this purpose, the relatively non-valuable male is sacrificed rather than the valuable female.

These extra-sexual characters are especially to be found in the polygamous species where the male (because of his power to fertilise many females) is especially worthy of this sacrifice, as in the deer.

(4) *Males who Sacrifice Themselves to Enemies.*—The balance

of nature, which, as a rule, keeps the number of individuals of a species more or less constant, ordains that individuals must frequently fall victims to animals of prey. In view of the greater value of the female, it will be an advantage to the species if, of pairs, males are killed rather than females : this is brought about by conspicuous colour or form in the male, often only assumed during the association of the pair, by structures which hamper his movements, and in other ways ; on the other hand, the female remains inconspicuously coloured and of a retiring disposition : she does not draw the attack of the enemy like the gaudy dress and striking form of her partner.

Not all animals present these extra-sexual characters ; in some birds and insects, for instance, both sexes are conspicuously coloured. Experiments have shown that these are relatively unpalatable—kingfishers, crows, etc.—and their conspicuous colour is of a warning nature to distinguish them from other species and thus to warn an enemy of their distasteful nature. It follows that the association of conspicuous males with inconspicuous females should be especially prevalent among palatable species—those liable to attack from enemies. This is undoubtedly the distribution ; the very palatable ducks, pheasants, and finches may be cited as examples. Further, polygamy, by decreasing the relative value of the male, should encourage these sacrificing methods : here, again, the prediction is true, as in the peacocks and the pheasants ; also the male is relatively decreased in value when he plays no part in rearing the young : this too is associated with brilliant males, as in the ducks ; compare the partridge, which is not polygamous, and in which the male is as important to the young birds as the female—both sexes are protectively coloured. Again, this character of the pair would be of no use to powerful animals able to defend themselves, as in the carnivora—hawks, swans, and geese, etc—which do not present extra-sexual colour differences.

From this theory it would be deduced that these characters would tend to disappear directly the pair was broken up ; in very many cases this is so, and in the case of the ducks, for instance, a special summer protective plumage is acquired by the male as soon as he deserts the female. It would also be deduced that the young would simulate the coloration of the female and be protectively coloured, but that, in unpalatable species, in which both sexes are conspicuously coloured, the young should in this case be conspicuously coloured also. This actually is a law of nature, with few exceptions.

We now come to those rare cases in which the female is more conspicuous than the male.

According to our thesis, we should expect to find that, for

some unusual reason, the female was of less value than the male. Now, in nature, males and females are usually approximately equal in number, and this is a reason why males are usually less valuable than females, for one male can fertilise many females. But if females were to outnumber greatly males, their relative value would be reversed : this is the case in many of those rare instances where females are brighter than males.

A second possible cause is, that sometimes the male alone tends the young, thus much increasing the relative value of the male to the pair.

Hence the colours of males and females are always related to the eyes of their enemies :

(*a*) In unpalatable and powerful animals, conspicuous coloration in both sexes to warn their enemies (in preying animals inconspicuous in both sexes, to be concealed from their quarry).

(*b*) In palatable animals where the sexes are of equal value, protective coloration in both sexes so as to be concealed from their enemies.

(*c*) In palatable animals where the male is the less valuable, the female is protectively coloured for concealment, the male more or less conspicuously coloured so as to draw attack on himself.

(*d*) In palatable animals where the female is the less valuable, the male is protectively coloured and the female more or less conspicuous.

Conspicuousness in nature may be produced by motion as well as by colour and form, and appears to be most often made use of at times of special danger : when, for instance, a family is attacked by a preying animal, the parents will display conspicuous movement to draw the attack on themselves, whilst the young hide. So, also, the male will draw the attack of an enemy from the female, who then behaves as the young do when the family is attacked.

It will thus not be unprofitable to consider briefly the displays of courtship, although our knowledge of them, much added to of late by such field workers as Howard, Huxley, and Selous, is still small. The activities play a rôle in the act of fertilisation, and at the same time give the performers an appearance of being in distress, to which all animals of prey are especially attracted ; moreover, the displays which animals use to draw an enemy away from their young are very similar to, and often identical with, the displays of courtship.

Now, just as with bright colours, so with these displays, we find that in some species the male alone displays, as in the peacock ; in others, both sexes display, as in the great crested

grebe ; in others, again, neither sex displays, or at most in a very simple manner. Further, there is evidence that the distribution of these variations in magnitude of display is similar to the distribution of bright colours. The most elaborate displays occur in palatable animals in which the male is less valuable than the female (pheasants, blackcock).

In unpalatable animals, display is much less in evidence. In palatable animals, where the sexes are equal in value, both sexes indulge in elaborate displays, as in cranes.

It is difficult to be quite sure of these facts, as our knowledge of the distribution of display is much less than that of colour, and, further, it is by no means easy to assess a display—to say that this display is more elaborate than that.

If this analogy between colour and display be true, then the selective factors which have been concluded to control the wearing of bright colours would also control the displays of courtship.

If these displays are only related to the act of fertilisation it would be expected that unpalatable rather than palatable animals would freely indulge in them, instead of the reverse.

If these displays are an important aid to fertilisation, it is difficult to explain why some species do without them, others are satisfied with a simple display, and others require an elaborate set of activities ; further, why, in some species, the male does not require the female to display, whilst in others he does ; and why some females are " coy," whilst others are not.

No theory of display can be of value unless it embraces these remarkable variations which we find on comparing one species with another. To summarise, this communication, which is itself very condensed, could not give a true impression of the thesis which has here been laid down with the object especially of bringing to the reader a line of thought which is perhaps new to him.

THE FACTORS GOVERNING THE MAMMALIAN SEX-RATIO

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In interest and importance the study of the sex-ratio merits a great deal more attention than it has received. During the last twenty years the problem has received practically no attention beyond casual mention in works on sex-determination, though previously the variation in the sex-ratio had been regarded as valuable evidence upon which to build theories of sex-determination, and the study of the two had always proceeded together. It is becoming increasingly probable, however, that the phenomena presented by the sex-ratio are only roughly connected with the facts of the determination of sex and, therefore, throw no exact light upon this problem.

In the circumstances, therefore, the best approach to the whole subject is to consider sex-determination from a genetic and cytological standpoint, and to discuss the variation in the sex-ratio in the light of the conclusion arrived at.

It is impossible to deal here with all the many theories of sex-determination, but this is unnecessary in view of the general, if qualified, acceptance of the chromosome theory. The outline of this theory is sufficiently well known to need no description, and only one or two particularly relevant points need be touched on here.

Sex-determination.—In mammals, with which we are primarily concerned, it seems that the males possess the odd chromosome, and this means that in spermatogenesis the spermatozoa are dimorphic, one type possessing the unpaired chromosome and the other being without a sex-chromosome.

Guyer has described dimorphic spermatozoa in a negro, and Von Winiwarter the same in a white man. These facts are well confirmed by the mode of inheritance of such sex-linked diseases as hæmophilia and Daltonism.

In cattle, horses, and pigs, Wadsedalek found that the spermatozoa were dimorphic, whilst the ova were all of the same type. The argument that in one type of spermatozoa there are n chromosomes and that in the other there are $n-1$

chromosomes, and that the male zygote has $2n - 1$ chromosomes while the female zygote has $2n$ chromosomes, leaves little doubt that in the cases studied by Wadsedalek, the accessory chromosome, if not actually the cause of sex, is at least invariably associated with it. In conjunction with the chromosome dimorphism of the spermatozoa, Wadsedalek found that the head lengths had a dimorphic frequency, suggesting that the extra chromatinic material of the one type increased the head size.

Malone found an unpaired chromosome in the spermatogenesis of the dog, and Zeleny and Faust have shown the spermatozoa to be dimorphic in head length.

Allen in the rat, and Yocum in the mouse, have found the spermatozoa to be of two chromosome constitutions, and in each of these animals the present writer has found dimorphism in the head lengths of the spermatozoa.

It is thus clear that there is a considerable accumulation of evidence showing that the chromosome theory is applicable to mammals. The implication of this theory is that the sex is determined at conception and that no influence acting subsequently can alter the determination.

The Sex-ratio at Conception, Birth, and Maturity.—It is clear that the chromosome theory implies that there is a sex-ratio at conception and right through fœtal life, as well as during individual life. The question thus arises as to whether the sex-ratio is constant from conception to senescence. *A priori* there is no apparent reason why the ratio should be constant through life, and such a result would, of course, be dependent upon the sexes dying equally fast, of which there is no evidence. All the facts there are point the other way, that sex-mortality is differential. In view of this, it would be of great interest to examine the sex-ratio at frequent intervals from conception to the greatest age attained by individuals ; but with the partial exception of man, sufficient data do not exist for such an undertaking. The problem is, however, brought into more manageable limits by the selection of salient points in the life-cycle and comparing the sex-ratio at these different epochs.

There are three well-marked stages in mammalian life, conception, birth, and maturity, and these form convenient milestones at which the ever-fluctuating sex-ratio may be examined. The conception-ratio, as such, is inaccessible, but, as the birth-ratio can usually be accurately determined, a consideration of the changes occurring between the secondary [1] and the primary ratio will throw some light on its general nature. It has been mentioned that in all probability sex is

[1] The terms primary, secondary, and tertiary have been respectively suggested for the sex-ratios at conception, birth, and maturity.

irrevocably determined at conception and, therefore, changes in the sex-ratio between conception and birth can only occur as the result of sex-reversal or unequal elimination of the sexes. In each case the importance of the factor is governed by two things—the amount and the sex-incidence. Sex-reversal is of negligible importance and may be dismissed from a practical standpoint. The relative elimination of the sexes, however, is of immense importance, as an example will make clear. Suppose that at birth 100 males and 100 females are found, and suppose it is known that 40 males and 10 females have been eliminated between conception and birth. Then the primary ratio will be 140 males to 110 females, which is a sex-ratio of 127·3,[1] as against the secondary ratio of 100.

The Amount of Fœtal Elimination.—First, to deal with the amount of fœtal elimination. A review of the literature dealing with the elimination of conceptuses leaves no doubt that this process goes on in two ways. The most common and generally recognised manner is, of course, abortion, but retrogression and possible reabsorption of conceptuses also seems to be a factor requiring recognition. My own experience with mice illustrates this. In some cases the most remarkable variation in the size of the conceptuses was found on dissection, and in one uterus every grade was present between a large healthy fœtus and a degenerate cyst, smaller than the apparently normal placenta to which it was attached, whilst other conceptuses had probably disappeared entirely. This disappearance cannot have been brought about by abortion, as such a process would have terminated the pregnancy entirely. There is a method of finding out roughly how many fertilised ova fail to produce normal full-time fœtuses. The number of ova liberated can be ascertained by counting the corpora lutea left in the ovary, and when a doe has been in continuous contact with a male, the great probability is that all the eggs ovulated will be fertilised. Thus by comparing the number of corpora lutea with the number of normal fœtuses, the number of fœtuses which have been eliminated can be found. In my own work on mice, in 8 does, 74 fœtuses were found the day parturition was due, and on sectioning the ovaries, 82 corpora lutea were observed. This shows a loss of 8 ova, which is a percentage of 9·7 of the presumable conceptions. In the rat Long and Evans found that the average size of litter was only 6·7, whereas the average number of eggs ovulated was 10, which shows a very high elimination rate. Hammond in a number of pigs found 267 normal fœtuses and 396 corpora lutea, a loss of 48·3 ova per 100 normal fœtuses; and 101 normal fœtuses and 116 corpora lutea in 80 ewe sheep, a loss of 14·8 per cent. In these cases

[1] Calculated throughout as males per 100 females.

of Hammond's a number of the missing conceptuses were found as atrophic remains.

Thus in the four cases in which investigation has been made the amount of fœtal elimination is considerable.

The amount of abortion is little known for animals, if we make the partial exception of farm live-stock. In man, however, the facts are better known, and a table given by Routh shows an average from seven authorities amounting to about 20 per cent. of all pregnancies. From the records of St. Mary's Hospital, Manchester, I found that the abortions from 1911 to 1920 amounted to 1,659 as compared with 8,384 births. This gives 16·5 per cent. of all pregnancies. It is clear, therefore, that the amount of fœtal elimination is large—quite sufficient to have a profound effect upon the birth-ratio if the sex-ratio of the eliminated conceptuses is at all peculiar.

The Sex-incidence of Fœtal Mortality.—With regard to the sex-ratio of fœtal retrogression, there appeared to be no data available. One of my own experiments, however, seems to throw light upon this problem. Kirkham's work with mice, on the prolonged gestation of a litter which immediately follows a preceding one, has shown that the effect of lactation concurrent with gestation is to inhibit the implantation of the blastocysts. Also, continued suckling of a first litter by a re-pregnant doe must make conditions for the litter *in utero* about as bad as possibly could be, and one would therefore expect more elimination to occur in such litters. This was actually found to be so. In does with superimposed litters the elimination of fœtuses was found to rise to as much as 23·1 per 100 normal fœtuses. In view of this greater elimination, the sex-ratio of these second litters is of great interest. In cases where the previous young had been suckled less than six days, the sex-ratio of fourteen litters was 80·5, and where the previous young had been suckled six days or more, the numbers of males per 100 females was only 62. In view of the normal equality of the sexes in mice, these results clearly show that bad conditions *in utero*, causing more eliminations, penalise the males to the greater extent, and that the sex-ratio of fœtal eliminations has a definite excess of males.

Estimates of sex-ratio of abortions vary between 250 and 101, with an average of about 150, and in no case has investigation shown an excess of females among abortions. The one or two assumptions which have been made that abortion falls most heavily on the females seem to have been manufactured for the purpose of explaining particular phenomena. As supporting the supposition of an excess of males among abortions, the well-known sex-ratio of stillbirths may be mentioned. Bodio gives figures for eleven European countries, and the

variation is between 124·6 and 142·2, a range well in keeping with the estimates of other authorities. At St. Mary's Hospital I found that in 1,358 stillbirths the sex-ratio was 133·6.

The Sex-ratio at Conception.—We have now discussed the two necessary factors for arriving at a rough estimate of the conception-ratio. With values for these factors at his command, Schultz calculated a primary ratio of about 108, which is rather lower than the calculations by other authorities, which go as high as 116. Confirmation of the hypothesis that the conception-ratio is higher than the birth-ratio is to be found in Jewell's work on the fœtal sex-ratio in cattle. This author found in 1,000 fœtuses a sex-ratio of 123·21, which is far above the estimates of the birth-ratio in cattle.

Post-natal Mortality.—Having considered the changes which take place between the birth- and conception-ratios and the consequent nature of the primary ratio, we may now consider the changes taking place between the birth- and maturity-ratios and the consequent nature of the tertiary ratio. In this case, of course, we have information as to the exact nature of the maturity-ratio in man. In other mammals little or nothing is known. Wild animals are difficult to sample representatively, and the preservation of domestic animals depends largely on selection for economic purposes. For man, however, a fairly complete study can be made. The average birth-ratio in England and Wales from 1838 to 1914 works out at 104·1 with very little fluctuation, the highest for this period being 105·0 and the lowest 103·2. This means that at birth a small but definite preponderance of males exists. To take the year 1913, the birth-ratio was 104·0, but during the first month of life the sex-ratio of infantile mortality was 129, during the second and third months 132, and the average for the first year was 125. This agrees very well with what has been said about the sex-ratio of prenatal mortality. During the first five years of life the sex-ratio of mortality is 118·4, and for individuals between five and ten years old the sex-ratio of mortality is 100·7. At ten to fifteen years of age, however, the sex-ratio of mortality is 93·3, and this is the only period during the whole of the life-cycle in which the sex-ratio of mortality is below equality, that is to say, where more females die than males. The reason is obvious. From then to the age of sixty to sixty-five, the sex-ratio of mortality shows a progressive and continuous rise and afterwards a slight decrease.

From the sex-ratio of infantile mortality it is not hard to see that the original excess of males is constantly reduced and the males soon become a minority. Thus during the first year of life the sex-ratio is 102·0 as against the birth-ratio of 104·0, and in the second year of life the ratio is 101·5. In the

third to fifth years this has decreased to 100·3, the average for the first five years of life being 101. In the second five years, from the age of five to ten years, the females have assumed the lead and the sex-ratio is reduced to 99·9. With the exception of slight rises in the sex-ratio at fifteen to twenty and at forty to fifty years of age following the slightly increased mortality of females at ten to fifteen and at forty to forty-five years of age, the preponderance of females steadily increases from the age of ten years right through life, until at the age of eighty-five and over, the sex-ratio of about 65,000 individuals is 55·2.

Having briefly considered the variation in the sex-ratio from conception to senescence, we may look in some detail at the birth-ratio, which is the only ratio of the three of which we possess material for an analysis.

The Variation in the Sex-ratio at Birth.—From what has been said about fœtal elimination it is clear that fluctuation in the secondary ratio may be the product of either of two factors, a varying amount of differential prenatal mortality or a variation of the primary ratio with an approximately constant amount of elimination. It is, therefore, interesting to consider the circumstances in which the secondary ratio does fluctuate, and to endeavour to determine the probable cause of the variation. This dual aspect of the secondary ratio will very probably be seen to account for the peculiar anomalies which have so far beset the study of the sex-ratio. For instance, it is well authenticated that the age of the mother has some influence on the sex-ratio of the offspring. This fact is in direct contradiction to the supposed sex-heterozygosity of the male, because if sex is determined by dimorphic spermatozoa, the mother is automatically excluded from any influence on sex-determination. Considering the recognised influence of the age of the mother, Goldschmidt is led to doubt the chromosome theory of sex-determination. It should be noticed, however, that the only ratio which we know to be affected by the age of the mother is the secondary ratio, whereas the facts of sex-determination and the heterozygosity of the male are concerned solely with the primary ratio. The possibility thus arises that changes between these two ratios may account for the influence of the age of the mother. Working from the records of St. Mary's Hospital, I found that the amount of abortion increases from 1·7 per 100 births in the case of mothers of thirteen to seventeen years of age, to 43·6 in the case of mothers of forty-three and over. It is evident, therefore, that the amount of fœtal elimination varies very considerably with the age of the mother and, as we have shown above, the sex-ratio of fœtal elimination is very high. Thus the fact that the secondary sex-ratio decreases with the ageing of the mother is very

probably accounted for by this varying amount of differential elimination. Thus there is no inherent incompatibility between the theory that the mammalian male is heterozygous for sex and the observed influence of the age of the mother on the birth-ratio.

Another factor which has been said to influence the birth-ratio is the parity, that is to say, the number of the pregnancy, and lowering of the sex-ratio with later births has been described for man, rats, and mice. Here again it is hard to say how influence by this factor on sex-determination can be harmonised with sex-determination by the male. From the records mentioned above, however, I found that very probably the same explanation holds good as did for the influence of the age of the mother, and that the influence is not in determination, but in elimination.

Another factor which is said to influence the birth-ratio is the relative age of the parents, but as the dozen or so authors who have investigated the subject seem to be fairly evenly divided as to what the results are, it is difficult to explain the supposed influence.

The high sex-ratio among Jews has been a source of endless controversy and various speculations have been made. It seems very probable, however, that the great care taken by pregnant mothers of this race reduces the amount of prenatal mortality, and the consequent decrease in the wastage of males before birth sufficiently accounts for the slightly higher birth-ratio which is found.

Punnett, from a consideration of the birth-ratios of different classes of people in London, came to the conclusion that, if nutrition had any effect on the determination of sex, it was in the opposite direction to that which is usually supposed. He found that in the poorest classes the sex-ratio was low, less than equality, and in the higher classes, represented by Burke's *Peerage*, the sex-ratio was found to be over 107. This is just the reverse of what would be expected from the old theory that bad nutrition produces an excess of males. These results puzzled Punnett considerably and led him to abandon the supposition that metabolism affects sex. Much the most likely interpretation of these results, however, is that the birth-ratios, as found, were in no way representative of the respective primary ratios. There can be no doubt that the amount of differential elimination between the primary and secondary ratios is infinitely more in the class showing a birth-ratio of 99 than in that where it is 107, and it is probable that the primary ratio of the ill-nourished class was not lower than that of the well-nourished, but that differential elimination had produced this result in the birth-ratio. It is possible to investigate this

point by finding a case of varying nutrition where the amount of prenatal elimination might reasonably be supposed to be about constant. In the Report of the Registrar-General for 1919 there is a graph showing the sex-ratio at birth of England and Wales since 1876, together with *The Economist* index number of the wholesale price of food. It will be seen from this that the variation is extraordinarily concurrent, that when the price of food is low the sex-ratio is also comparatively low, and that the rises are also coincident. In these cases the results cannot be due to fœtal elimination, because the bad conditions are correlated with increases in the sex-ratio, not with decrease, as would result from fœtal elimination. Thus it may be assumed that the birth-ratios expressed in this graph are more or less indicative of the conception-ratios, and that the conception-ratio therefore increases under bad conditions. This is probably the explanation of a number of experiments by Schultze, who found that excessively bad conditions over some months produced no effect on the birth-ratio in mice. What appears to have happened is that bad conditions raised the primary ratio, and that the excessive amount of fœtal elimination, consequent upon the severe conditions, afterwards reduced the ratio again to something approaching the normal.

The Origin of the High Primary Ratio.—We have seen that the birth-ratio is usually somewhat above equality and that the primary ratio is almost certainly very much more above equality. This has to be brought into line with the fact that, on chance, it would be expected that an ovum would have an equal chance of being fertilised by a Y-spermatozoon or an X-spermatozoon and that an equality-ratio would result. The fact that it does not appear to be so has been used to criticise the mechanistic conception of sex-determination, but this is really unnecessary. Granted that the spermatozoa are formed in equal numbers, it does not necessarily follow that an equal number of each type would reach the ovum. There are two ways in which the equilibrium might be disturbed. First, the spermatozoa are known to live very considerable periods, and during this time there must be an intense struggle for existence, and we may reasonably suppose that the fittest survive in the greatest majority. In this struggle for existence the possession of an extra chromosome may be a disadvantage or an advantage, and consequently one type or other may survive in greater numbers. The second way in which the original equilibrium may be disturbed is in the relative mobility of the two types. The spermatozoa always have a long way to traverse on their own account in the female genitalia, and it is known that the distance may be very great compared with the size of the spermatozoa. What happens is a race to the ovum, and the

possession of an extra chromosome may again be either a disadvantage or an advantage, thus handicapping or assisting its possessor. In many cases, as we have shown above, the head lengths of the spermatozoa vary according to the presence or absence of the extra chromosome, and owing to their minuteness the spermatozoa are possibly greatly assisted by Brownian movement, which would have much more effect in the case of the small-headed type than on those possessing long heads. The possibility of differential mobility and virility of the two types of spermatozoa accounting for the inequality of the primary ratio has been hinted at by a few authors, including Thomson, Bugnion, Doncaster, and Morgan, but as yet the experimental aspect is unexplored and presents a fascinating field for future work.

The Inheritance of the Sex-ratio.—No discussion of the sex-ratio would be at all complete without mention of the question of its inheritance.

There is some confusion of thought between the heredity of sex and the inheritance of the sex-ratio. The former really implies that sex is determined by properties inherent in the gametes, and not by extraneous circumstances. The inheritance of the sex-ratio is a rather different matter. This term implies that the proportion of the sexes in the progeny of an individual is subject to hereditary influence. This statement, however, needs to be examined a little more closely. From the nature of the dimorphism of the spermatozoa of mammals it is obvious that there must always be an hereditary tendency to produce the sexes in approximately equal numbers, so that the general nature of the sex-ratio is hereditary, just as much as the capacity for spermatogenesis is hereditary. Thus far the heredity of sex and the inheritance of the sex-ratio are synonymous.

The usual meaning of the term "the inheritance of the sex-ratio" is rather different. We have seen that, while there is a general tendency for the sex-ratio to be one of approximate equality between the sexes, absolute equality is rare. Thus the question arises as to whether, in addition to the mechanism tending to produce the equality-ratio being hereditary, the factors causing disturbances of the equality mechanism, and producing the slight divergences from equality, are also hereditary. This point usually constitutes the problem of the inheritance or non-inheritance of the sex-ratio, and should really be expressed as the problem of the inheritance or non-inheritance of the factors causing divergence from equality in the sex-ratio.

Wood, Heron, and Weldon have worked the problem out from a biometrical standpoint and have come to the conclusion

that there is no inheritance of the sex-ratio. All these authors took litters as the unit, and worked out the correlation between the sex-ratio of parental sibships and filial sibships. Starting from rather a different standpoint, I examined a large number of human genealogy tables, with the result that I found six in which the sex-ratio of the whole family of some hundred of individuals was more than 110, thus bearing out the agricultural assumption that there are strains in which one sex or other strongly predominates for generations, and which would therefore appear to indicate the inheritance in some cases of the sex-ratio. When analysed, these families showed a total of 1,001 males to 791 females and a sex-ratio of 126·6. The interesting point appeared when the male lines of these families were separated from the female lines. The sex-ratio in the male lines was found to be 142·5 and in the female lines 102·5. Now there is every reason to suppose that members of these families had, on the whole, married persons with normal sex-tendencies, and the fact that in the male lines a very high sex-ratio was found suggests that the male-bearing tendency derived from the male-bearing strains overwhelmed the normal propensity of the females married and kept the ratio high. The female lines, however, show a ratio which is practically normal, and the females cannot, therefore, have carried the abnormal tendency vested in the family as a whole. These results indicate, firstly, that the sex-ratio is in some cases inherited, and secondly, that it is inherited through the males, who must therefore be the determiners of sex. This, however, is far from postulating the universal inheritance of the exact nature of the sex-ratio, and it is most probable that the factors which have the greatest weight in moulding the sex-ratio at birth are in no way hereditary.

Conclusion.—Considering the general trend of the above discussion, it would not seem too much to say that the phenomena presented by the sex-ratio of mammals can all be explained in harmony with the chromosome theory of the sex-heterozygosity of the male. Also, the interesting generalisation may be made that the sex-ratio at any given time is a product of the proportions of the sexes at conception and the amount and sex-incidence of the ensuing mortality.

THE GROWTH AND DECAY OF COMMUNITIES

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When, a dozen years ago, I attempted to trace the successive waves of human activity, the application of such historical results to the present time seemed more than would be tolerated by modern self-satisfaction. The comparison of past periods suggested that a collapse was not far distant, but I could only hint such a conclusion, which would then have been scouted as impossible. Yet recent years have shown the weakness which underlay the prosperous appearances. The War only revealed this weakness, it did not cause it. The cause lies much deeper. For a few months after the War our national credit, and that of Central Europe, stood only a sixth above and below that of France. It was then believed that there was little difference of stability, the War was but an episode in the opinion of men of business. Four years have served to ruin Central Europe, and greatly depreciate France, solely by the inherent weakness of the present stage of civilisation. The folly of war has been much less than the follies of peace, the destruction by the policy in peace has been the more deadly.

Now that the realities of life are before all men, we may try to gather somewhat of the causes which underlie growth and decay in communities. Diseases must be diagnosed before they can be resisted. It may be the clearest way to look first at communal conditions, then at the individual character involved, and lastly at the various effects of change.

The last great cycle of civilisation in classical times is that best known, but we can draw to some extent from earlier cycles. The plainest result is that each cycle has had a similar growth, in the successive order of Art in sculpture, painting and literature, and after that in the physical expansion in mechanics, science and wealth. This constant order gives a good reason for regarding the study of it as a purely scientific

statement of phenomena, and not as a matter of merely personal impression. The history of human eclipses is as determinate as that of solar eclipses, and much more important to us.

In reviewing the rise of communities, the earliest need which compels union, is the need of defence; it is that which holds together the rudest tribes in order to resist wild animals and the aggression of other tribes. It was always the most potent bond in the past,—defence against Persia, against the Cimbri, against the Helvetii, against the Goths, against the Danes, as we can remember it in 1859 against the Austrians in Italy,—in each case it was defence which welded the community together. Does anyone suppose that without this need, the various States of our Empire would hold together for a single generation? In the decay of States, it is the habit of security, in which men forget the past necessities, which leads them to disunion. A possible foe is a welcome stimulant to a State. It is but a short-sighted view to prefer separation where any tolerable union is possible. Long-sighted Czechs say they would not have wished for separation from Austria if they had enjoyed the equality which Ireland had with England.

The next need for a rising State is subordination, to prevent trespass between its subjects. The ruin of ancient Greece was its lack of subordination, only the firmest force held it together for any purpose. The insubordination of the third century in the Roman Empire destroyed its capacity to resist the barbarians; it was only the stern measures of Aurelian and the organisation of Diocletian that gave it a century of breathing space, in which to educate the races that threatened it. The firm governing by the Normans gave each country which they dominated, the power to rise rapidly. When the habit of agreement has grown, it constitutes obedience to law, as a substitute for force. But this habit of legality leads to a loss of resource, which leaves people helpless, as the Britons were on the retreat of Rome, and as various peoples would now be, if we left them unsupported. Somewhat of this same action of law, is seen in the tame acquiescence in various encroachments of our own bureaucracy.

The extent of subordination and control has usually been stretched or abused, for (1) safety, (2) advantage of trade, (3) monopolies, or (4) for sheer plunder of goods. The last we may leave aside, as we know its futility for any permanent advantage; and monopolies, as in Madagascar, are only a mitigated form of plunder. Trade advantage is sound enough, and it is entirely right if it prevents a monopoly by others. Such are the terms on which we hold all the Crown Colonies.

The stretch of control only for the sake of safety, was the cause of growth of the Roman domination, though later abused. The hold of Rome on the Rhine or the Euphrates was solely for safety; and such is the reason of our hold on Egypt and on Southern Ireland at present.

We now turn to the more serious factors of individual character. Of these, foresight and imagination are the most essential; they stimulate protection against troubles, and provide against future difficulties. In the portraits of the two great kings who established the organisation of Egypt, Khufu and Khafra, we see expressed the most powerful foresight. In the rise of Roman power, there was always the tenacious foresight which made the people ready to suffer for the sake of their future. After the sack by the Gauls, after Cannæ, in the Italian revolt, it was foresight which gave them the tenacity that saved them. It was foresight which distinguished our greatest kings, whose work still lasts, William I, Edward III, and William III. The kind of subordination required is that of co-operating with the foresight of the ruler, while retaining to the full, individual enterprise. Any limitations which are not essential are certain to check the useful activities, as we see in the trade restrictions of the Middle Ages.

National Justice and Honesty have been an effective road to power, though usually corrupted by success. The Persians thought a lie the greatest disgrace, and being in debt was almost as bad; that was the foundation of their control of the world. The Roman entry on international power was due to their character for a disinterested love of order. They were invited to interfere with the overbearing and disorderly Greeks; after doing so, they exhorted them to common sense and decent conduct, and then retired altogether, leaving the Greek cities free, with a liberty which they abused as they always had done in the past. Polybius says, "Greek statesmen, if entrusted with a single talent, though protected by ten checking clerks, as many seals, and twice as many witnesses, yet cannot be induced to keep faith; whereas among the Romans . . . men have the handling of a great amount of money, and yet from pure respect for their oath, keep their faith intact." A couple of centuries of success altered this character, We know what a very large asset in our favour has been the reputation acquired abroad by our officials and merchants; and our last transaction with the United States has been true policy at all costs.

National unity, however essential for a rising power, has its perils after greatness is reached. Too strong a racial feeling leads to despising other nationalities, and avoiding intermarriage with them. Every great period of civilisation

has been the product of a racial mixture, though it is only some mixtures that will produce greatness. A plant will deteriorate or die off if always propagated by slips ; it needs a mixture introduced from different conditions and character. So it seems to be with races ; a new variety of mind is needed to adapt an old type to a fresh lease of life. In Egypt every great period was led by a foreign race coming into power. The prehistoric and dynastic ages show more than a dozen foreign conquests, which brought in successful races from all quarters. In Greece it was the northern Achaians that founded the classic period. Rome was incessantly invigorated by incorporating fresh races, so much so that at last the armies were entirely foreign. England from its position on the edge of a continent, and with an easier climate than the mainland, has had the immense benefit of continual mixture preventing stagnation. The Bronze Age men flocked over from the Rhine ; the Celts followed,—the Atrebatii, Belgæ, Parisii, and others were equally on both sides of the Channel. Many more races were poured in by the Romans as auxiliaries. Then the Saxon and Dano-Norman invasions covered the land. The Flemings brought in trade and ability. The religious persecutions, and, above all, the Revocation of the Edict of Nantes, drove the best brains of the West over here. Within our own memory we have benefited much by very able men finding a freer scope here than on the Continent. This recombination of abilities every few centuries has resulted in the mixed race having now occupied a large part of the globe. Each of the stocks has contributed some needed qualities. Lothrop Stoddard has remarked on the uniform physical type of English administrators, without realising that he is precisely describing the Norman, which was the most able ruling race wherever it went. Probably the physical causes are the same as underlie the strength of Japan, where certainly at least three different types are apparent. This continued reversion to the different ancestries gives an obvious advantage, as providing the variety of types which are needed for different careers.

How far will this mixture extend ? In the United States the imperceptible shading from black to white in a large mass of the people, and the introduction of a large proportion of dark south Europeans, promises to unify nearly the whole population in a few centuries, despite the bitter antagonism of the pure whites. The Dutch have a large mixed population at the Cape and in Java, like the English in India and the French in Canada. Too many of these were mixtures of the undesirables on both sides, for us to make any fair appreciation. The Scotch in Canada and New Zealand seem to have made

some successful blends with the fine type of natives of those lands. The example of South America is held out as evidence of the bad results of mixture of races which are too divergent. From this point of view it would be interesting to know of a blend of the Mongolian type of north Scotland with the northern Chinese, where there seems already some common ancestry, and yet diversity of civilisation. This might prove a favourable line for the introduction of some of the Chinese qualities which are needed here.

Having looked at the factors needful in the rise of a community, it remains to consider those which cause its decay, and which we need to study in order to avoid them so far as possible, and postpone their action. In the earliest civilisation which can be traced, reign by reign, we see in Egypt the extremely rapid rise of construction, reaching the highest perfection of accuracy, and the finest art, and then a slow decline in continual copying and cheapening of the work, until it fell into a barbaric style. This is the typical course of every civilisation, not only in Egypt, but in all countries. The decline is always gradual, almost imperceptible, until some external force collapses the hollow form.

Rome is the great example for us to study, as we know so much detail of its social state. It began its downfall by the democratic provision of half price corn to the citizen, and finally free corn from abroad. This was to favour the townsman, regardless of the countryman and his living. The first result was farming by slaves, and later the depopulation of the country, so that the richest districts of Campania, Etruria, and Lombardy were almost without inhabitants, and Rome entirely depended on the African corn fleet. The trade unions raised the price of labour, and in order to meet the difficulty of housing the poor, the builders were granted a monopoly, on condition of their doing work for the proletariate at a nominal rate, that is, at the cost of all above them. We attain the same end by doles out of taxes.

Each master had to supply an amount of cheap work in proportion to his registered capital. Therefore everyone worked with a *minimum* of capital, and this was legally met by making all the capital inalienable from the business in which it was used. Then no children, boys or girls, might leave the business, and take their share of capital out of it. Thus all trade property, and its owners, and their descendants, were tied for ever to their respective businesses. By A.D. 270 no one might leave a business, and by A.D. 369 all property was tied up to the business in which it was placed. This all arose from the dole system and compulsory trade unions.

It is little wonder that a blast of free barbarism was welcomed to sweep away this parody of civilisation.

Connected with this there was a steady depreciation of the currency, and an exactly compensatory rise of prices. This was evidently carried out by the Trade Unions, rivalling each other in advancing prices, each trying to profit at the expense of others, as railwaymen, builders, and bootmakers do now. When this course had reached the limit of the silver coin being reduced to plain copper, no further depreciation seemed possible, as printed notes did not exist. Then Diocletian put out his edict of prices at copper values, to fix the standard against further advances by the Unions. But this did not stay the plague. The Unions continued to advance prices in nominal money of account, and deemed the copper coin higher in such money of account. This system, by continually reducing debts during a couple of centuries, wiped out the ready-money capitalist, and must have thus greatly increased the rate of interest. The scramble went on (like ours of a couple of years ago), and it continued till the prices in nominal value were a million times the original amount. Germany has beaten this by a rise of a thousand million. The result was that prices began to be regulated by fractions of the gold coin, as small as $\frac{1}{720}$ of the solidus, or $\frac{1}{5}$ of a penny, but usually $\frac{1}{20}$ or $\frac{1}{30}$ of the solidus, *7d.* or *5d.* as a unit. This of course had to be paid in silver or copper at a current ratio to gold. It all sounds very modern, and if the troubles of the past had been commonly known, they might have saved the wreck of the present.

The lesson for us in this steady depreciation of currency during many centuries in Rome, is that it undermined the habits of saving and of self-denial. It taught every man to look to the Government for his dole of food, instead of relying on his own work and his savings. This reliance on others is one of the greatest causes of insecurity in a community, as it is bound to put the cost of the careless on the shoulders of the careful. If the careless were compelled to live like the careful, there might be some claim for help. But when we see that the careful man now cannot afford to marry till 30 or 40, why should the careless who marry at 20, expect doles in every form from those who deny themselves in order to meet their own obligations? The careful man now will avoid buying food which he deems extravagant; why then is he out of his self-denial to pay for education and doles for the careless who squander on luxuries? In every direction the proportions of expenditure of those who rely on help from others, is more wasteful than the expenditure of those earners who are taxed to pay this assistance. There can be but one

end to such a system, general poverty. Even the leaders of the careless are quite unabashed. They demanded a capital levy on the careful, to pay for the reduction of taxes on cinemas and beer for the careless. Now with a fine zeal they demand fifty millions for education, and propose to take it from our national defences, which do not cost a third of the 400 millions spent on drink. When those who wish to reform this world have such a sense of proportion, there is little hope for the future if such a system continues.

One of the greatest economic questions now is the return due to parents who bring up future citizens. The only proposals made are to throw the expense on taxation in various forms, of education, free meals, and costs of maternity. It is entirely disregarded that the money for this is to come from those who are struggling to raise a family without assistance. The legitimate solution would be that the wages of all men up to at least 25, should be largely banked by the Government, individually, and that out of this they should receive help for children later on. The older habit of personal saving, and gaining sufficient before marriage, having disappeared among the careless, the only rightful substitution is compulsory saving, and that should be taken from those who are to benefit by it.

In many directions we see the tendency is to those courses which have led in the past to the dissolution of communities. In each case it is the easy course. The depreciation of currency, in which all belligerent Europe has indulged, is the worst form of a capital levy. It taxes all those who have trusted to the good faith of others by lending, that is, the saving class. It adds to the profits of the manufacturer who has borrowed, instead of taxing him; it discourages all saving, and promotes waste. Every day by its fluctuations it destroys the possibility of any permanent arrangements in business. We have resisted this easy course, but there is pressed on us as a substitute, the capital levy. Here again it is to penalise the careful for the benefit of the careless. Who will save, at least in his own country, if the result of his self-denial is to be taken from him? Rather will everyone spend on themselves all that they receive. A levy would be an ignoring of debts, which would mean the stoppage of any powers of borrowing by Government, and in future having to pay very high interest to cover the risk of confiscation. The instant result would be the exportation of the fluid capital which forms the wage fund, before it was seized by the levy, and therefore the stoppage of all wages.

The idea of the State taking from one person in order to present benefits to another is fundamentally unsound. When

carried out to a large extent, as by Rome, it ruins the whole community. When we tried to follow it in the old poor law, it had similar effects. Wages went down and taxes increased so that there was no economic basis left. Our experience of a century ago showed that such was the road to ruin. It is as impossible to tamper with the economic value of labour, as it is to tamper with the value of goods. What is given from taxes comes off wages in the long run. The total of distributed products is not increased, only it is paid for by taxes on everybody, instead of payment by those who use the products. If carried out completely it would mean that everyone was a slave of the State, and prices were merely nominal. The National Workshops, and the present state of Russia, show the end of that course. It will certainly seem incredible to men in the future that we should for years go on deploring the urgent need of houses, and yet pay millions a year to builders to save them from working at the same pay as other men. If house builders did a full day's work at the same wage as shipbuilders, there would be no shortage of houses, and the community would not be maintaining them idle to no purpose.

When we look further we come on the basic question of the State and the individual, in the difference of their duties. There is a terrible antinomy which underlies much that we must decide upon. There are two different standards, and it is impossible for the State to be ruled by the same code as the individual. For instance, it is wrong for a man to take the law into his own hands by retaliation ; but it is right for a judge to compel compensation as the law directs. It is wrong for anyone to deprive a man of liberty or of life ; but it is right to do so by the agents of the law, according to law. Thus we cannot apply principles of private life to State conduct. How far is this to extend ? It cannot be allowed to place the State above all morality, to approve of the slavery of Belgium, or of Assyrian wars, or the Mongol invasions. Clearly there must be a sharp limit, but we cannot here discuss that ; our present quest is to see what are the causes of decadence in a State, and how far the State can protect itself. In this we must avoid the confusion of mixing private with public ethics.

For the welfare of the whole community, the first consideration must be the improvement of the ability of the people,—ability in all ways, of skill, of invention, of character. The training for this, is to let the results of incapacity be felt. If everyone fares equally well, no matter what their ability, there is no stimulus to improvement. Whatever alleviates the results of incapacity, is a check on the general inducement

to improve. Let us suppose, for instance, that some natural causes swept off the least capable quarter of each generation; then there would be room for four children instead of three, and assuredly four would be born to fill the vacancy. The birth rate closely keeps pace with the death rate. Thus by continual weeding the standard would be raised. The State looks to the average capacity, and not to the hardship of the individual; and from the State point of view the causes which tend to wipe out the least capable are the means of advance. The "sacredness of human life" is much more respected by raising the standard of ability in the future, than by insisting on conserving the failures of the present. Here we come up against the rightful compassion of the individual, the best instinct of helpfulness is at stake in the fate of the unfortunate. Where can the contradiction be met, how can it be reconciled? Is it not to be dealt with by letting economic law strictly protect the State for the future, and leaving private and voluntary help to deal with the individual in the present? This deals much more effectually with the work-shy, and peculiar cases.

That is not the solution which is now in fashion, the law is being continually altered to favour the incapable, to the detriment of the future welfare of the community. The endowment of unmarried mothers and their children, out of taxation of the married, in some form, is a directly increased toleration of their position and action. By all means tax, or reduce to slavery, the men who are responsible, but do not diminish the inducement to take up the legal responsibility of marriage, nor throw the burden of support upon those who keep their obligations. It is individual help, knowing all the circumstances, that can alone deal healthily with such cases. Remember that Christianity has had a hard uphill fight to lift the moral standard of the world; it has succeeded, better in some countries than in others, but only imperfectly in any place. To weaken legally the constraints that have been found needful, is a treachery to all the centuries of struggle for advance, in the past. Pity, kindness, and an alleviation of the results of wrong, are the duty of individuals, but not of the State which looks to the future. It is pleaded that the death rate of illegitimate children is much higher than that of others; very true, but that is Nature's way of weeding out incontinent stocks. The struggle for the growth of self-restraint in every way, is the struggle for the elevation of man. The diminution of the reproductive pressure has been the aim for reaching a higher mental position. Nature has worked for that by contagious diseases, and the tuberculosis resultant from that weakness. Every step that is taken against that group of

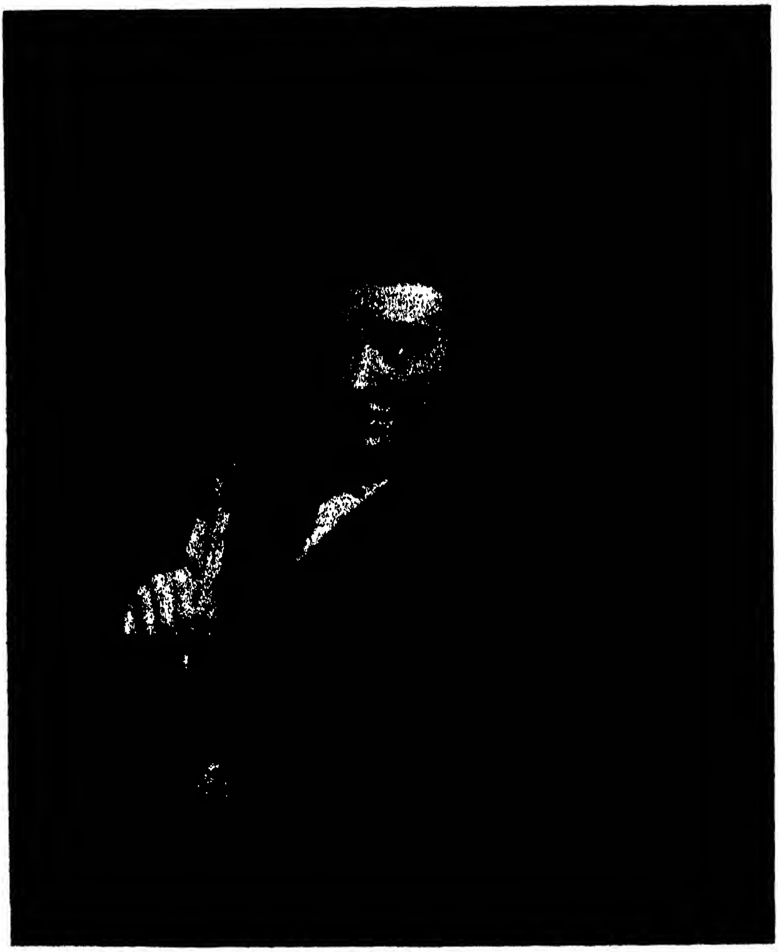
diseases is not a real gain, but a loss to the future. The great activities for the eradication of such diseases at present, will do away with one of the most useful weedings which would lift the general quality of the community. The same is true of the restrictions on drugs. The weaker and more undesirable types of mind tend to exterminate themselves by drugs. If left alone they would be beneficially weeded out, but we insist on keeping them, whether they like it or no.

Another legal injury to Nature is in the prevention of the ill effects of accident. Because a few dipsomaniacs will drink anything out of a bottle, without even smelling or tasting it, the beneficial use of carbolic acid is much restricted to the poor. More lives will be lost by not using disinfectants, than will be lost by misuse. No poisons should be interdicted, except those which might be taken unawares. In this, and other cases, let us warn, instruct, and guide in every way, publicly and privately; but the results of carelessness and incapacity must be left, to help in weeding out such faults, and so raise the individual and the race. In the vaccination difficulty, let us register all unvaccinated persons, and put the cost of smallpox hospitals and all incidental expenses upon them. Then the disease may be allowed to weed out the careless and foolish, with little loss or injury to others.

To make the world fool-proof, is to encourage the growth of fools. It is not the way that *Homo sapiens* was evolved. An extra mortality of one or two per thousand falling on the careless and foolish, will be the best possible preparation for the future of the race. The present provision of maintenance and coddling in all directions, has produced a type of mind in many workers which prevents their having the least discretion or ability to carry on their work, so that they need incessant watching to guide them. This, and the restrictions on output, have produced a state of mind, and of action, which prevents these men from being worth their keep to the community. It is this dead weight which is surely pulling down our civilisation to its end. If by the 'right to maintenance' and the 'minimum standard' we persist in burdening our capable stocks, in order to propagate the incapable, we shall insure the ultimate destruction of the capable along with the incapable.

Our ideals need to be entirely overhauled, and made to fit the needs of the future, and not be ruled by the sentimentality of the present phase. By law and custom we should encourage nothing that is not the best for the nation a century hence. If we still persist in removing all the training

process of natural consequences, we must be prepared to put something as effective in its place. It is very doubtful if we can understand the nature of what we propose to remove, and it is pretty certain that we cannot replace natural checks with any safe effect. There is no better course in disease of the person than watching and assisting Nature, and we had better remember that in dealing with the diseases of communal life.



ERASMUS DARWIN.
From a picture by Wright of Derby.

POPULAR SCIENCE

ERASMUS DARWIN

By F. W. Shurlock, B.A., B.Sc., F.Inst.P.,

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In a previous article on "The Scientific Pictures of Joseph Wright," it was pointed out that the artist was genuinely interested in physical science and that his pictures—The Orrery, The Air-pump, and The Alchymist—give an excellent idea of the stage of development which had been reached. It is probable that the instruments depicted were the property of Dr. Erasmus Darwin or of the Derby Philosophical Society, of which he was the distinguished first president.

Darwin was Wright's friend and doctor, so it is not surprising that the artist should have painted his portrait. The mezzotint engraving, by John Raphael Smith, Darwin himself thought well done, although his friend Edgeworth considered that the rather strong modelling of the brow and lips gave him a somewhat discontented expression which failed to do justice to his real benevolence and good humour. However that may be, the picture is clearly that of a man of shrewd intellect and strong character. From other sources we learn that he was above the middle stature and heavily built, that he stammered, was deeply pitted with the smallpox, and walked with the aid of a stick, being slightly lame as the result of an accident to a carriage that he had invented.

Erasmus Darwin came of a Lincolnshire family and was born at Elston, near Newark, in the year 1731. At the age of ten he was sent to Chesterfield School, whence in due course he went to St. John's College, Cambridge. At Cambridge he read classics with some mathematics. His future career seems to have been already determined, for we find him leaving Cambridge for a term to attend Hunter's lectures in London. After taking his Bachelor of Arts degree in 1754, he went to Edinburgh to study medicine, returning to Cambridge in the following year for the degree of Bachelor of Medicine. He then went again to Edinburgh to complete his studies.

In the autumn of 1756 he settled in practice in Nottingham, but remained there only two or three months, as he got no patients. In November 1756 he removed to Lichfield, and about a year afterwards married Mary Howard, who died in the year 1770. He soon acquired a practice which steadily increased, and some twelve years later was worth about a thousand pounds per annum. Although, following the honourable tradition of his profession, he did much gratuitous work among the poor, his practice lay chiefly among well-to-do people and extended over a wide area. It involved long and frequent journeys which were made in his coach.

In 1781, eleven years after the death of his first wife, he married the widow of Colonel Chandos Pole, of Radbourne Hall, whose children he had attended. On his second marriage he left Lichfield and, after living for two years at Radbourne Hall, removed to Derby and later to Breadsall Priory, where he died in 1802 in his seventy-first year.

As a youth he was fond of poetry. He was also interested in mechanics, and it is recorded that he used "to show little experiments in electricity with a rude apparatus constructed from a bottle." These tastes prevailed to the day of his death.

A man of liberal mind, his writings contain few references to political events. He seems to have sympathised with the North American colonists in their struggle for independence, and to have hailed the beginning of the French Revolution as likely to further the cause of freedom and to make France a great and prosperous example to other nations. As might be expected, his generous nature sympathised warmly with Howard's noble work of reforming the prisons throughout Europe. His real interests, however, apart from the profession in which he achieved distinction, were confined to physical and biological science, the mechanical arts, and poetry.

It was his habit to keep commonplace books in which he recorded his observations and speculations which covered a very wide range. One of these was devoted to sketches of machines and suggestions for their improvement. His carriage was fitted with conveniences for reading and writing, of which he did a great deal during his professional journeys.

Dr. Darwin became a Fellow of the Royal Society in 1761, and his papers to the *Philosophical Transactions* indicate the wide range of his scientific activities. The first of these—"Remarks on the Opinion of Henry Eeles, Esq., concerning the Ascent of Vapour"—was written at Lichfield in 1757. In this he controverts the view "that every particle of vapour is endued with a portion of electric fire; and there is no other sufficient cause assigned for their ascending." He describes

a number of simple experiments in heat and electricity, and concludes that "though clouds may sometimes possess an accumulation of electricity, yet that this is only an accidental circumstance; and thence can have no possible influence either in the elevation or support of them."

The paper "Experiments on Animal Fluids in the Exhausted Receiver," published in 1774, is interesting as showing that Dr. Darwin was in the habit of experimenting with the air-pump. Blood obtained by cupping, ligatured sections of blood-vessels, and certain animal organs were examined under the receiver of an air-pump to show that there are no grounds for holding "that an elastic vapour of some kind exists in the blood-vessels." The last of the Lichfield papers, which did not appear until 1788, was on "A New Case of Squinting."

The paper "Of an Artificial Spring of Water," published in 1785, after his removal to Derby, describes the way in which Dr. Darwin obtained a good supply of water by deepening a disused well near his house, and shows that he had grasped the principle of the artesian well.

The paper "Frigorific Experiments on the Mechanical Expansion of Air" appeared in 1788. In it experiments are described, which had been made some years previously, in which air, compressed in an air-gun, was allowed to cool for an hour before discharge, and then to impinge in a continuous stream on the bulb of a thermometer, which showed a lowering of temperature of about two degrees. Experiments were also made with a thermometer in the receiver of an air-pump, with a like result. Lastly a hole about the size of a crow-quill was bored into a large leaden air-vessel, placed at the commencement of the principal pipe in the water-works which then supplied the town of Derby. The water from four pumps, worked by a water-wheel, was first entered the lower part of the air-vessel, and thence rose to the top of St. Michael's church into a reservoir, about thirty-five or forty feet above the level of the air-vessel. Two mercury thermometers were previously suspended on the air-vessel, that they might become of the same temperature with it. As soon as the hole was opened, the stream of air was allowed to impinge on the bulbs of the thermometers, which indicated a lowering of temperature of four degrees. The conclusion is reached that, "in all circumstances when air is mechanically expanded, it becomes capable of attracting the fluid matter of heat from other bodies in contact with it," and some meteorological consequences of this are discussed. The physical papers reveal Dr. Darwin as a natural philosopher, with the habit of experimenting, fully versed in the physical science of his day, conversant with the literature and maintaining close relations with other workers. They serve also

to remind us of the relatively rudimentary condition of some important branches of physics at this period.

Dr. Darwin gained a high reputation in his day as an author both in prose and verse. Shortly after the death of his first wife he commenced his important work *Zoönomia, or the Laws of Organic Life*, which, however, was not published until 1794. It is dedicated "To the Candid and Ingenious Members of the College of Physicians, of the Royal Philosophical Society, of the Two Universities, and to all those who study the Operation of the Mind as a Science, or who practice Medicine as a Profession," and the preface explains that it is an endeavour to reduce the facts of animal life into classes, orders, genera, and species, and by comparing them with each other, to unravel the theory of diseases. In the first part of this work the author treats of physiology and pathology. The second part is a treatise on medicine, whilst the third deals with materia medica. The *Zoönomia* was translated into German, French, and Italian, and was placed by the Pope on the "Index Expurgatorius."

The scope of the *Phytologia, or the Philosophy of Agriculture, with the Theory of Draining Morasses and with an Improved Construction of the Drill Plough*, which was published in 1800, is sufficiently indicated by its title. Whilst dealing primarily with biological science and its applications to agriculture and sanitation, the physical sciences and the mechanical arts are by no means neglected. One of the sections treats of light, heat, and electricity; the influence of electricity in forwarding the germination of plants is discussed, and an account is given of Bennet's electric doubler; there is also a description of the drill plough and of a machine for raising water.

A *Plan for the Conduct of Female Education in Boarding Schools*, issued in 1797, contains much sound advice. It is interesting to note that Dr. Darwin advocated the teaching of natural history, botany, and chemistry. He goes on to say that "an outline of the sciences, to which mathematics have generally been applied, as of astronomy, mechanics, hydrostatics, and optics, with the curious addition of electricity and magnetism, may best be acquired by attending the lectures in experimental philosophy, which are occasionally exhibited by itinerant philosophers." This makes it clear that but slender provision was made for teaching the physical sciences at this time. Visits to factories are recommended for the summer vacations.

Some brief account of the poetical works must be given. A poem *On the Death of Prince Frederick*, written whilst at Cambridge, was published many years afterwards, and others appeared from time to time. During the early years of his practice in Lichfield Dr. Darwin seems to have written but

little poetry, but whilst there he purchased some eight acres of land which he converted into a botanic garden. This led him to write his poem *The Botanic Garden*, which is in two parts. Part I, "The Economy of Vegetation," treats of the physiology of plants, and Part II, "The Loves of the Plants," explains the system of Linnæus. Of these the second part was published in 1788, Part I following two years later. The poem is a serious treatise on botany written in imaginative verse. It is written in ten-syllable rhymed couplets and was an artistic and financial success. The poem is accompanied by voluminous "philosophical notes," which form a scientific commentary and furnish an admirable review of the state of science in the eighteenth century. The "Arguments" of the cantos read like the indexes of current numbers of the *Philosophical Transactions*. In the poem itself there are passages relating to the physical sciences and their applications: thus there are descriptions of electrostatic experiments, including those of Franklin, references to the work of Torricelli and Boyle, and a description of the steam-engine. As an example of his poetry and as illustrating his prophetic instinct in matters of science, the following lines may be quoted:

> "Soon shall thy arm, unconquered steam! afar
> Drag the slow barge, or drive the rapid car;
> Or on wide-waving wings expanded bear
> The flying chariot through the fields of air.
> Fair crews triumphant, leaning from above,
> Shall wave their fluttering kerchiefs as they move;
> Or warrior bands alarm the gaping crowd,
> And armies shrink beneath the shadowy cloud."

A note adds that, "as the specific levity of air is too great for the support of great burthens by balloons, there seems no probable method of flying conveniently but by the power of steam, *or some other explosive material*,[1] which another half-century may probably discover."

The Temple of Nature, or the Origin of Society: A Poem with Philosophical Notes was published in 1803, the year following the death of the author. It is in four cantos, and the additional notes cover a wide range. The poem deals with the origin and reproduction of life, the development of the mind, the origin of language and writing, and the problem of good and evil.

Dr. Darwin achieved fame, but perhaps not lasting fame, as a poet. His views on poetry are set forth in three "Interludes" in prose dialogue in *The Botanic Garden*. He appears to have thought that poetry should chiefly confine itself to the word-painting of visible objects, and his aim was to enlist poetry in the service of science. As there can be no finality

[1] The italics are inserted.

on the presentation of science, it is hardly surprising that the poems are now little read.

Dr. Darwin constantly exercised his mind on the problems which were investigated by his grandson, Charles Darwin, and whatever may be the ultimate judgment on his poetry, his works will doubtless remain a landmark in the history of biological science. Dr. Krause, a competent authority, states that "almost every single work of the younger Darwin may be paralleled by at least a chapter in the works of his ancestor—the mystery of heredity, adaptation, the protective arrangements of animals and plants, sexual selection, insectivorous plants, and the analysis of the emotions and sociological impulses; nay, even the studies on infants are to be found already discussed in the writings of the elder Darwin." The detailed investigation and definite establishment of principles is, of course, the work of his illustrious grandson. The conclusion is inevitable that to Erasmus Darwin his grandsons, Charles Darwin and Francis Galton, as well as other members of this remarkable family, are largely indebted for their scientific bent and for their interest in scientific pursuits.

Erasmus Darwin was obviously a man of active and original mind, great powers of observation, great industry, great versatility, and with a strong tendency to generalisation and the framing of theories. The latter he justifies in the following passage: "Extravagant theories, however, in those parts of philosophy, where our knowledge is yet imperfect, are not without their use; as they encourage the execution of laborious experiments, or the investigation of ingenious deductions to confirm or refute them." He was, as we have seen, equally alive to the value of experiments, and we may quote in this connection his dictum that a fool is a man who never made an experiment in his life.

As to the principles that should guide conduct, Dr. Darwin considered that the communication of happiness and the relief of misery was the only standard of moral merit, and held that "the sacred maxims of the author of Christianity, 'Do as you would be done by' and 'Love your neighbour as yourself,' include all our duties of benevolence and morality; and, if sincerely obeyed by all nations, would a thousandfold multiply the present happiness of mankind"—a message that is not without its lesson for our own epoch. A hereditary tendency to gout made him a staunch advocate of temperance, and he is said to have materially reduced the amount of drinking amongst his patients. Another personal characteristic was his capacity for making friends with good and able men—among them Keir, Edgeworth, Boulton, Watt, and Wedgwood—and for retaining their friendship.

A man of great energy, he endeavoured to incite others to works of mercy and to the study of science. In 1784, anticipating an outbreak of smallpox in Derby, he proposed the establishment of a society and the hire of a room as a dispensary where the medical men of the town might give their services gratuitously. There was then no public institution in the town for the relief of the poor in sickness, and in the circular which he drew up the hope is expressed that the dispensary may prove the foundation-stone of a future infirmary, a hope that has since been magnificently realised.

Whilst at Lichfield Dr. Darwin started a botanical society, but found few kindred spirits, as Mr. Brooke Boothby, afterwards Sir Brooke Boothby, Bart., who shared Dr. Darwin's poetical and scientific tastes and Mr. Jackson appear to have been the only other members. Communications were sent in its name to magazines, and it published a translation of the *Genera Plantarum* of Linnæus for which Dr. Darwin was mainly responsible.

On his removal to Derby he founded the Derby Philosophical Society, which held its first regular meeting on July 18th, 1784, when he delivered an interesting presidential address. The society purchased books of natural history and philosophy in English, French, and Latin which were circulated to its members. Among its non-resident members were the two members of the Lichfield society and Dr. Robert Waring Darwin, of Shrewsbury, the father of Charles Darwin. The original rules, a reprint of Dr. Darwin's first presidential address, and two early manuscript books, one containing a catalogue of books with records of their issue and the other the accounts of the society, are in the Derby Public Library. Possibly others are in existence, and it is very desirable that they should be traced. Besides the library, the society owned mathematical and philosophical apparatus, fossils and other specimens. It was in existence in 1857. Occasional scientific lectures were given in the theatre under its auspices. Mr. George Spencer, the father of Herbert Spencer, was at one time secretary of the society, and Herbert Spencer did some work in connection with it.

There is a bust of Erasmus Darwin, with copies of his principal works and his address to the society, in the Derby Public Library, and his signature appears twice in the account-book to which previous reference has been made. His microscope is in the Derby Museum and his memorial tablet in Breadsall Church.

NOTES

Independent *versus* Subsidised Research (R. Ross).

On the 9th of October I delivered an address at the Anderson College of Medicine, Glasgow, on the Management of Medical Research. It was published in full in *Nature* four days later, but I should like to add a few comments here. I chiefly laboured the contrast between *independent* and *subsidised* research—a theme which is now beginning to attract some attention. Apparently all the older philosophers and mathematicians were *independent* workers who lived (when they were not poisoned, crucified, burnt, or imprisoned) on patronage, teaching, medical practice, or astrology. The first case of *subsidised* research was, I hear, that of Aristotle when Alexander gave him a grant for the collection of marine animals; but Tycho Brahé's Uraniborg (1576) seems to have been the first subsidised "research institute." Last century many such (the Pasteur and the Jenner Institutes, and numbers of observatories and chemical institutes) were founded; and I estimate, or rather conjecture, that the world must now be laying out quite a million pounds a year for subsidised investigations in these institutions, in universities, and for research scholarships, expeditions, etc. At the same time the world (which has much more piety than science) is subscribing ten times this amount for "Protestant Foreign Missions" alone—namely an average of £9,594,254 a year. I estimate that in this country medical subsidised research alone must be now receiving about a quarter of a million a year—though Britain gives nearly ten times as much, namely, £2,310,000, to the fortunate Protestant Missions.

Of course almost all this quarter of a million goes to laboratories and full-time or part-time paid investigators, and the independent workers seem to share but little in the bounty. Yet, not only has almost the whole of medical history been built up by the independent workers—Harvey, Jenner, Pasteur, Lister, Koch, and scores of others—but, even in these days of laboratories, they have been known to achieve occasional results of some small value. Thus during the last quarter of a century medicine has advanced chiefly in the domain of tropical diseases; and here almost all the initial work was done by private and unpaid investigators, though paid commissions often consolidated the discoveries afterwards—and sometimes wasted much money in the process, or even impeded progress.

I know that the cost of the initial advances in tropical medicine to the public (at least) was almost zero, but can speak with full certainty, for an example, only regarding my own work on malaria and mosquitoes—a history of which will be found in my *Memoirs* (Murray). All my preliminary studies up to the key-observation of the 20th of August 1897 did not cost the Government of India (whose officer I was) one farthing. Subsequently it put me on special duty for a year at a salary increased by 200 rupees a month and gave me the help of some native assistants; and the whole cost of this to Government was just about £240. For this sum, then, the world ascertained how the most widespread of tropical diseases is carried and may be prevented (the same work cost me personally almost exactly £500). The early work of Manson, Bruce, Leishman, and others was probably equally inexpensive.

Compare with all this the quarter of a million a year which I gather we are now spending on subsidised medical researches, and we shall be able to form some estimate of the costs of subsidised and independent researches respectively.

Of course I am not in any way opposed to subsidised researches—let the world spend as much as it can upon them; but I think that it should also pay something—say 5 per cent. or 10 per cent. of its available funds—to encourage the very cheap and yet very successful independent researches. These, except along clinical lines, are now seen to be generally ruinous for the men who have been so foolish as to undertake them; and the result may be imagined. The best scheme, costing about £20,000 a year—much less than is now largely wasted on junior "research scholarships"—is that which was laid down by the British Science Guild some years ago (SCIENCE PROGRESS, vol. xiv, page 637, 1920). It really amounts to a scheme of research professorships for life and is much needed. Without it the world often loses the services of its most approved investigators—while larger sums are being spent on hiring untried and inexperienced young men just out of the schools.

The following passage in my Address requires special attention:

"What always amazes me is the fact that there are millions upon millions of human beings whose health and whose very existences are constantly threatened by numbers of diseases, and yet who never subscribe one farthing for the medical researches which endeavour to defeat these terrible enemies of theirs, and often succeed in doing so. Yet thousands of these same people pour out their subscriptions and bequests for all kinds of projects, many of which are futile; while even those good and generous people who maintain our hospitals and universities seem often to forget that behind hospital practice and behind university teaching there is the great science which inspires both."

"Chousing the Doctor."

The dispute regarding the national payment of panel doctors again opens an old trouble. We once heard an American maintain that the treatment of its doctors is an index of a nation's state of civilisation, and that nowhere are they so honourably regarded as in the States. The medical profession have always considered themselves bound by a rule of honour that they must treat the sick whether they be paid or not for doing so; and in some cases physicians' fees cannot be recovered at law. Hence on the part of the (civilised) public there arises in equity an equal obligation of honour to pay its doctors. Seldom if ever do doctors refuse to attend the seriously sick at any hour of day or night; but we fear that the tacit bargain is not so well kept on the other side; and many practitioners admit that from a quarter to a third of their proper fees are bad debts. The same spirit is beginning to pervade the field of medical research, and we have actually heard politicians assume that it is the *duty* of doctors to investigate the causes and treatment of disease during their leisure from clinical practice! At the same time, in this country at least, the politicians have officially refused to pay anything for actual discoveries which have resulted from these investigations, which they assert the doctors should engage in at their own cost. About the same time the Royal Commission on Awards to Inventors refused to consider medical cases at all because, it argued, doctors did such work for nothing! All such instances are contrary to natural justice, according to which those who receive benefits from professional men should pay for them if possible. They are cases of what is called "chousing the doctor."

The same doctrine is beginning to creep into the affairs of general science. Thus on the 16th of October last, the Lord President of the Council asserted

with satisfaction that many men of science would work for Government at less remuneration than similar private work would receive. In other words, Government was exploiting these gentlemen's patriotism. As *Nature* observes (October 27), "rarely are the mercenary advantages of the one-sided arrangement so baldly claimed by a responsible minister." A later letter in *Nature* scarcely mends matters.

For centuries past the public has received not only gratuitous medical treatment for its poor, but also, what is more important, gratuitous medical discovery. Naturally it is beginning to ask why it should pay for any medical work at all. "Praise the altruism of the doctors and let them starve" is becoming a kind of national motto. Patients who roll up in their motor-cars grumble at a two-guinea fee or forget their doctor's bill for years. So now, when the nation has established an unsatisfactory medical insurance scheme, it attempts to have the work done on sweated payments controlled by rich but interested societies.

The profession itself is largely to blame for this. For years many of them have been saying with the Pharisee, "God, I thank Thee, that I am not as other men are, extortioners, unjust. . . . I give tithes of all I possess." So they do: they advise Government for nothing, and they sit nobly on managing committees. But one generally finds, on inquiry, that they have been drawing comfortable life-long professorial salaries which they obtained when they were young in consequence of a little second-rate scientific work; and that they have a wonderful scent for honours and new lucrative posts. For instance, one of these gentlemen, who scorned any form of payment for his scientific discoveries, boasted immediately afterwards that he received fees from British Colonies at the rate of £1,000 a month.

There is only one sound principle for dealing with scientific and medical benefits, namely, that which is employed for all other kinds of effort—honest payment for honest work done. All professional services should be paid for under all circumstances, and at a rate proportioned to their value. Any other system is always one of "graft," with ulterior considerations in the background.

The Absurd Theory of Evolution.

For some time past efforts have been made in the United States to suppress the teaching of evolution in schools and universities on the ground that the theory is unsound. The next thing will be that the sun and the stars will begin to circle round the earth as they did before Copernicus. To those who believe that men are merely the larvæ of angels everything is possible except reality. We were therefore very glad to see a little corrective which Dr. W. Bateson, F.R.S., administered in *Nature* of September 1, 1923. He feels that it was he who, though unwittingly, dropped the spark which started the fire, and he takes the opportunity to correct those who had listened to him but apparently had not understood him. "Though no one doubts the truth of evolution," he says, "we have as yet no satisfactory account of that particular part of the theory which is concerned with the origin of *species* in the strict sense. The purpose of my address was to urge my colleagues to bear this part of the problem constantly in mind. . . . When such confessions are made, the enemies of science see their chance. If we cannot declare here and now how species arose, they will obligingly offer us the solutions with which obscurantism is satisfied. Let us then proclaim in precise and unmistakable language that our faith in evolution is unshaken. Every available line of argument converges on this inevitable conclusion. The obscurantist has nothing to suggest which is worth a moment's attention." We learn from Dr. Bateson that "Mr. William Jennings Bryan, with a profound knowledge of the electoral heart, saw

that something could be made " of this anti-evolution stunt. In Kentucky a Bill for suppressing all evolutionary teaching passed the House of Representatives and was only rejected, apparently, by one vote in the Senate. Arkansas and Oklahoma have distinguished themselves by similar folly, while Florida has forbidden any instructor to teach any hypothesis " that links man in blood relation to any form of life." The Americans must be a very superior race indeed and entirely different from the remainder of humanity !

Poems of Science.

In the Introduction to his *Poems of Science, Pages of Indian Earth History* (Erskine Macdonald, London, 1923) Mr. K. A. Knight Hallowes, M.A., A.R.S.M., F.G.S., of the Indian Geological Survey, quotes Wordsworth as having remarked that " the remotest discoveries of the chemist, the botanist, or mineralogist will be as proper objects of the poet's art as any upon which it can be employed." This dictum must come as a shock to the many literary men who have considered that science and poetry, though they may be sisters, are always quarrelling. And Mr. Hallowes gives some more quotations in the same sense. Of course the passion for discovery, which is the noblest passion of the human mind, is a prime subject for poetry; but there are very few people indeed who have ever really experienced it, and still fewer of those who have experienced it possess the capacity to commit poetry regarding it. But, as Wordsworth implied, the more flowery walks of science are often suitable for the sister of science to stroll in—on fine mornings. Mr. Hallowes's book contains, not only an interesting introduction in which he discusses this theme, but also twenty excellent sonnets describing his thoughts and emotions while he was engaged in the Indian Geological Survey. To him the gigantic evolution of the earth is a subject for passionate study. He sees not only the mountain and the river, and the flowers and forests between them, but also the past history of the earth; and he triumphs, not only with the triumph of nature, but also with the triumph of the human intellect. That is fine and proper in days when many people seem to think that the human intellect is merely despicable. We gather that the sonnets are only selections from among his literary efforts. They are all euphonious, grave, and calm, and well describe such landscapes as most frequently must impress themselves upon the geologist in India. Any one of them will suffice as an example of his work, and we therefore give the first one :

Where Man has never or but seldom trod,
Among wild mountains and their forest glooms,
'Mid tangled boughs and intertwining blooms,
Hushed as the haunt of some Greek sylvan god,
I trace the brook, unknown to angler's rod,
Which the deep valley crowned with crags entombs,
And envy yon kingfisher's rapid plumes,
As slowly through the river-sand I plod.
With clutching hands these waters for an age
Have drawn aside the shrouding cloak of soil,
So that I read, as from an open page,
How lavas here did once solidify
With clouds of steam, to form this world of toil—
This planet where we mortals live and die !

In *Circe's Worshippers and other Poems*, by Frank Finn, B.A., Z.S., Guide Demonstrator, Horniman Museum (Selwyn & Blount, London, 1923, price 1s.),

the author gives us sixteen very capable little lyrics chiefly on birds. Mr. Finn possesses both invention and technical skill—a rare combination. The two following clever word-pictures illustrate his style:

SIR CHANTICLEER

Of flame and shining steel
I stride in rich array;
With daggers on each heel
Armed for my life-long fray,
My sanguine crest I rear
Aloft for all to see,
As fits a cavalier
And squire of dames like me.

FLYING-FOXES

We swarm across the evening sky,
But not in flocks, as wildfowl fly,
For each grim vampire singly swings
With smiting sweeps his wide-webbed wings
Behind black muzzle keen with greed
On Indian peasants' fruit to feed.
And when again at dawn we come
To the tall trees we call our home,
Each to himself a branch would have;
And so we snarl and scratch and rave
Till slumber overcomes our spite
And limp we hang where last we light.
Folly in this you mortals see,
Though you would fight for all the tree!

Goitre.

Medical science often suddenly expands in the most unexpected directions. One of the important endemic diseases is goitre, which is connected with cretinism on the one hand and with myxœdema on the other. As everyone knows, it was common in parts of Switzerland, India, and elsewhere, and caused not only considerable deformity, but much mental degeneration. All sorts of theories as to the cause of it have been prevalent; but now we apparently owe to the Americans the simplest possible method of prevention and also, in some cases, of cure, namely by minute doses of iodine —see *The Prevention of Simple Goitre*, by David Marina, C. H. Lenhart, O. P. Kemball, and J. M. Rogoff, compiled and edited by G. N. Stewart, Director of the H.K. Cushing Laboratory and Experimental Medical Western Reserve University, 10940 Euclid Avenue, Cleveland, Ohio, U.S.A. This pamphlet is a collection of separate studies, but we hope that the authors will bring out a little book on the subject without so much repetition and overlapping. The prophylactic treatment as carried out for the past three years in certain schools consists only of the administration of 0·2 gm. of sodium iodide in daily doses for ten consecutive days, repeated each spring and autumn. Schoolgirls are much more prone to affections of the thyroid gland than are schoolboys. Large numbers of children have been and are being treated.

It is interesting to hear that the ancient Greeks treated goitre with the ash of certain seaweeds—which contain iodine. This is an example of the fact that popular knowledge on medical treatment is sometimes far in advance of scientific knowledge.

Notes and News.

The Nobel Prize for medicine for 1922 is to be divided between Prof. A. V. Hill, of University College, London, and Prof. O. Meyerhof; the prize for 1923 is divided between Dr. Banting and Prof. Macleod, of Toronto. The physics prize for 1923 has been awarded to Prof. R. A. Millikan, of Pasadena, California.

The Royal Medals awarded by the Royal Society have this year, with the approval of the King, been given to Sir Napier Shaw for his researches in meteorology, and to Prof. C. J. Martin for his work on animal metabolism.

The other medals of the Society have been awarded as follows: The Copley Medal to Prof. H. Lamb, for his researches in mathematical physics; the Davy Medal to Prof. H. B. Baker, for his researches on the complete dryness of gases and liquids; the Hughes Medal to Prof. R. A. Millikan, for his determination of the electronic charge and of other physical constants.

The chief officers of the Society for the session 1923–4 are:—*President:* Sir Charles Sherrington; *Treasurer:* Sir David Prain; *Secretaries:* Mr. W. B. Hardy and Mr. J. H. Jeans; *Foreign Secretary:* Sir Arthur Schuster.

Prof. F. O. Bower has been re-elected President of the Royal Society of Edinburgh for the year 1923–4.

Dr. Andrew Balfour has been appointed Director of the new School of Hygiene which is to be established in London as a result of a two-million-dollar gift from the Rockefeller Foundation. Mr. C. M. Wenyon will succeed him as Director of the Wellcome Bureau of Scientific Research.

The first award of the Priestley Medal of the American Chemical Society has been made to Dr. Ira Remsen, Emeritus Professor of Johns Hopkins University.

Sir Robert Hadfield has received the first award of the Thomas Turner gold medal of the Birmingham University Metallurgical Society.

Mr. G. E. Duveen has given £10,000 to the University of London for the establishment of a lectureship in otology. The lecturership will be held at the University College Hospital Medical School.

Among the names of the workers in the science cause whose deaths have been announced during the last quarter we have noted the following: Mr. H. Ayrton; Dr. E. F. Bashford, pathologist; Dr. J. A. Harker, O.B.E., F.R.S., physicist; Sir H. H. Hayden, F.R.S., lately Director of the Geological Survey of India; Mr. F. J. N. Jenkinson, Cambridge University Librarian, dipterist; Mr. E. K. Muspratt, vice-chairman of the United Alkali Co.; Dr. H. McLeod, F.R.S., most widely known as designer of the McLeod vacuum gauge; Lord Morley, O.M., F.R.S.; Dr. A. A. Rambaut, F.R.S., of the Observatory, Oxford; Hon. N. C. Rothschild; Dr. J. E. Stead, F.R.S., President of the Iron and Steel Institute; Prof. James Sully, Emeritus Professor of philosophy, University College, London; Prof. J. Violle, physicist, best known for his work with Vautier on the velocity of sound.

The meeting of the British Association at Liverpool this year was one of the most successful on record. At least 15,000 people attended the free public lectures, and more than 7,000 paid for admission to the Scientific Exhibition held in the Central Technical School. The presidential address delivered by Prof. Rutherford was transmitted by wireless and was heard as far away as Switzerland. This address and those of the presidents of the several sections is published, as is now customary, by John Murray (*The Advancement of Science* 1923; price 6*s*. net). During the course of the meeting the Council of the Association unanimously nominated Major-General Sir David Bruce to fill the office of President of the Association for the year 1924–5, when the meeting will be held at Toronto.

The Western Galleries of the Science Museum at South Kensington were closed on September 17 and the collections contained therein will in due course be removed to the new buildings opposite the Royal College of Science.

The galleries containing the Colonial Collections of the Imperial Institute are also, it would appear, to be closed on the ground of economy. These collections are to be dispersed and replaced by the War Museum Collection from the Crystal Palace. It is not clear whether this disgraceful decision is final. The Colonial Collection is unique and of great educational and commercial value; the value of the War Museum as a factor in the future development and prosperity of the country would appear to be precisely zero. The view-point of the people who proposed to supplant the one by the other is simply inexplicable.

The new magnetic laboratory rendered necessary by the proposed electrification of the railways near Greenwich is to be erected on private land near Abinger Bottom. The site is protected from interference by building operations by Abinger and Wotton commons. The opposition to the original scheme, which involved the enclosure of public common land on Holmbury Hill, has thus been successful. It appears, too, that equal success will reward the opposition to the erection of a wireless station at Avebury, but there is no such satisfactory information regarding the proposal to retain the Tank Gunnery School at Lulworth Cove.

An important scientific exhibit will be included in the British Empire Exhibition at Wembley next summer. The Royal Society will co-operate in the organisation, and the arrangements for the chemical section are already well advanced. Among those who will be responsible for the various sub-sections of this part of the exhibit are Sir Ernest Rutherford, Prof. McLennan, Prof. Donnan, Sir John Russell, and Mr. C. F. Cross. No information regarding the arrangements for other branches of Science are available at the time of writing.

The legacy of one million francs left by the late Prince Albert of Monaco to the French Academy of Medicine is to be used for a prize of 120,000 francs, which will be awarded every two years for certain kinds of medical service or discoveries. The council of the Academy has not yet decided whether the prize shall be an international one.

The Director of the Department of Terrestrial Magnetism at the Carnegie Institute, Washington, Dr. L. A. Bauer, gave an interesting account of preliminary results of an analysis of the earth's magnetic field which is being made in his department, at the meeting of the American Philosophical Society, Philadelphia, last April. It appears that about 94 per cent. of the earth's field arises from magnetic and electric forces inside the earth, about 3 per cent. to an electric system in the atmosphere, and the remainder to a system equivalent in its effects to electric currents passing perpendicularly through the earth's surface. For the year 1922 the magnetic axis for the earth-field was inclined at $11 \cdot 5^\circ$ to the axis of rotation, and the magnetic moment was $8 \cdot 05 \times 10^{25}$ unit pole-cm. Gauss's analysis for 1830 gave this magnetic moment as $8 \cdot 55 \times 10^{25}$, so that the earth's intensity of magnetisation has been diminishing during the last eighty years at an average rate of 1/1,500th part per annum. No explanation of this rather remarkable decrease is available. Further details regarding the results which have been deduced will be found in *Terrestrial Magnetism and Atmospheric Electricity* (March, June, September 1923).

Papers by Prof. St. John and by Dr. Evershed show that both these observers are now of opinion that the solar spectral lines show a displacement towards the red end of the spectrum, as predicted by Einstein. According to *Science* (September 28, p. x), St. John has calculated that the Einstein displacement of the lines amounts to 86 per cent. of the total observed effect, "the remainder being due to other well-known effects resulting from the conditions in the solar and terrestrial atmospheres and to the sun's rotation on its axis." With this conclusion the three effects predicted by Einstein as a result of his theories of relativity may be taken as having been verified by actual observation.

An appeal is being made to the public for donations towards a fund of £50,000 for the foundation of a Ross Institute for research on tropical diseases. The appeal is signed by many of the most eminent persons of the day, including Sir Charles Sherrington, Sir Humphry Davy Rolleston, and Sir Anthony Bowlby; Prof. Roux (Director of the Pasteur Institute, Paris); Prof. J. Bordet, N.L. (Director of Pasteur Institute, Brussels); Surgeon-General Cumming (Director-General, Public Health Service, U.S.A.); Rt. Hon. H. H. Asquith; the Marquess of Lansdowne; Lord Hardinge of Penshurst, and very many others. Lord Leverhulme is acting as President of the Institute, and Lord Willoughby de Broke as Hon. Treasurer, to whom subscriptions should be sent at the offices of the Institute, 56, Queen Anne Street, W.1. In their appeal the Committee point out that "France has her Pasteur Institute, in memory of the great scientist and to carry on his work; America has her Gorgas Institute at Panama, and Japan her Kitasato Institute; and it is strongly felt that Great Britain should similarly honour one of her greatest investigators." Since Sir Ronald Ross made his great discovery on August 20, 1897, after much distressing toil and many, many failures, the number of lives which have been saved must run into several millions; in a letter to *Nature* (September 29) Dr. Malcolm Watson estimates that over 100,000 lives have been saved in Malaya alone. How Major Ronald Ross made his discovery is told by Wilfred Partington, the Secretary of the Institute, in these words:

"On August 16 Ross captured an *Anopheles* mosquito on the wall of his room—a different type to those on which he had been working. Strangely enough, later the same day, one of his collectors brought him a bottle of about a dozen of the same type. The round of dissections, comparisons, and tabulations began again, but, under the microscopic examination, they all yielded negative results until the last of the batch remained. The dramatic finale of the long quest shall be told in the discoverer's own words:

"'The dissection was excellent, and I went carefully through the tissues, now so familiar to me, searching every micron with the same passion and care as one would search some vast ruined palace for a little hidden treasure. Nothing. No, these new mosquitoes also were going to be a failure: there was something wrong with the theory. But the stomach tissue still remained to be examined—lying there, empty and flaccid, before me on the glass slide, a great white expanse of cells like a large courtyard of flagstones, each one of which must be scrutinised—half an hour's labour at least. I was tired, and what was the use? I must have examined the stomachs of a thousand mosquitoes by this time. But the Angel of Fate fortunately laid his hand on my head; and I saw a clear and almost perfectly circular outline before me of about 12 microns in diameter. The outline was much too sharp, the cell too small to be an ordinary stomach-cell of a mosquito. I looked a little further. Here was another and another exactly similar cell. The afternoon was hot and overcast; and I remember opening the diaphragm of the sub-stage condenser of the microscope to admit more light and then changing the focus. *In each of these cells there was a cluster of small granules, black as jet. . . .*'[1]

"It was the malarial pigment. The next day the cells had grown larger; they were the malarial parasites, and it was not long before their life-cycle from the stomach to the proboscis of the mosquito was followed. The secret of many years of research was revealed. The discovery had proved both the species of mosquito which carries the parasite, and the form and the position of the parasite within it. Thus, not only was the way opened at last for systematising the best measures for combating the carrying of malaria by this species of mosquito, but scientists had the original source of

[1] *Memoirs*, by Ronald Ross. (John Murray, 1923.)

the disease traced for the first time, the direct consequence being the facility for investigating the insidious developments of malaria in its many and often disguised forms."

It is not the intention of the founders of the Institute that it should compete in any way with the existing Schools of Tropical Medicine in London and Liverpool. Their work is, of necessity, mainly directed to the training of young medical men for practising in the Tropics. The primary object of the Ross Institute would be research; a clinical establishment would be maintained in intimate conjunction with its laboratories, and expert workers would be able to carry out there, under favourable conditions, their attack on the many outstanding problems of tropical disease.

The regular publication of the new *Journal of Scientific Instruments* commenced with the appearance of the first number last October. The *Journal* is produced by the Institute of Physics with the co-operation of the National Physical Laboratory, Dr. J. S. Anderson, of that Laboratory, acting as first editor. It is intended that the matter contained in it should fall under one of the following heads: (1) Papers dealing with the practical and theoretical aspects of Scientific Instruments; (2) Notes on new instruments, modifications of old ones, and descriptions of instruments of special historic influence; (3) Laboratory and Workshop notes; (4) Reviews and correspondence. The price per monthly issue of 32 pages is 2*s.* 6*d.* (or 2*s.* to members of the Institute of Physics). The first number commences with an important paper by Prof. Filon, F.R.S., on the Measurement of True Height by Aneroid. A special aneroid dial is used which gives the "lapse-rate" height, *i.e.* that determined on the assumption that the temperature gradient upwards is constant. Although intended primarily for aeroplane use, an aneroid with such a dial is an instrument of general interest. It is to be hoped that the editor will bear in mind that instruments whose application is entirely restricted to aeronautics are of very little interest to the general physicist. Other papers deal with the measurement of the internal diameter of sealed glass tubes, the temperature control of a Pulfrich refractometer, and a general-purpose recording drum devised by Prof. C. V. Boys. The *Journal* is well printed and of convenient size for the production of comprehensible drawings—a matter of the utmost importance for a publication of its kind.

The *Report of the Oxygen Research Committee of the Department of Industrial and Scientific Research* (H.M. Stationery Office, Imperial House, Kingsway, W.C.2.; price 8*s.* 6*d.*) contains a full discussion of the problems involved in the storage and transport of large quantities of liquid oxygen. It is divided into four parts. Part I, entitled The Storage of Liquefied Gases, deals with the theory of the Dewar vacuum vessel when used as a container of very cold liquids. The purely theoretical part of the chapter was largely prepared by Dr. Harker, whose death we have had to record with great regret in these Notes. The experimental work suggested by the theory was carried out by Prof. Briggs, of the Heriot-Watt College, Edinburgh, who also investigated the effects of carbon and silica placed as absorbents between the walls of metal vacuum vessels. Such absorbents are essential for the practical use of metal containers, but there is a risk of explosion when charcoal is used if the walls of the container are pierced (*e.g.* by a bullet), so that the surface of the charcoal is suddenly drenched by liquid oxygen. Part II deals with the manufacture of metal vacuum vessels. These are now made by joining spun copper hemispheres with a special form of soldered joint, but a great deal of investigation has been necessary in order to determine the best method for the construction and evacuation of large numbers of flasks. Parts III and IV deal with special difficulties which have arisen in the use and transport of the flasks. An appendix to the report contains a valuable collection of thermal data relating to the liquefaction of gases and to the properties of substances at low temperatures.

Other recent reports from the *D.I.S.R.* include Report No. 17 of the Food Investigation Board on Mould Growths upon Cold-store Meat (price 1*s*. 6*d*. net), which contains a paper reprinted from the *Transactions of the British Mycological Society* (vol. viii, part iii, 1923), with a popular introduction, the *Report of the Aeronautical Research Committee* 1922–3 (price 2*s*. net), *Fuel Research Board, Technical Paper No.* 7, on the Low-temperature Carbonisation of Coal in Vertical Retorts (price 9*d*. net), and *Building Research Board, Special Report No.* 7, on Heat Transmission through Walls, Concretes, and Plasters (price 1*s*. 6*d*. net). This latter contains results which should be of great interest to those about to build houses. In addition to experiments carried out by Dr. Ezer Griffiths at the National Physical Laboratory, the Report contains an account of experiments carried out at the Norwegian Technical Academy on twenty-four experimental huts identical in every respect save wall construction. The general conclusions show that the cheapest type of one-brick English *hollow* walls are as efficient as any others both from the point of view of heat insulation, first cost, and strength.

Pamphlet No. 2 of the British Science Guild Publicity Service is entitled *Thermionic Valves and their Uses* and is written by Prof. J. A. Fleming. It details the history of the invention of the Fleming two-electrode valve and of the subsequent improvements. The latest development is in the construction of high-power generating valves for wireless transmission. These have been made possible by the discovery in the laboratories of the Western Electric Co. of America that a copper tube with a sharp edge can be welded to a glass tube. In large valves the great source of trouble is the heating of the metal cylinder by the bombardment of the electrons. In the metal-bulb valves the copper part forms the anode cylinder, and it can be kept cool by immersion in water. Valves of 10–100 kilowatts have been made in this way, and the General Electric Co. of America is said to be preparing a valve of the Fleming type with an output of 1,000 kilowatts (*i.e.* 1,300 horse-power) ! Pamphlet No. 3 issued by the Guild contains an account, written by Sir Robert Hadfield, of his discovery of manganese and other steels, and reminds us how much we owe to the production of the various alloy steels which followed his original discovery.

Possibly owing to their large subsidies for scientific research, the Americans are certainly at present adding much to our general knowledge. Mr. Maynard M. Metcalf, of the Orchard Laboratory, Oberlin, Ohio, sends us a very detailed report, of nearly five hundred pages, on the *Opalinid Ciliate Infusorians*. These are parasites of the cæcal portion of the rectum of Anuran hosts, and the author treats the group with great completeness throughout.

We have received a pamphlet in English regarding the VIIIth Olympiad to be held in Paris in 1924, entitled *Contests of Art*. A number of juries consisting of men distinguished in Architecture, Literature, Music, Painting, and Sculpture have been appointed in order to decide regarding prizes and medals which it is proposed to give in these subjects. Further information can be obtained from the Secretary of the Executive Committee, 30 Rue de Grammont, Paris (2e arrt).

The Research Defence Society, 11 Chandos Street, London, W.1, has issued its ninth annual pamphlet, *The Fight against Disease*. The present pamphlet consists of an able lecture by John C. McVail, M.D., LL.D., delivered in the House of Commons on July 25, 1923, on Smallpox and Vaccination. He maintains four propositions, namely: (1) smallpox is worth preventing ; (2) smallpox can be prevented by vaccination ; (3) smallpox cannot be prevented without vaccination ; (4) properly conducted vaccination is very safe. How anyone can be foolish enough to think that smallpox is not worth preventing would be unbelievable by those who have ever seen a case of smallpox—the poor wretch lying groaning in bed, rolling from side to side, with his or her face bloated to about twice its size and

covered with a mass of stinking pustules—thousands of them, and each as painful as a single vesicle resulting from vaccination. Add to this picture a risk of death which often reaches 25 per cent., and a very considerable risk of being blinded and of being pitted for life and injured in various organs. True, recent smallpox in Britain has been of the milder variety, but we hear that the case-mortality has suddenly increased largely in the United States. As we said in our last issue, it is time that the causal agent of smallpox were determined—if it has not been determined already. Regarding Dr. McVail's second and fourth theses, no one who has any knowledge of the subject retains any doubt; but regarding his third thesis there is some difference of opinion. Many medical men think that early detection of cases and their isolation, without any general vaccination of all infants or of adults, will suffice to keep the disease in check; and they quote the fact that the disease has not prevailed very largely in Britain of recent years, in spite of the fact that infant vaccination has been so much reduced. But this is doubted by many of those who know how difficult it really is to detect cases early enough and then to isolate them effectually, especially in the case of the young.

Various business houses are now issuing what are often excellent pamphlets on the subjects with which they deal. For instance, Messrs. Fellowes's Medical Manufacturing Company, New York, sends us one on Cancer which summarises the whole subject in a manner which will be useful to many medical men. And we have recently received from Messrs. Findlater, Mackie, Todd & Co., London, an interesting little brochure by Mr. William J. Todd, their director, called *Handbook of Wine*, How to Buy, Serve, Store, and Drink it. It is really quite an elegant though brief literary work, with some telling quotations from Meredith and Saintsbury. Wine is a glorious thing, but it must always be taken with moderation or water! Even "Senatorial Port" must not be trusted too implicitly; and though "Burgundy has great genius," we must not follow it too trustingly. True, "Port speaks the sentences of wisdom, Burgundy sings the inspired ode"; but caution must always sit in the Chair. Sherry is "A wine of all work" and Claret has an "Almost feminine grace and charm"; while Champagne boasts of an "Immediate inspiration," and Sauterne, Moselle, and Hock possess "Rather uncanny and sometimes palling attractions." Rightly such base things as whisky, beer, and gin do not enter into this Parnassian survey. The gods never drank them, and why should we? But, on the other hand, so far as we know, the gods never had gout—though the behaviour of Zeus occasionally suggested that inconvenient trouble.

CORRESPONDENCE

To the Editor of SCIENCE PROGRESS

PSYCHOLOGY AND EDUCATION

FROM COL. ARTHUR LYNCH

SIR,—The record of the British Association at its Liverpool meeting was so good in parts that this excellence has tended to cover a defective side in which the quality was far below the proper standard. I refer to the Sections of Psychology and Education.

This has given me so much concern—partly because I am here fighting for my own hand, or rather for that ideal of science which has ever led me on—that I have studied the psychology of the matter itself with special respect to the professional mind; and I now endeavour to express in brief form the substance of what I have gathered.

The papers in the Psychological and Educational sections were almost entirely bad, and hardly one exhibited the marks of a veritable scientific handling.

I am not here a stickler for mere form, but in any scientific exposition, even of subjects usually found to be so elusive, we should have a reference to deep foundational ideas, a search for great generalised principles, a conduct of the argument step by step with as much rigour as possible, and an outcome in a vision of fertile applications.

Judged by standards such as these, how was it possible that the majority of the papers could have been received at all? That, for instance, of Bishop Welldon? That was mainly opinionative; many of the others should have been disbarred on the ground of deficient intellectual calibre.

It may be said that such popularisations tend to encourage interest in science: I reply that the proper course of the development of science is by virtue of valid science, and that the popularisation of mere polemical discussions or of newspaper booms, with their penchant for the sensational and silly, retards rather than helps science.

False values are introduced, inverted standards obtain official sanction, the perspective of reputations, either of work or of men, becomes absurdly distorted, and the spurious work finally chokes up the avenues through which true science may have to struggle for recognition.

The history of science is studded with examples of great work disregarded, and we in another generation look back with wonder; I say, simply, the same results are produced to-day and by the same causes, and all this is true in particular of psychology.

The main reasons are: The tendency of professors to form a close borough not for the Advancement of Science, but for the furtherance of their own, possibly in some cases intellectual, interests. The inbreeding of the Universities by which professors whose brains have become addled by false learning select their successors by reason of their conformity to their own bad teaching. The tendency of professors, editors of scientific and medical

journals, scientific societies and associations, to "drink the label" instead of exercising an independent judgment, to kotow to titles and authorities, to roll logs, to play the safe game, to entrench themselves in grooves, and, in endeavouring to serve inferior ends, to let the veritable interests of true science fall out of consideration.

All these faults were manifested in the programme of the British Association, and if they escaped general criticism, that result is due to the very evils of which I complain, and it serves to point the moral.

Yours faithfully,
ARTHUR LYNCH.

September 20, 1923.

THE DARWINIAN THEORY

I—FROM R. A. FISHER, M.A.

DEAR SIR,—In regard to Prof. MacBride's reply to my letter in your October number, I may be excused from replying to those parts which are irrelevant, and still more from retorting upon those parts which are personal. In that section, however, which *is* devoted to my criticism, Prof. MacBride displays such an astonishing lack of familiarity with the biometrical method, and still more with the established results of Biometry, that a few words of elucidation may not be out of place.

Prof. MacBride claims that, apart from "sports and monstrosities" to which, according to him, Mendelian inheritance is confined, differences between brothers are not heritable. I called his attention to *human stature* as an example, which has been extensively studied, of that heritable fluctuating variability, which represents the almost universal type of variability upon which Darwin relied in developing the theory of Natural Selection. This appeared to be a case in which we can definitely assert, on the basis of existing knowledge, that differences between brothers are inherited. To my surprise, Prof. MacBride repudiates even this instance, with characteristic emphasis (p. 290):

"Now Mr. Fisher's objections are characteristic of the loose kind of statistical juggling that passed for research in heredity before the insistence on exact experimental methods. How, for instance, does Mr. Fisher know, or how could anyone know, that differences in stature between brothers are inherited? This could only be done if the brothers married women of exactly the same genetic constitution, and how could this sameness be ascertained? One has only to state the position clearly in order that everyone can see the absurdity of it."

The biometrical mode of reasoning depends essentially on the fact, which has been amply confirmed by experiment, that, however variable a population may be, the mean of a number of individuals is generally less variable, and if our sample is a random one, is less variable in a calculable proportion, than is a single measurement. For example, individuals of our population appear to be normally distributed in stature, with a standard deviation of about 2·5 inches; consequently an individual chosen at random will rarely differ from the mean by as much as three times this amount, and will only differ from the mean by (say) five times this amount in exceedingly rare instances. The mean of 1,000 individuals chosen at random will be very much more stable; its standard deviation will be about a twelfth of an inch, and it will rarely differ from the general mean by as much as a quarter of an inch. If, therefore, we find that two samples of this size differ in their mean

by half an inch, or an inch, we conclude that the observed difference is not due to chance in the selection of the individuals making up the samples, but is due to the manner in which the selection was made ; such differences are said to be *significant*, and it is by basing his argument on significant differences that the biometrician surmounts the difficulty which looms so large to Prof. MacBride, that we cannot mate our brothers to genetically identical women.

Taking for example the stature of 1,000 pairs of brothers and that of one son of each, and comparing the mean stature of the sons of the taller brothers with that of the sons of the shorter brothers, we can see if the difference is significant. If the average difference between the brothers was two inches, we should find, in accordance with the consistent body of biometrical research, that the average difference between the sons was very near to one inch. Such a difference is assuredly not due to chance. If our opponent will not admit that it is due to the differences in the paternal inheritance, he must ascribe it to the mothers. Then compare the statures of the mothers. We shall probably find that the taller brothers have in fact married the taller women ; the average difference will be about half an inch. The case then stands thus : difference between brothers—two inches ; difference between sons—one inch ; difference between wives—half an inch. If these be the facts, the common-sense investigator will not hesitate to recognise that part at least of the differences between the sons was ascribable to their fathers ; and that the differences in stature between brothers were in fact heritable. If further refinements were necessary, we might, with extended material, confine our attention to those cases in which the wives were of the same height, but we need not pursue our opponent farther into the wilderness of fantastic hypotheses which further observations would force him to put forward if he continued to evade our conclusion. The prettiest biometrical experiment for disposing of the peculiar doctrine of Prof. MacBride would be to select only those cases, which are fairly numerous, in which two brothers have married two sisters ; for in this case, on Prof. MacBride's view, the wives would be genetically identical.

The above examples are designed rather for simplicity of explanation than as examples of the way in which the practical biometrician goes about his business. They answer Prof. MacBride's difficulty about methods of research in human heredity, but, being hypothetical, they do not tell us to which conclusion such research would in fact lead. In practice biometrical data are not collected afresh for each specific question, and are handled by somewhat more complicated calculations (*e.g.* correlation coefficients) ; the use of these more advanced methods does not, however, involve any fresh assumptions. To demonstrate the existence of heritable differences between brothers, it is sufficient to point out that the correlation of father and son is very near to 0·5, while that of uncle and nephew is near to 0·3. The effect of average differences in the mothers is accurately eliminated by using for comparison the partial correlation (0·44), instead of the total correlation between father and son.

To Prof. MacBride all this is a "loose kind of statistical juggling," just as, according to the same authority, all characters which mendelise are "sports and monstrosities." If Prof. MacBride had put forward an alternative explanation of the body of facts upon which biometrical conclusions are based, he could be entitled to a respectful hearing. This he has not attempted ; he ignores the facts and abuses the method. Consequently it would not be surprising if his advocacy of Kammerer's view should carry, if possible, even less weight to statistical, than it does to Mendelian, students of heredity.

Yours faithfully,

R. A. Fisher.

Rothamsted Experimental Station.

October 31, 1923.

II—From Prof. E. W. MacBride, F.R.S.

Dear Sir,—I do not wish to prolong the controversy between Mr. Fisher and myself: it is obvious that Mr. Fisher's answer does not touch the main point of my criticism of the biometrical method, which is this, that biometrical methods are worthless except when applied to biologically purified material. To the biometrician, men are just men, that is, interchangeable and equivalent units; to the biologist, if they are of different races—or especially if they are crosses between different races—they are not comparable. Mr. Fisher talks of the "well-known fact that, however variable a population is, the mean of a number of individuals is less variable, etc. etc." What is a "population"? Is it a sample of a pure race, or is it any random assortment of individuals from a mixture of races? If the latter, what results of any value can be got by mathematical measurement? What chemist would try to draw conclusions from the properties of an unanalysed mixture?

One of our best British anthropologists, Fleure, has recognised this, and asserts that the first step in anthropological research is to define types. He says that types must be characterised from samples that have remained pure from intermixture, and that the average measurements of head-index (for instance) taken over a wide area have very little value. Mr. Fisher gives hypothetical figures which he thinks prove that the sons of brothers differ in stature and that these differences are not due to differences in the height of the mothers. I should like to see details of the actual working out of these relations of stature between the members of the families of brothers. But even if it should be found that there was something to be said for the supposition that where the mothers were the same in appearance and hereditary potentiality the offspring differed, that would not support Mr. Fisher's hypotheses, *viz.* that "mutations" are continually occurring within in the race and that the selection of these is the means by which evolution proceeds. There are differences in nutrition to be considered, and *crosses between different races will yield offspring in which in the same family the characters of the parent races will segregate.* What the causes of individual human variation are it will generally be impossible to disentangle. Such questions can only be decided by experiments on purified material. This is recognised by the latest authority on human inheritance—Lenz—who is an ultra-Mendelian. He frankly says that Mendelism cannot be demonstrated from human statistics: he concedes that we are only justified by analogy in applying Mendelian laws to human material, and that the laws themselves can only be demonstrated by careful experiments on animals and plants. That is the whole crux of my former answer. When these careful experiments have been undertaken they do not bear out Mr. Fisher's hypothesis.

Mr. Fisher accuses me of "ignoring facts" and of "abusing" the [statistical] method. I deny that his mathematical computations are biological facts at all, and in scorning the method I am taking up precisely the same attitude as that adopted by Dr. Bateson and his pupils in the great discussion with Weldon and Pearson at the British Association meeting in 1904. Weldon and Pearson had produced work on the inheritance of colour in poppies, based on observations on flowers unshielded from the visits of insects, and Bateson, rightly I think, refused even to discuss results obtained under such conditions.

I care very little what statisticians like Mr. Fisher think of Dr. Kammerer. I have never yet met a biologist, whether a Mendelian or not, who did not admit that, *if Kammerer's results were confirmed*, they would absolutely prove his contention that the effects of habit were inheritable. One of the leading

younger Mendelians at the discussion at Cambridge made this statement in unequivocal terms.

But it may interest readers of SCIENCE PROGRESS to learn that, leaving out altogether the cytolysin experiments of Guyer, confirmations of Kammerer's results have come in from *four different quarters*, and one of these is a paper recording researches by Pavlov, one of the leading physiologists in the world. I feel convinced that when these results are more widely known, all true biologists, including Mendelians and statisticians *who are also biologists*, will be convinced.

Lastly, I have reason to hope that light is beginning to dawn on the causes of the appearance of Mendelian sports; if the work to which I refer is confirmed, it will show once for all the hopelessness of taking Mendelian sports as the units in the evolutionary process.

Yours faithfully,
E. W. MACBRIDE.

IMPERIAL COLLEGE OF SCIENCE AND TECHNOLOGY,
SOUTH KENSINGTON,
LONDON, S.W.7.
November 1, 1923.

[This correspondence must now be closed in this Quarterly.—EDITOR.]

DR. KAMMERER IN CAMBRIDGE

FROM A. G. THACKER, A.R.C.SC.

DEAR SIR,—In the October number of SCIENCE PROGRESS Prof. MacBride criticised at length my brief comments upon Dr. Kammerer's lecture in Cambridge. But he does not really dispute my principal criticism, that Dr. Kammerer confused the issue by a generalisation that had no relation to the real points in dispute. Dr. Kammerer stated that "if what is hereditary cannot change" there could be no evolution. I pointed out that the followers of Weismann—being evolutionists—never alleged that "what is hereditary could not change," but agreed that a change which was really a germinal change would of course be inherited. Prof. MacBride's only reply is to say that Lamarckians also think that a change must be a change in the germ-cells if it is to be hereditary. Of course Lamarckians also think this. But I was not at that point discussing the Lamarckian view; I was defending the followers of Weismann from Dr. Kammerer's extraordinary misstatement. The real point at issue is, of course, not whether hereditary changes occur, but how they occur—as Prof. MacBride himself says.

All this is very obvious, and need never have been mentioned but for Dr. Kammerer's generalisation. But it is, I think, important to get rid of all *a priori* assumptions as to how germinal variations may arise. On the one hand, there is no reason to suppose that the germ-plasm is necessarily absolutely stable in constitution unless it is influenced from without. In a substance having the chemical complexity of the nucleus of a germ-cell, purely internal reactions might well cause instability and change. On the other hand, many of us now think that the earlier Weismannists were too dogmatic in endeavouring to rule out altogether the possibility of germinal changes arising from somatic changes. We should be prepared to find variations arising either from internal reactions or from external influences; though if I wished to enter into dialectics I should point out to Prof. MacBride that no variation could be "uncaused."

On the question of Dr. Kammerer's actual experiments, I suggested that

it was desirable to suspend judgment pending fuller and more extensive information. The negative results obtained by Mr. M. Fox in his recent experiments on *Ciona* (see *Nature*, November 3) appear to show that more information is necessary on that point. It is perhaps true that the Lamarckian factors might be operative, and that yet it might be difficult to obtain direct evidence of their reality. But this is all the more reason for collecting as much information as possible about those few cases in which such direct evidence is supposed to be forthcoming.

Yours faithfully,
A. G. THACKER.

November 15, 1923.

ESSAYS

THE THEORY OF ELECTROLYTES (H. N. Allen, B.Sc., Ph.D.).

A remarkable advance in the theory is made in a recent paper by P. Debye and E. Haeckel, which deals mainly with the lowering of the freezing-point of water by dissolved electrolytes.[1]

If P_k is the osmotic pressure calculated by van't Hoff's law, on the assumption that the electrolyte is completely dissociated, the actual pressure will in general be smaller than this; and for each concentration can be represented by f_0 P_k, where f_0 is a variable fractional coefficient which diminishes as the concentration increases, and approaches unity as it diminishes to zero. The number of free single particles for each concentration can, according to Guldberg and Waagesch, be determined on thermodynamical principles, and Planck considers that the whole complex of interdependencies can be founded on thermodynamics.

The electrical conductivity, according to the ordinary theory, depends on the number of dissociated ions; and this property, the lowering of the freezing-point, the increase in the boiling-point and the osmotic pressure should be interdependent. Strong acids and bases, and their salts, however (the "strong" electrolytes), do not show the required agreement, the differences becoming *greater* at high dilutions. If $1 - f_0$ is plotted as a function of the concentration c, the curve, in the case of binary electrolytes like KCl, tends to the zero-point with a finite tangent, depending on the equilibrium constant K, according to the classical theory. If the molecule is split into ν ions, the law of mass action gives $1 - f_0 = \nu - 1/\nu \times c^{\nu-1}/K$ for small concentrations, so that where there are more than two ions there should be contact of higher order with the abscissa axis. All the proposed practical interpolation formulæ made $1 - f_0$ proportional to a fractional power of the concentration; and F. Kohlrausch found that the power was 0·5 for conductivity, at infinite dilution.

W. Sutherland[2] tried to remodel the theory on the assumption that the dissociation was in all cases complete; and N. Bjerrum in 1909 showed that this view was correct, and followed if the electrostatic forces were taken into account. The change in the theory is similar to that in the van der Waals gas theory, when actual gases are substituted for the original ideal gases; but in that case the forces between the molecules fall off much quicker than the inverse square law, which holds for the ions, requires. Milner has carried out the necessary calculations[3]; but his method leads to mathematical difficulties; and the relation between $1 - f_0$ and c can only be given in graphical form. He assumed that his results would hold for high concentrations, which is not the case.

[1] "Gefriespunctserniedrigung und verwandte Ersheinungen," *Phys. Zeits.*, May 1, 1923.

[2] *Phil. Mag.*, January 14, 1907.

[3] *Phil. Mag.*, **23**, 531, 1912; *ibid.*, **25**, 743, 1913.

J. C. Ghosh has recently studied the question [1]; but his results are not altogether reliable. P. Hertz has endeavoured to make use of the electrostatic influence of the ions on one another, as affecting their mobility, to explain the facts of electrical conductivity; but his theory is based on the kinetic gas theory; and he uses ideas connected with the free path in gases at low pressures, which have no real application in this problem.

In order to facilitate the mathematical treatment of the problem of the lowering of the freezing-point, the authors introduce the fundamental function G defined by the equation:

$$G = S - \frac{U}{T}; \tag{1}$$

where S is the entropy, U the energy, and T the absolute temperature, of the solution.

They deduce from this that

$$dG = \frac{p}{T}\,dV + \frac{U}{T^2}\,dT.$$

U is then divided into two parts, U_k the energy given by the classical theory and U_e the additional electrical energy. From (1) it follows that

$$T^2\frac{\partial G}{\partial T} = U; \quad G = G_k + G_e;$$

and $G_e = \int \frac{U_e}{T^2}\,dT$. They proceed to derive the value of the deviation Θ of the lowering of the freezing-point from that given by the classical theory. The following are some of the results obtained:

(1) In the limit, for small concentrations, the percentage deviation of the lowering of the freezing-point is proportional to the square root of the concentration for all electrolytes.

(2) If the dissolved molecule is split into $\nu_1 - \nu_i - \nu$ ions of different kinds $1 - i - s$, with valencies $z_1 - z - z_s$, the above deviation is proportional to a valency factor

$$w = (\Sigma\nu_i z_i^2 / \Sigma\nu_i)^{\frac{3}{2}}.$$

(3) For small concentrations the above deviations are inversely proportional to the 3/2 power of the dielectric constant of the solvent.

The deviation $\Theta = \Delta_k - \Delta_t/\Delta_k$, where Δ_k is the lowering of the freezing-point given by the classical theory with complete dissociation, and Δ_t is that observed. Θ is also equal to $1 - f_0$.

For small concentrations the theory shows that

$$\Theta = w\,\frac{\epsilon^2}{6DkT}\sqrt{\frac{4\pi\epsilon^2}{DkT}\,n\Sigma\nu_i}, \tag{2}$$

where $T = 273$ for dilute solutions in water, $\epsilon = 4{\cdot}77 \times 10^{-10}$ 19 *e s u*, n is Loschmidt's number $= 6{\cdot}06 \times 10^{23}$, $k = 1{\cdot}346 \times 10^{-16}$ ergs, D is the dielectric constant of water at O°C $= 88{\cdot}23$, and $\Sigma\nu = \nu$ the number of ions into which a molecule of the salt splits up. This gives $\Theta = 0{\cdot}270\,w\sqrt{\nu\gamma}$. Curves are drawn showing the relation between Θ and $\sqrt{\nu\gamma}$ for $MgSO_4$, $La(NO_3)^3$, K_2SO_4, and KCl; and for other salts having these valency relations; they are found to be tangent at the origin to the straight lines obtained by giving w the values 1, $2\sqrt{2}$, $3\sqrt{3}$, and 8, corresponding to the number and valencies of the ions of the salts; and to follow these lines closely for some distance.

[1] *Chem. Soc. Jour.*, **113**, 449, 627, 707, 790, 1918.

For high dilutions the theory shows that the diameter of the ion can be neglected, and it is in this way that the above equations are obtained; for higher concentrations the deviations depend on the individual properties of the ions, and particularly their dimensions. The curve for $CsNO_3$ approximates to the straight line law much further than that for KNO_3, and so in order for CsCl, KCl, NaCl, and LiCl, the curve of the last-named salt bending quickly over towards the abscissa axis. The mathematical theory shows that Θ can be obtained for higher concentrations by multiplying the expression (2) by a factor, which depends on what is known in the theory as the characteristic length $1/\kappa$, which is of the same order of magnitude as the ion diameter. When this is taken into account, experiment and theory are found to be in good agreement for different salts, over a considerable range of concentration. For very high concentrations other factors previously neglected have to be considered; but there appears to be little doubt that even here the molecules of the salts are split up into their ions.

A qualitative explanation of the conductivity relations is also given, the most important features being the braking action of the stream of ions of opposite sign on the motion of the ions; and the braking action due to the rotating of the dipoles of the solvent owing to the electrostatic forces, as the ions pass between them. A full mathematical treatment of this problem is promised.

THE ANNEALING OF GLASS (H. V. Mallison, M.A., B.Sc., University College, Exeter).

It is well known that if glass is in a state of stress, its quality is deteriorated, as it is liable to crack easily and there is a risk of the surfaces becoming distorted. This is more especially the case with optical glass, as a stress alters the refractive index of the glass and produces the phenomenon known as "double refraction." Hence it is essential that in glass lenses, prisms, etc., there should be as little stress as possible.

The method used commercially for the removal of stress in glass is known as *annealing*. This may be roughly described as heating the glass until it becomes comparatively soft, keeping it at that temperature for some time, and finally cooling it more or less slowly.

Formerly this process was rather haphazard and tentative as regards the time of annealing and rate of cooling, and a large proportion of failures resulted, with consequent waste of time and material. Now, however, the times and rates can be calculated with a very fair degree of precision, and the chance of a failure at present is quite small.

The object of this paper is to describe the processes which take place when glass is heated or cooled, with the mathematical terms which most nearly represent them.

When a piece of glass is being heated, the outer layers expand; consequently there is a pull exerted on the inner layers, and the glass is in a state of stress, there being a tension in the inner layers and a compression in the outer layers. Similarly, when a piece of glass is being cooled, the outer layers contract first, and a state of compression exists at the centre. In the heating and cooling of glass, these two effects are equal and opposite; *i.e.* if a piece of glass originally unstressed is heated to a certain temperature, none of its stress being lost, and then cooled to the former temperature, there will be no stress finally in the glass. If, however, when the glass is heated an amount of stress s is lost, when the glass is cooled again it will have a stress—s, equal in magnitude but opposite in sign to that which was lost.

This stress obviously depends on the rate of change of temperature, and would disappear if at any time the temperature were kept constant, since all

parts of the glass would then expand equally. In calculations, the time taken for the temperature to become constant throughout the glass, and hence for the stress to become fixed, is ignored, as it is small compared with the other times considered. Hence it is assumed that the stress is discontinuous at the beginning and end of heating or cooling.

The stress here referred to also depends on the shape and nature of the glass, and varies through the piece of glass during heating. One of the simplest cases is that of a large plane slab of glass. In this case, if $2b$ is the thickness, F the stress at any point, x the distance of the point from a plane equidistant from the surfaces of the slab, the stress is given by the equation

$$F = a\frac{d\theta}{dt}(b^2 - 3x^2),$$

where a is a constant depending on the type of glass, θ is the temperature (generally measured in degrees Centigrade), and t is the time.

This equation, originally proved for a constant rate $\left(\frac{d\theta}{dt} = \text{const.}\right)$, is easily seen to be true for any continuous rate of heating or cooling, as it may be regarded as the limit of an infinite succession of constant rates, each lasting for an infinitesimal time.

The stress is usually measured at the centre, where it is a maximum, or at the surface, where the strain of opposite sign is a maximum.

Hence if s_0 is the strain at the centre, it is given by the equation

$$s_0 = c\frac{d\theta}{dt},$$

where c is a constant depending on the thickness and type of glass.

If, also, there was originally a stress present in the glass, whose value at the centre was $+ s_A$ (tension), the stress at any time during heating is given by

$$s_0 = c\frac{d\theta}{dt} + s_A.$$

The same equation holds for cooling.

The stress that exists during heating or cooling is called the *temporary* stress; that which remains when the glass is kept at the same temperature is called *fixed* or *permanent* stress.

In addition to this phenomenon, there is also the process known as *annealing*, or *plastic* or *viscous flow*. When glass is heated so that it becomes soft and plastic, without losing its original shape, the stress gradually disappears if the temperature is kept constant, owing to the redistribution of the material of the glass. The rate at which it disappears, at a given temperature, depends in some way on the amount of stress present. Clerk-Maxwell (" On the Equilibrium of Elastic Solids," *Trans. Roy. Soc. Edinburgh*, vol. xx, pt. 1) suggested that it might be directly proportional to the stress, *i.e.* $-\frac{ds}{dt} \propto s$. This equation, however, has been found not to agree very well with experiment; and as a result of careful experiments Messrs. Adams and Williamson (" The Annealing of Glass," *Journal of the Franklin Institute, U.S.A.*, vol. cxc., pp. 597–631 and 835–70, 1920) suggest that a much better agreement is given by the equation $-\frac{ds}{dt} \propto s^2$.

This result was obtained independently by Messrs. W. M. Hampton, B.Sc., and W. E. Ward, of the Glassworks of Chance Brothers & Co., Ltd., Smethwick, Birmingham, in 1920.

Hence
$$\frac{ds}{dt} = -\lambda s^2$$

gives the rate at which stress disappears.

The quantity λ depends only on the temperature of the glass, not on t, and is found very approximately to double itself for every rise of 10° C. in temperature.

Thus
$$\lambda = \text{const.} \times 2^{\frac{\theta}{10}},$$
$$= 2^{\frac{\theta-\theta_0}{10}},$$

where θ_0 is a constant, and depends on the type of glass.

It should also be noticed that, since the stress relieved may be either tension or compression, the "annealing equation" above should properly be written

$$\frac{d/s/}{dt} = -\lambda s^2.$$

Such a form, however, is unsuitable for purposes of calculation, and in practice the process would have to be divided into two parts when $s = 0$, *i.e.* where the stress changes sign. For the purposes of calculation the old equation, being self-adjusting in this respect, would have been much easier.

The problem is now, given these two effects, to find the stress in the glass at any time during heating or cooling. In the ensuing equations the value of the stress at the centre alone is considered.

For simplicity consider cooling only.

Suppose that at the moment of cooling there is a stress s_A present in the glass, and that after cooling for time t, the glass has lost an amount s of stress. Let s_t be the actual stress in the glass after time t, and $\frac{d\theta}{dt}$ the rate of cooling (which is, of course, negative).

Then the stress that would exist in the glass were there no original stress and no stress lost is $c\frac{d\theta}{dt}$.

Hence the actual stress

$$s_t = c\frac{d\theta}{dt} + s_A - s.$$

Again, cognisance has to be taken of the sign of s, the stress lost. For the sake of simplicity it may be supposed that s_t remains positive. (This is usually the case in practice.)

Thus if ds_t is the decrease in stress in a small additional time dt, then

$$ds_t = c\frac{d^2\theta}{dt^2} - ds.$$

ds is the stress lost by annealing in time dt. s_t being the actual stress in the glass, stress is being lost by annealing in accordance with the equation

$$\frac{ds'}{dt} = -\lambda s_t^2,$$

$$\text{or} \quad ds_t = -\lambda s_t^2\, dt.$$

This presupposes that s_t and θ do not change; but in time dt they alter by first-order small quantities only.

Hence the stress lost, ds, is equal to $+\lambda s_t^2 dt$ to the first order.

Thus
$$ds_t = c\frac{d^2\theta}{dt^2} - \lambda s_t^2\, dt.$$

Hence
$$\frac{ds_t}{dt} = c\frac{d^2\theta}{dt^2} - \lambda s_t^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(i)}$$

This equation gives the stress at any time during cooling when the cooling rate is known.

If S denotes the *permanent stress* that would remain in the glass if it were kept at the temperature to which it is cooled after time t, then S is equal to the original stress together with that lost by plastic flow during cooling.

Thus $S = s_A + s$ . . . . (ii)

s is given by the equation obtained above,

$$\frac{ds}{dt} = +\lambda s_t^2 \qquad . \quad . \quad . \quad . \quad . \quad \text{(iii)}$$

From these equations all the necessary calculations may be made.

Equation (i) is Riccati's equation, and cannot usually be solved in finite terms. It may be solved graphically for given numerical values by Runge's method. There is, however, one case of great practical importance where the equation may be solved—when

$$\frac{d^2\theta}{dt^2} = 0, \ i.e. \ \theta = at + b \ (a, \ b \text{ constants}),$$

the case of *linear cooling*. This case has been worked out in detail by Mr. W. M. Hampton, B.Sc. (whose work has been referred to above), who has obtained in large-scale annealing results which are the best that have been hitherto arrived at.

Two methods of annealing can be derived from these considerations:

I. *The Old Method*.—Anneal quickly (at a relatively high temperature) and cool slowly. In this way the temporary stress introduced by cooling, $c\frac{d\theta}{dt}$, is small, and therefore little stress can be lost by plastic flow during cooling, to reappear as permanent stress when the glass is cold.

II. *Adams and Williamson's Method*.—Anneal slowly (at a relatively low temperature) and cool quickly. For as cooling takes place through temperatures at which annealing is very slow, very little plastic flow can occur.

The deciding factor between these two methods is that of *time*. The preferable one of the two is that which, on the whole, takes the least time.

The problem in the annealing of glass is this: Given a piece of glass with a certain stress s_1 (at the centre), to reduce this stress to s_2. The glass has first to be heated to a suitable annealing temperature. As is seen above, this should be chosen so that the total time taken over the process is a minimum. The stress s_A to which s_1 must be reduced has then to be calculated; the time taken over this process will be given by the annealing equation. In fact, if t_A is the time taken,

$$\frac{1}{s_A} - \frac{1}{s_1} = \lambda t_A.$$

The time t_c taken over cooling at a given rate may then be calculated from equations (i), (ii), and (iii); and the whole time taken is then $t_A + t_c$.

In calculating stresses, it must also be remembered that no stress must exceed a certain maximum, as otherwise the glass may "fly."

HISTORICAL NOTES ON THE ASSAY OF GOLD (D. Stockdale, B.A.).

It is improbable that any of the modern processes for assaying or purifying gold, such as those which involve the use of nitric acid, sulphuric acid, or chlorine gas, were known in ancient times, yet it is certain that the Greeks and Romans knew both how to assay and how to purify bullion.

A coin of Philip of Macedonia, for example, contained 997 parts of gold

per 1,000, and it is most unlikely that there was any source of native gold of such fineness.

Geber, who lived probably in the ninth century, is credited with the discovery of nitric acid, but sulphuric acid was known much earlier; yet it is unlikely that the latter was obtainable in any great quantity in those early times. It is believed that the modern process of "parting" by nitric acid was discovered by the Venetians in the latter half of the fifteenth century. They had an island in the Adriatic which was most strictly guarded and from which they sent out such large stores of fine gold that the rumour spread abroad that the Philosopher's Stone had at last been discovered. However, Biringuccio, in 1540, gave the secret away by publishing an account of the process.

The object of this paper is to suggest the probable methods by which gold was purified from silver in ancient times, and no very careful study of the reactions or of the exact conditions was made during the experiments. Many interesting instructions for bringing about this separation are to be found in old books, but these appear to be merely variations of two processes, the "Assay by Antimony" and the "Cementation Assay," in which the purifying agents are sulphur, in the form of antimony sulphide, and common salt respectively.

Most of the information given in this paper was obtained from Percy's *Metallurgy* (1880) and from the *Assays of Metals* (1683), a book translated from the German of Erckern, sometime Assay Master-General to the Empire of Germany, by Sir John Pettus, Deputy-Governor of the Mines Royal.

The Assay by Antimony

By "antimony" is meant, not the metal, but the sulphide, which is found as the mineral, stibnite. Stibnite is a substance with a low melting-point, and it can easily be purified from most foreign matter by liquation. The ancients very probably had large stores of this material at their command.

Pettus speaks of the process as "an old invention," and describes it as follows:

"Take one part of Gold and two parts of good clean Antimony, put it together in a Crucible, blow it, let it flow, and when well flown together, then pour it into a warm Cup made of Iron or Brass, and greased with Tallow or Wax, let the Antimony and the Gold cool in it, and then you must turn the Cup and dash it upon a stone, whereby the Antimony may go together with the Regulus, which have settled below and looks a gray-yellow colour, and may be easily beaten out." This must be done two or three times with fresh sulphide. Then the metal is to be heated in a current of air to drive off the antimony "so you have a very fine and high Gold."

This method of assay was probably used in very early times. An account of it is to be found in the writings of Basil Valentine (fifteenth century). Indeed, Valentine looked on antimony as the cure for all the ills. He used it to purify gold, men, and cattle.

The directions given by Pettus were carried out in the laboratory. The alloys experimented with contained about 50 per cent. of gold, the remainder being silver, and one fusion with antimony sulphide raised the gold content to about 70 per cent., two fusions to about 90 per cent. Undoubtedly comparatively pure gold can be obtained in this way, though the amount of gold lost during the assay is large. Probably the percentage loss would have been very much smaller if the weight of alloy experimented with had been larger.

The results of three typical experiments are given below, the first two results being obtained after one fusion, the third after two fusions with stibnite. The check analyses on the alloys were carried out in the ordinary modern way by parting with nitric acid.

Weight of Gold in Original Alloy.	Percentage of Gold in Original Alloy.	Weight of Bead after Stibnite Treatment.	Percentage of Gold in Final Alloy.	Weight of Gold lost.	Percentage of Gold lost.
Grams.		Grams.		Grams.	
8·256	49·56	11·854	65·38	·507	6·14
5·072	48·81	6·648	69·52	·450	8·86
14·766	49·33	13·224	88·13	3·116	21·10

The mechanism of the process seems to be as follows:

The silver in the bullion reacts with the sulphur in the antimony sulphide to form silver sulphide, the silver so reacting being replaced in the alloy by metallic antimony; but the reaction does not proceed to completion. As silver seems to distribute itself in a definite ratio between the metallic layer and the regulus, many fusions are necessary before the gold becomes very pure, the regulus, of course, being removed after each fusion.

The contents of the crucible settle into two layers, the gold-rich one, at the bottom, being easily separated from the sulphur-rich one at the top. The bead is easily cleaned, although it is rather brittle, and the antimony is removed from the gold by heating in a cupel in a furnace, the antimony being volatilised.

The loss of gold may be due to two causes. First, a little gold sulphide, which finds its way into the upper layer, may be formed. Then little droplets of gold, which do not run together and sink to the bottom, may be held up mechanically by the sulphides. Of course, gold and silver removed from the ingot in this way are easily recovered from the regulus.

In some experiments the stibnite was replaced by galena, which possesses the disadvantage of a high melting-point. Again the silver in the bullion was displaced, but this time the bead was so brittle and adhered so firmly to the regulus that a separation of the two layers was impossible. The galena freezes before the metals, and thus, as the alloy sticks to the upper layer, a cavity is formed in the bead as the temperature falls. A brittle, hollow bead cannot be cleaned.

The Cementation Assay

Though possibly the oldest process for assaying and purifying gold, it has only recently fallen into disuse, having been in operation in South America in 1833. A passage in Pliny (first century) evidently refers to this method, while Albertus Magnus (thirteenth century) and G. Agricola (1494-1555) give clear accounts of the procedure, which is described by Pettus as follows:

"Rhenish Gold must be beaten thin and cut in little pieces, as broad as Crowns. Upon such beaten Gold, or Gold Gilders the Cementing is to be done thus, Take 16 Loths [1] of powder of an old dry Tile, then 8 Loths of Salt, and 4 Loths of white Vitriol, grind these ingredients all together small, and moisten them with Urine or sharp Vinegar, like Copel Ashes, so is the Cement powder prepared."

The gold is placed in a pot, each strip being covered by a layer of cement about a quarter of an inch deep, and the whole is heated in a furnace at a dull-red heat for twenty-four hours, being then allowed to cool in the furnace.

"Then take it out and open it and wash the Cement Powder off with warm water, then the Gold will be found very near 23 Carats on the Content." If purer gold is required the process must be repeated, using a little saltpetre in the cement.

The cement mixture used in the experiments in the laboratory consisted of 32 grams of a finely powdered tile, 16 grams of common salt, and 8 grams

[1] A loth appears to be 4 drachms avoirdupois.

of crystallised zinc sulphate, and the operations were carried out as described above, experimenting with gold-silver alloys of widely different composition. When the alloy contains only 25 per cent. by weight of gold, the pure gold finally obtained is in the form of a very thin leaf, which is difficult to handle.

The results given below show that gold of over 23 carats was in every case obtained, and this after only one cementation, while, by making a small correction, by using a check, it was possible to estimate the gold in the original alloy quite accurately. With practice, and by careful adjustment of the temperature at which cementation is carried out, it is probable that more exact results could be obtained.

Weight of Alloy.	Weight after Cementation.	Weight of Gold after Cementation.	Weight of Gold in Alloy by Calculation.	Weight of Gold in Alloy by Synthesis.	Error.	Percentage of Gold Initially.	Percentage of Gold Finally.
grams.	grams.	grams.	grams.	grams.	grams.		
·5055	·4980	·4962	—	·5055	—	100·00	99·65
·9974	·5000	·4959	·5076	·5026	+ ·0050	50·38	99·18
1·5052	·4982	·4936	·5057	·5045	+ ·0012	33·48	99·10
2·0038	·4952	·4871	·5027	·5046	− ·0019	24·75	98·35

The chemistry of the process is very complex and difficult to understand. A simple explanation is that the sodium chloride reacts with the zinc sulphate to give zinc chloride. This reacts with the alloy, giving silver chloride, which passes away through the pores in the tile powder, the zinc which is formed being oxidised and the oxide volatilised. Examination of a partially cemented alloy shows that the cement is attacking it by intercrystalline penetration, and the purified gold is very porous, so porous that water can pass through it easily. The effect of the cement is to remove every baser metal present, leaving the gold behind as a sort of network.

In the absence of air, common salt will not attack silver, but in the presence of air, molten common salt will easily dissolve that metal. Possibly the iron in the tile is essential if the reaction is to proceed, but, more probably, the sole part the powder plays is to remove the silver chloride from the alloy mechanically as it is formed.

Some experiments were tried under exactly the same conditions, the zinc sulphate being replaced by ferrous sulphate. The results obtained were not nearly so good; a 50 per cent. gold alloy, for example, after treatment, had only lost about one-third of its silver.

ESSAY-REVIEW

The Effect of Storminess upon the Temperature in Circumpolar Regions in Winter. By E. V. Newnham, B.Sc., being a review of: **Climatic Changes: their Nature and Causes.** By Ellsworth Huntington and Stephen Sargent Visher. [Pp. xiii + 329.] (London: Oxford University Press, 1922. Price 17*s.* 6*d.* net.)

This book deals primarily with the changes of climate that are believed to have taken place over the earth during the period of many million years corresponding with the deposition of the sedimentary rocks. The principal evidence for such changes is furnished by the distribution of the fossilised remains of animals and plants, by indications of glacial action upon the various rocks, and other geological considerations; for the relatively small portion of this time during which the human race is known to have existed there is some additional evidence afforded by the distribution of the more populous cities and the nature of their engineering works. Mr. Huntington's name is associated with yet another source of information, namely the variations of thickness of the growth-rings of certain trees, which variations are dependent to a large extent upon the amount of rain which falls at certain seasons; the period about which information is given is in this case the comparatively brief one of rather over three thousand years. The scope of the problems attacked is so great that the authors found it necessary to obtain the assistance of specialists in the many subjects involved. Most of those who assisted in the work belong to Yale University, but the names of several others are mentioned, in particular that of Prof. T. C. Chamberlin, of Chicago. Their combined efforts have resulted in a work of general interest which not only summarises much recent work in geology and astronomy, but is helpful in indicating possible new lines of research.

To most meteorologists Chapters IV, VII, and VIII, which deal with the authors' so-called "solar cyclonic" hypothesis of the origin of the glacial periods and historical "pulsations" of climate, will be found the most interesting.

The full argument in support of this hypothesis is to be published later in a companion volume to the present one, entitled *Earth and Sun*. This arrangement is not altogether a happy one from the point of view of the reviewer, as it makes criticism of the main thesis difficult. Nevertheless, such criticism may be attempted in the hope that difficulties and objections (possibly not foreseen by the authors), if made now, may be cleared up in the final work. A few extracts from the book will indicate the main outlines of the solar cyclonic hypothesis.

After referring to Köppen's discovery of the paradox that when sunspots are most numerous, *i.e.* when, according to Abbot, the sun emits most energy, the earth's surface is on the average about 1° F. colder than at times of few spots, the authors continue (p. 53) as follows:

"Another large group of investigators have shown that atmospheric pressure also varies in harmony with the number of sunspots. Some parts of the earth's surface have one kind of variation at times of many sunspots, and other parts the reverse. These differences are systematic and depend

largely upon whether the region in question happens to have high atmospheric pressure or low. The net result is that when sunspots are numerous the earth's storminess increases and the atmosphere is thrown into commotion. This interferes with the stable planetary winds, such as the trades of low latitudes and the prevailing westerlies of higher latitudes. Instead of these regular winds and the fair weather which they bring, there is a tendency toward frequent tropical hurricanes in the lower latitudes, and toward more frequent and severe storms of the ordinary type in the latitudes where the world's most progressive nations now live. With the change in storminess there usually goes a change in rainfall. Not all parts of the world, however, have increased storminess and more abundant rainfall when sunspots are numerous. Some parts change in the opposite way. Thus when the sun's atmosphere is particularly disturbed, the contrasts between different parts of the earth's surface are increased." Again (p. 57): "One of the most interesting results of recent investigations is the evidence that sunspot cycles on a small scale present almost the same phenomena as do historic pulsations and glacial fluctuations. For instance, when sunspots are numerous, storminess increases markedly in a belt near the northern border of the area of greatest storminess, that is, in Southern Canada and thence across the Atlantic to the North Sea and Scandinavia." (Diagrams are given illustrating this point.) "Corresponding with this is the fact that the evidence as to climatic pulsations in historic times indicates that regions along this path—for instance Greenland, the North Sea region, and Southern Scandinavia—were visited by specially frequent and severe storms at the climax of each pulsation. Moreover, the greatest accumulations of ice in the glacial period [1] were on the poleward border of the general regions where now the storms appear to increase most at times of solar activity. Even more clear is the evidence from other regions where storms increase at times of many sunspots. One such region includes the south-western United States, while another is the Mediterranean region and the semi-arid or desert parts of Asia farther east. In these regions innumerable ruins and other lines of evidence show that at the climax of each climatic pulsation there was more storminess and rainfall than at present, just as there now is when the sun is most active. In still earlier times, when ice was accumulating farther north, the basins of these semi-arid regions were filled with lakes whose strands still remain to tell the tale of much-increased rainfall and presumable storminess. . . . From these and many other lines of evidence it seems probable that historic pulsations and glacial fluctuations are nothing more than sunspot cycles on a large scale."

From the foregoing it will be seen that according to the solar cyclonic theory there were long periods during which the sun was hotter than it is now, and that cyclonic storms were more intense at these times, and the mean temperature of the earth's surface lower. This lowering of temperature, larger than that which occurs at sunspot maxima, accompanied by increased precipitation, which in countries like Greenland and Scandinavia would be in the form of snow during most of the year, resulted in extensive accumulations of ice that easily survived the stormy summers, with their scanty sunshine.

No doubt the evidence that the greatest glaciation occurred just north of the areas which now receive most precipitation at times of many sunspots will be given very fully in *Earth and Sun*. One may observe, however, that the correlation coefficients found by Walker [2] between sunspot numbers and rainfall are generally small and often of opposite sign at places not very far apart, and the impossibility of satisfactorily forecasting rainfall from information about the state of the sun at the present time must tend to cause

[1] The reference here is evidently to the Pleistocene period.

[2] "Correlation of Sunspots with Rainfall, Temperature, and Pressure," *Mems. Indian Meteor. Department*, vol. xxi, Parts 10, 11, 12, 1915.

scepticism as to the reality of many of the connections claimed. In the present volume the assumption is made that during glacial periods the prevailing westerly winds of temperate latitudes not only existed, but were on the average far stronger than they are to-day. It is suggested, further, that this resulted in less well-developed warm poleward ocean currents of the type of the Atlantic Drift and Japanese Current in the Northern Hemisphere—surface currents arising from the prevalence of south-westerly winds between areas of sub-permanent high and low pressure—and that additional lowering of the temperature in middle and high latitudes resulted. To justify this rather surprising conclusion, it is further assumed that when the sub-permanent low-pressure systems such as the Icelandic " low " are strongly developed (*e.g.* at sunspot maxima), greater fluctuations of wind-direction occur, which prevent much general movement of the water. This last assumption might well be tested by what takes place at the present time. The experience of those accustomed to deal with synoptic weather charts for the North Atlantic would probably be found to lead them to adopt an exactly opposite view. To such it is a familiar fact that one of the most persistent types of pressure distribution is that in which the Azores anticyclone and Icelandic " low " are well developed, causing unusually strong westerly winds of long duration and mild weather in winter on the Atlantic seaboard of Europe and usually also over a large part of north-west Europe. It is when these centres of action are feebly developed that the complicated distributions of pressure occur, giving rise to frequent fluctuations of wind. Severe frosts are then common in Europe, and it is difficult to believe that the Atlantic Drift is better developed than at other times.

An illustration of the temperature changes brought about by increased storminess is furnished by a comparison between the temperatures recorded at certain stations just south of the Arctic Circle during the unusually stormy February of 1903 with those recorded in February of 1901 and 1902, both relatively quiet months over the North Atlantic. These months were selected because of the unusual contrast in the number of deep cyclones observed. Examination of the *Tägliche Synoptische Wetterkarten* of the *Deutschen Seewarte* showed that in the first-mentioned month 41 storms with readings below 980 millibars visited the North Atlantic, while for February 1901 and 1902 the numbers were 18 and 21 respectively. One may search through the data for many years without finding storminess greater than in February 1903. Not only was the total number of storms great, but for actual intensity a cyclone centred near Iceland on the 24th, which attained to the rare depth of 927 millibars, must be nearly, if not quite, unprecedented, while another on the 19th had nearly as low a pressure at the centre. During such a month the authors of this book would presumably expect a temperature well below the normal in a country like Scandinavia, where the winter is tempered by the warm ocean currents from lower latitudes—currents alleged to be weak at such times. The figures show, however, a reverse effect:

MEAN MAXIMUM AND MINIMUM TEMPERATURES AT HAPARANDA (SWEDEN) AND BODÖE (NORWAY)

—	Maximum.		Minimum.	
	Haparanda.	Bodöe.	Haparanda.	Bodöe.
February 1903 (stormy) . .	26° F.	37° F.	+ 6° F.	29° F.
February 1901 (quiet) . .	13° F.	28° F.	− 10° F.	19° F.
February 1902 (quiet) . .	18° F.	31° F.	− 10° F.	23° F.

Without exception maxima and minima alike are notably higher, instead of lower, during the stormy period. At Godthaab (West Greenland) the mean temperature at 8 a.m. was + 10° F. for the stormy year, but + 27° F. for the quiet years, a reverse effect due, no doubt, to the greater frequency of strong winds from the pole on the American side of the Icelandic "low" when this system is well developed; at Kioge Bay (East Greenland) there was no significant difference between the two means. One can detect in the figures for Godthaab a suggestion that increased storminess may actually mean lower temperatures in winter over a part of North America, but this effect is not a general one, nor is it safe to assume either a greater or a less transference of heat from equator to pole to result from an increase of storminess in temperate latitudes, without a thorough investigation.

Perhaps the greatest difficulty of all in the way of accepting the solar cyclonic theory of the origin of the glacial periods—this particular argument does not, of course, apply to the more recent climatic "pulsations"—is caused by the uncertainty among geologists as to the approximate distribution of the continents of those days, and of their elevation. Even if a hotter sun would actually to-day cause deeper cyclones over the region which is now the northern part of the North Atlantic, it is not safe to conclude that it would have had the same effect during the Pleistocene, still less can we conclude that it would have had such an effect at earlier periods; for without sure knowledge of the distribution of land and water in those days, one cannot tell even whether this area was then one of nearly permanent low pressure, as it is to-day. In conclusion, one is tempted to reserve judgment upon the question of the climatic pulsations of historic times. The suggestion that these modifications are due to changes in the sun reacting upon the "centres of action" appears to be a valuable one, and worthy of still further investigation. Before the more difficult subject of the glacial periods can be mastered, it will be necessary for the geologists to settle their disputes and provide the meteorologist with more solid ground of established fact. Until such time it seems useless to pile supposition upon supposition in the hope of building up a sound theory that can hope to survive the fresh discoveries of even a few generations.

REVIEWS

METEOROLOGY

The Air and Its Ways. By SIR NAPIER SHAW, Sc.D., F.R.S. [Pp. xix + 237, with 100 figures.] (Cambridge: at the University Press, 1923. Price 30*s.* net.)

IN this book Sir Napier Shaw has collected a number of his essays, written for the most part during recent years. Although the range of subjects is considerable, the work is not the complete textbook of meteorology that might perhaps have been expected from the title and from the high price of the publication. It is to be hoped that the author will some day publish such a textbook, a volume which will include the substance of *Forecasting Weather*, and in an abbreviated form the contents of the *Manual of Meteorology*, together with certain portions of the present work.

This book includes chapters on the teaching of meteorology in schools, on units of measurement, the organisation of the Meteorological Office, and essays dealing with the application of meteorology to agriculture. The portion of the book which is likely to be of the greatest interest to advanced students of meteorology includes Sections 7 to 9, entitled "The Cyclonic Depressions of Middle Latitudes," "The Structure of the Atmosphere up to Twenty Kilometres," and the Rede Lecture on "The Air and its Ways." To appreciate these sections, it is absolutely necessary for the reader to have a general knowledge of certain branches of physics, in particular of thermodynamics. In the first of these essays the author discusses the suggestion of Prof. V. Bjerknes that the cyclones of temperate latitudes are phenomena analogous to wave motion on a line of discontinuity between cold polar and warm equatorial air. He suggests as an alternative cause the rotation set up by local convection. The adoption of such a view appears at first sight to be merely a return to the theory widely accepted thirty years ago, which supposed cyclones to be regions of continually ascending warm air, the low pressure being due to the low density of the warm air—a theory discredited by the discovery that at considerable heights the cyclone is normally colder than its surroundings. He suggests, however, that the statement of the convectional theory has in the past been incomplete, owing to lack of recognition of a phenomenon which may be termed "eviction." When warm air rises convectionally there is reason to believe that eddies are set up between it and its environment, so that some of the surrounding air is dragged upwards. This air may be removed by suitable winds at great heights. This scouring action is termed "eviction." Air approaching the core of low pressure formed in this way will acquire rotation due to the deflection caused by the earth's spin, and ultimately the central column will become cold on account of the expansion of the inward-flowing air. The inertia of the revolving air is assumed to be sufficient to maintain the system for a long period after the convection which originated it has ceased. In a subsection the author meets certain objections to this theory. An apparatus is described which is claimed to imitate atmospheric conditions on a small scale, and in which a miniature cyclonic disturbance is apparently caused by eviction.

In "The Structure of the Atmosphere up to Twenty Kilometres" some useful diagrams are given which make it possible to study the thermodynamics of the general interchange of air between equatorial regions and high latitudes. In *The Air and its Ways* the idea of regarding the process as the working of a heat engine is developed, and it is shown that, if moist air rises at the Equator to the tropopause and passes to temperate latitudes while remaining at the tropopause, subsequently descending gradually to the level of the sea and returning Equatorwards as part of the trade-winds, this air passes through a cycle in which one-quarter of the heat taken in near the surface is converted into work (work spent in maintaining the motion against opposing frictional forces), the remainder being delivered up to the stratosphere, which plays the part of the refrigerator in the atmospheric engine.

Reference has been made earlier in this review to the high price of *The Air and its Ways*. In conclusion it should be said that the justification for this becomes more and more apparent the more closely the work is studied. Although the text amounts to only a trifle over two hundred pages, an immense amount of useful information is compressed into these pages and into the hundred illustrative diagrams. Some of these diagrams represent years of work and are almost indispensable to serious students of meteorology.

E. V. NEWNHAM.

Medical Climatology of England and Wales. By EDGAR HAWKINS, M.A. M.D. [Pp. xiv + 302, with 149 charts.] (London: H. K. Lewis, 1923. Price 25*s.* net.)

THIS book is designed to help doctors in the choice of places with suitable climates to which to send their patients. The climatic features of a large number of centres of population and health-resorts are set out diagrammatically for each month. These diagrams are supplemented by brief verbal descriptions, not only of the climate, but of the nature of the subsoil, the exposure of the town to winds of different direction, and the suitability of the place for various complaints.

The general arrangement of the book seems to be quite satisfactory and it should prove of great value to medical men, who can scarcely be expected to know, for instance, whether a place reputed to be cool and bracing has these qualities at all times of the year. The method of setting out the climatological data, however, is unscientific, and it appears doubtful whether there is sufficient practical advantage to justify the method. The method can be illustrated by a concrete example. From the diagram for Newquay we learn that the mean temperature there in November is on the average 8 per cent. above the normal for England and Wales as a whole, *i.e.* the numerical value of this mean is obtained by increasing the numerical value of the general normal by 8 per cent. Similarly the mean rainfall is quoted as 56 per cent. in excess of the general normal for that month. The natural conclusion to be drawn from these figures would be that Newquay in that month is not remarkable for mildness, but is, on the other hand, an extraordinarily wet place. The truth or otherwise of this conclusion could be tested only by examining diagrams for other parts of England and Wales. This being the case, the deviations from some standard "normal" might just as well have been given in degrees of temperature and inches of rainfall, etc.; the actual individual normals even might have been employed. It should be noted, however, that the practical drawbacks of the system would tend to disappear with continued use of the book, as it would soon be discovered that the significance of a certain percentage departure from the general normal varies according to the element under consideration.

E. V. N.

PHYSICS

Practical Heat. By TERRELL CROFT, M.A.I.E.E., M.A.S.M.E., M.I.E.S. [Pp. ix + 713, with 631 illustrations.] (London: McGraw-Hill Publishing Co., 1923. Price 25*s.* net.)

IN a book intended for those of limited mathematical knowledge, it is essential that all explanations should be simple. The author in his *Practical Heat* has given us a clear insight into the fundamental principles of heat as applied to engineering practice, particularly suitable for the non-technical student, for whom we believe the text is really intended. The copious and clear illustrations add not a little to the value of the work.

Some of the explanations, especially that on entropy, should appeal to many technical readers, to whom the usual definitions often convey so little meaning.

Throughout the text is given a series of equations from which various computations may be made; these, unfortunately, are not generally developed. Neither are their uses thoroughly discussed. One does not learn, for instance, on reading the chapter on combustion, how difficult it is to obtain an accurate analysis of a flue gas, or how to test whether the analysis found is feasible or not.

Bare formulæ as given may be sufficient for the non-technical engineer when dealing with the elementary portions of heat, but if applied to an engineering problem, without a knowledge of the practical limitations, will inevitably give quite valueless results.

Ll. E.

Elements of Glass-blowing. By H. P. WARAN, M.A., D.Sc., Ph.D., F.Inst.P. [Pp. x + 116, with 40 figures.] (London: G. Bell & Sons, 1923. Price 2*s.* 4*d.* net.)

GLASS manipulation plays so large a part in the work of a modern research laboratory that a book containing an adequate description of present-day technique is assured of a hearty welcome. The author of this book has exceptional qualifications for its preparation, for, in addition to his own very considerable personal experience, he has had the opportunity of observing the methods used at the Cavendish Laboratory and in the glass-working shops at Leyden University. Perusal of any of the processes which are described shows that, combined with this thorough knowledge of the subject, is the ability to explain to others precisely how the work should be done. The subject is considered under six heads—Cutting, Bending, Joining, Bulb-blowing, Enclosed Work, and Spinning. No information at all is given concerning the selection or the testing of different kinds of glass, the working of lead glass, and the treatment of old glass.

The production of neat and reliable apparatus of the most complicated character is largely a matter of certainty and precision in the three simple operations of bending, joining, and bulb-blowing. Of these three processes the last two are treated in great detail, and the complete novice will find his exact procedure carefully described and marked out with warnings of Do and Don't. The more difficult operation of bending is dismissed in a very summary fashion, and it is to be hoped that this defect will be remedied in the next edition. The chapter on enclosed work contains a long and careful description of the fitting of electrodes into vacuum vessels, and also deals with the construction of double-jacketed condensers and of Dewar flasks of various forms. The construction of ground joints and stop-cocks is discussed in the chapter on Spinning, together with work of the thistle-funnel type. The final chapter, intended rather for the interest of the reader than for

practical application, describes the commoner methods used in the manufacture of glass tubing, thermometers, flasks, etc.

The book is well illustrated, the price most moderate, and the type used for printing gives its pages a most pleasing appearance.

D. O. W.

Advanced Practical Physics for Students. By B. L. Worsnop, B.Sc., and H. T. Flint, M.Sc., Ph.D. [Pp. vii + 640, with 9 plates and 394 figures.] (London: Methuen & Co., 1923. Price 21s. net.)

The course of Practical Physics described in this book is based on the manuscript instruction papers used in the advanced physics laboratory at King's College, London. It contains an account of almost all the usual routine experiments, together with a few others which have not yet come into common use. It is up-to-date, and is certain to be well appreciated by students and teachers concerned with pass or honours courses. This appreciation will, however, be qualified by the opinions which users hold on three particular points: namely, the amount of theoretical work which should be included in a practical textbook; the extent to which it should anticipate inevitable experimental difficulties; and the degree to which it should encourage the student to be critical of his experimental methods and of his results. The first of these three points is also intimately connected with the size and cost of the book—matters of considerable importance to the average student of physics.

The authors have considered it desirable to go very fully into the mathematical theory of the experiments which they describe; probably as much as a quarter of the whole book is occupied in this way. It opens with an introduction of thirty pages devoted to an elementary exposition of the calculus, and contains in later pages the working of the formulæ for such items as the bending of beams, the flow of liquids and gases through tubes, the Fabry and Perot interferometer, and of the numerous methods used for the comparison of inductances. For the most part the student will find these things in his theoretical textbooks; repetition in the practical book increases its cost and decreases its portability—a no less serious matter.

The extent to which the textbook should help the student over his difficulties is not easy to decide. On the whole the authors may be said to have struck the happy mean: the only notable exception which the reviewer has noted is in the description of the experiment to determine $\frac{e}{m}$ and v for the cathode rays where no mention is made of the need for covering the Braun tube with well-earthed tinfoil. The absence of any critical discussion of many of the experiments is, however, a serious defect. To take a few examples at random. Jaeger's method and Sentis' method for surface tension are described without any suggestion of their inherent inaccuracy; so also is an experiment designed to determine the variation of the viscosity of water with temperature. The specimen data given on p. 166 relating to the time-period of an oscillating disc obviously need comment, and in the biprism experiment it is taken entirely as a matter of course that the two virtual images of the slit should lie in plane of the slit.

Among the many excellent features the chapter devoted to ammeters, voltmeters, and galvanometers deserves special mention, and also some of the more novel experiments, *e.g.* Poynting's method for the determination of the radiation constant, S. W. J. Smith's experiment on the variation of the residual magnetism of a carbon-steel rod with temperature, the Lecher wire experiment, and an application of Newton's rings to the determination of the change in length of an iron bar on magnetisation. The obvious omissions are not very numerous, but certain of them are rather surprising.

There is no reference to any modification of Rankine's method for the viscosity of gases, to the use of the phonic wheel for the frequency of a tuning-fork or to Carey Foster's method for the determination of mutual inductance in terms of resistance and capacity. There is also no reference to any of the practical arts such as soldering or glass-blowing, and the section on radio-activity is inadequate.

There can be no doubt that a second edition of the book will be necessary at an early date, when it may be hoped that some of these omissions may be rectified. At the same time the plates, which serve no useful purpose, might be left out along with much of the quite commonplace mathematical theory, so as to reduce the size and cost, which, after all, are the most serious defects.

D. O. W.

CHEMISTRY

Alcoholic Fermentation. By ARTHUR HARDEN, Ph.D., D.Sc., F.R.S. *Monographs on Biochemistry.* Third Edition. [Pp. iv + 194.] (London: Longmans, Green & Co., 1923. Price 6*s*. 6*d*. net.)

WHILE the main headings in the present edition are the same as in the previous one, there has been some rearrangement and a new chapter has been added on the reducing enzyme of yeast; in this chapter the author gives a résumé of the more modern views on the mechanism of reduction and also a brief reference to glutathione. The function of the reducing enzyme is discussed more fully in the chapter on the chemical changes involved in fermentation, where also will be found an account of the three forms of fermentation described by Neuberg. The chapter on the action of inhibiting and accelerating agents has been considerably extended and includes much of the author's own more recent work on this subject. The increased amount of attention which has been devoted to the subject of alcoholic fermentation within the last few years is reflected in the growth in the size of the list of references, which now extends over thirty-one pages as against eighteen in the last edition.

P. H.

A Treatise on Chemistry. By Sir H. E. ROSCOE, F.R.S., and C. SCHORLEMMER, F.R.S. Vol. II—The Metals. Sixth Edition, revised by B. MOUAT JONES, M.A., and Others. [Pt. I, pp. xvi + 830; Pt. II, pp. viii + 831-1566.] (London: Macmillan & Co., 1923. Price 50*s*. net.)

THE first edition of this classical treatise on Chemistry was issued in 1878 as an attempt to provide a suitable textbook for students at secondary schools and universities, as at that date a satisfactory work of the type required was practically non-existent.

The original edition was at once successful, and has indeed served as the prototype for most subsequent textbooks. Through no fault of Schorlemmer's, the volume on organic chemistry failed to find the same welcome as the two inorganic volumes; this, indeed, was due to the singular apathy shown towards organic chemistry in this country in the past, with the disastrous results seen during the war, with its famine in dyes, drugs, and explosives. The inorganic volumes have, however, become the standard textbooks in many schools and colleges for several decades and have passed through five editions, the present one being the sixth.

Principal Mouat Jones has been fortunate in having had the assistance of several distinguished colleagues in the laborious work of revision. The introductory chapter on valency, the classification of the elements, atomic numbers, isotopes, atomic structure, and spectrum analysis has been revised by Mr. L. J. Hudleston, and Mr. T. V. Barker has again dealt with

the section on crystallography. Groups I and II have been brought up to date by Prof. H. V. A. Briscoe; Alkali Manufacture by Prof. J. R. Partington; Groups III and V by Dr. H. F. V. Little; the Rare Earths and the Titanium Group by Prof. J. F. Spencer; Group VI by Mr. A. A. Eldridge; the Germanium Group and Manganese by Mr. C. R. Bury; Group VIII by Dr. O. R. Howell; while Prof. C. O. Bannister has once more revised the metallurgical portions, and Dr. Makower the section on the Radio-active Elements. For the purpose only of convenience in handling and binding, the book has been divided into two parts, the pagination being continuous. Whilst the outstanding results of the "New Chemistry" and the "New Physics" have been referred to and are well summarised, it has been felt that anything like a complete account of the new theories would be out of place, so that, although the work is thoroughly up-to-date, the general style and character of the book have been maintained.

On the whole the balance is well preserved, though in one or two places one feels that a relatively unimportant point has been stressed whilst leaving others somewhat neglected; thus over a page is devoted to the sodium bronzes, an interesting but not very important subject, whilst the chromium-steel alloys, which form the basis of the highly important "rustless" steels, have to be contented with two or three lines. Mention, again, might be made of the uses of magnalium for airship construction, whilst the use of calcium carbide for the large-scale manufacture of alcohol, acetic acid, and acetone might be noted.

There can be no question, however, that in their present form the volumes are thoroughly readable and up-to-date and, to say no more, fully maintain the tradition and high standard of the earlier editions. It is perhaps almost superfluous to add that the contributions of printer and publisher are fully adequate, the printing and arrangement being excellent in every way.

F. A. MASON.

Thermodynamics and the Free Energy of Chemical Substances. By GILBERT NEWTON LEWIS and MERLE RANDALL. [Pp. xxiii + 653.] (London: McGraw-Hill Publishing Co., 1923. Price 25*s*. net.)

THIS book will arouse a wider interest and will appeal to a much larger audience than is customary amongst treatises on thermodynamics. To the student of thermodynamics it presents the subject from a new aspect, which will well repay a critical study. To the chemist with his interest in the fundamental problems of chemical reaction it marks an important step in the development of chemical theory, an advance which is worthy of comparison with the contributions of Helmholtz and van't Hoff during the last century. However, it is in its practical aspects that its influence will be most weighty, for Prof. Lewis and his collaborators have developed an instrument of research which will render possible a fuller utilisation of the accumulation of physical and chemical data in the manifold operations of industrial chemistry.

Essentially a book on chemical thermodynamics, the conventions employed have been defined solely with regard to convenience in calculating the free energy of chemical reactions. These conventions, which make use of the conceptions of partial molar quantities, fugacities, and activities of chemical systems, are clearly defined in the introduction, and are illustrated by numerous examples. The latter would, however, have been more useful had the answers been given. Although the new notation is consistently used throughout the book, it is felt that the authors are scarcely justified in making such radical changes as that of naming the thermodynamic potential, "the free energy." The term "free energy" was defined by Helmholtz

as the total external work done in a reversible process, whereas Lewis gives as a definition of "free energy" the equation $F = E + PV - TS$ (p. 155). The latter function (Sackur, p. 174) is normally called the thermodynamic potential. Such changes merely add to the already existing confusion.

The importance of the authors' treatment of the processes occurring in solution cannot be over-emphasised. Values are given for the partial molar quantities, the activity coefficients of electrolytes which are directly available for the calculation of the free energy of these processes. The conception of activity coefficient is much too abstract for its employment in teaching the rudiments of physical chemistry, and one feels regret that the simpler conceptions, based on the kinetic theory, are being ousted by conceptions that have little or no physical basis. The physical causes underlying the breakdown of the simple relationships discovered by Raoult, Arrhenius, and van't Hoff are known qualitatively, and a discussion of the problem of activity from this aspect would make the transition from the old to the new much easier.

The book is remarkably free from errors, and much care has been exercised in its preparation. The equations are not only numbered, but the chapters in which they occur are given, and this facilitates reference. Convenient summaries are given, and the condensed summary of thermodynamic formulæ (p. 164) is especially useful. By the employment of graphical methods many laborious calculations are avoided, and the adoption of these methods gives the book a value for readers with an elementary knowledge of mathematics.

The third law of thermodynamics is developed on the basis of the convention that the entropy of a pure crystalline substance is zero, and the authors have given a "concreteness" to the entropy concept which should render possible its more frequent employment in chemistry.

W. E. G.

The Phase Rule and its Applications. By PROF. ALEXANDER FINDLAY. [Pp. xvi + 298, with 158 illustrations.] (London: Longmans, Green & Co., 1923. Price 10s. 6*d*. net.)

THAT Prof. Findlay's book on the phase rule has now reached its fifth edition is sufficient evidence of its popularity and usefulness. Within the scope of 298 pages the author has compressed a vast amount of useful information in a very readable form. Perhaps compression has been carried too far in some instances, as, for example, in the explanation of Jänecke's graphical methods on pp. 255–6, where insufficient detail is given to enable the student to derive the composition of a four-component system from fig. 138. Parts of the book have been rewritten, especially those relating to the diagrams of sulphur and phosphorus. Although the pseudo-binary nature of these systems is discussed, the author has very wisely omitted the complex diagrams of Smits on these elements. The iron carbon diagram has been modified in accordance with the recent work of Ruer and Goerens on the equilibrium relations of δ ferrite. The importance which the phase rule has assumed in mineralogy has been recognised by the inclusion of a section on the thermal studies of minerals, in which the system CaO, SiO_2, Al_2O_3 is examined in detail. The generalisation of Oswald's, p. 60, can hardly be dignified by the title "the law of successive reactions," for this is very frequently contradicted by exceptions, and it is surely incorrect to say that "it *explains* the formation of metastable forms of monotropic substances."

The additional matter included in the new edition is very welcome, and the textbook is still the best elementary treatment of the phase rule in the English language.

W. E. G.

GEOLOGY

Geomorphology of New Zealand. Part I—Systematic. An Introduction to the Study of Land-forms. By C. A. Cotton, D.Sc., F.N.Z.Inst., F.G.S. [Pp. x + 462, with 442 figures.] (Wellington, N.Z., Dominion Museum, 1922. New Zealand Board of Science and Art, Manual No. 3. Price 22s. 6d., cloth-bound.)

The main title of this book tends to obscure the fact that it is the finest general introduction to the science of geomorphology that has yet been written. Fortunate indeed are the geographers and geologists of New Zealand in having this book written to meet their special needs. Notwithstanding the fact that the great majority of the examples and illustrations are drawn from New Zealand, the geomorphologists of the world will find the book absolutely relevant, and its principles easily applied to the special conditions of their own lands. Prof. Cotton has, of course, absorbed much from the great master of the science, Prof. W. M. Davis, of Harvard, who has so happily wedded geography and geology in the study of land-forms; but he has added to the discussion many of his own original ideas.

The book begins with the study of the normal cycle of erosion and the land-forms which are successively developed under the continued influence of rain and rivers. The forms associated with faults, and with accumulations of waste, are then discussed. In connection with the former the fine "block-mountain" features of New Zealand are described. From Chapter XVI onwards the effects of various accidents and interruptions on the normal cycle are dealt with. The accidents may be climatic, leading to refrigeration and producing glacial erosion and accumulations, or to aridity, introducing æolian erosion and accumulations. Again, they may be volcanic, leading to extensive modification of the pre-existing land-forms. The interruptions of a cycle are usually due to earth-movements, but may sometimes be ascribed to fluctuations of the sea-level. The book ends with a study of marine erosion and coastal profiles, in which good use is made of the work of Gulliver and Johnson.

The matter of the book is well arranged, the manner logical, and the writing extremely lucid and concise. A special word of praise must be given to the abundant and well-chosen illustrations, mostly photographs of New Zealand scenery; but there are also many beautifully clear line drawings, and generalised land-form diagrams. A good series of reading references and a full index add much to the value of the book. The Dominion Museum may be congratulated on an excellent piece of book-production. We shall await with great interest the promised second volume, which is to contain a regional treatment of New Zealand land-forms in relation to the later geological history of the Dominion.

G. W. T.

Elementary Geology, with Special Reference to Canada. By A. P. Coleman, M.A., Ph.D., F.R.S.; and W. A. Parks, B.A., Ph.D., F.R.S.C. [Pp. xx + 363, with 197 figures.] (London and Toronto: J. M. Dent & Sons, 1922. Price 15s. net.)

The type of geological textbook in which the illustrations and examples are drawn from one particular continent or country receives a good exemplification in the book under review, which has been written by its distinguished authors with the Canadian student, mining engineer, and general reader in view. Besides its local advantage, this method has the additional merit of revivifying the whole body of the science, because its outlook, methods, and illustrations must be fresh and new to the jaded readers of

the old type of textbook produced under more sophisticated conditions. We gladly miss the old cliches in manner and illustration which were to be found in textbook after textbook of the older dispensation.

A little more than one-third of the book is devoted to the discussion of the general principles of geology. Chapter II deals with minerals and rocks, and is cast practically into the form of a glossary, the reader being advised to consult the standard works on mineralogy and petrology. The form of this chapter, however, makes it almost useless in a book of this kind. It would have served better to have introduced a more generalised treatment of minerals and rocks as the essential basis for the study of geology. The remaining chapters in this part deal with Dynamical Geology and Structural Geology. The critically minded student may ask how the foundered part of the "graben" illustrated in Fig. 56 arrived in its present position. This is a type of diagram which needs revision in most textbooks of geology.

The section devoted to Historical Geology is compounded of Stratigraphy and Palæontology, and the latter is done on a scale somewhat incommensurate with the scale of the book. It provides, however, an excellent compendium of Canadian palæontology, which will be of especial value to the non-Canadian worker.

The chapter on "The Making of the World" contains a concise and judicial contrast between the nebular and planetesimal theories of planetary origin, with the balance of opinion in favour of the latter. Since more than half of Canada is occupied by Archæan formations, problems concerning the origin and age of these are treated with special fullness. The authors comment on a common attitude in historical geologists of regarding igneous and orogenic episodes as accidental features unfortunately interrupting the ordered succession of events, and not worthy of similar detailed study.

The style adopted in the book is simple and concise almost to baldness. The illustrations throughout are excellent in quality, and most of them are brand-new. The absence of an index is only partly compensated for by a detailed table of contents. The book is well printed and free from typographical errors; but it is very heavy (avoirdupois) in relation to its size.

G. W. T.

BOTANY AND AGRICULTURE

Practical Plant Ecology. By A. G. Tansley, M.A., F.R.S. [Pp. 228, with 15 text-figures.] (London: George Allen & Unwin. Price 7*s.* 6*d.* net.)

The earlier part of this volume embraces a brief survey of British plant communities followed by an account of methods of investigation such as mapping (primary survey, grids, charts, and transects, etc.); the study of habitat factors, climatic, edaphic, biotic; and a concluding section on ecological work in schools. An Appendix deals with "Life-forms," survey methods, photographic methods, chemical determinations, and a classified list of books and papers. The last-named includes most of the published papers on British Vegetation, and it is to be regretted that it is not complete in this respect, the more so as some of the omissions are not easily accounted for. Moreover, we venture to think the value of the body of the work would have been considerably enhanced by much more frequent reference to the original papers on which the text is based.

As an elementary guide to the field study of Plant Ecology this little book supplies an obvious gap in our botanical literature.

We are glad to note that the author emphasises the real significance of ecological work as depending not so much on the nature of the problems

studied as upon the method of attack. To use the author's own phraseology it is a means of approach rather than a special branch of Botany.

Ecology is essentially synthetic, not merely with respect to the various branches of Botany, but in respect to other sciences as well—Chemistry, Physics, Geology, Meteorology, Zoology, etc. It is the correlation which it demands of the results achieved, in very diverse fields of research, which constitutes at once the chief merit of ecology and the chief difficulty of its pursuit in an age of extreme specialisation. This is the foundation of its power and charm in the hands of the competent teacher; but ecology is essentially a practical subject in which hypotheses must be checked by field observation and experiments, and in which quantitative data and meticulous accuracy are especially necessary.

Probably in no domain is it easier for the counterfeit of superficial observation to pass currency as the genuine coin from the mint of knowledge, and for this reason we heartily welcome a volume which will tend to promote a greater accuracy and more definite purpose in ecological observation.

E. J. SALISBURY.

The Ferns (Filicales). Treated comparatively with a view to their natural classification. Volume I: Analytical Examination of the Criteria of Comparison. By F. O. BOWER, Sc.D., LL.D., F.R.S., Regius Professor of Botany in the University of Glasgow. [Pp. x + 359, with 309 illustrations and frontispiece.] (Cambridge: at the University Press, 1923. Price 30*s*.)

PROF. BOWER early interested himself in the Ferns, and one of his very first papers was written on the nature of the leaf in these plants. The outcome of this interest has been a remarkable series of papers on these forms, papers in which their relationships and interrelationships have been one of his most important concerns. The volume under review is the first of two which will deal with the Ferns mainly from the point of view of their natural classification, and critically examines the data of comparison, while the second volume will deal in a constructive manner with these facts. Every organ and structure of the sporophytic and gametophytic generations is examined in detail, and comparisons are drawn when possible between these and corresponding structures in allied and fossil plants, so that the volume contains a large amount of information regarding the structure of the Ferns and their allies. The point of view of the treatise necessarily excludes any detailed treatment of the physiology and ecology of these plants, but at the same time it leaves one impressed with the fact that, while so very much is known of their structure and affinities, comparatively little is known of the former aspects of the study, even if stelar theories do seek to explain structure largely in terms of function. The chapter on "Size, a Factor in Stelar Morphology," based on an address delivered by Prof. Bower in 1920, is all the more interesting as it directs attention to work along experimental lines; for example, when it quotes from some work of McLean Thompson on the anatomy of a plant that had been starved, with consequent change in size of the stelar tissues, as would indeed have been expected, and with considerable structural alterations as well. It is to be hoped that the book will stimulate research on Fern physiology and ecology.

A number of quite new observations are recorded; amongst the most interesting of these is the statement that in *Notholæna trichomanoides* similar hairs have been found in the gametophytic and sporophytic generations.

There are cases amongst plants where the two generations are almost exactly alike, and in such we are fairly well acquainted with the more obvious features of similarity between the generations; but where these are distinctly different we have no measure of the quantitative nature of these differences,

and generally it may be said that we know very little of the inheritance of gametophytic characters. This helps to make the case, observed by Mr. Boodle, an extremely interesting one.

The book is fully illustrated, many of the illustrations having been specially prepared for it, and it need scarcely be said is an authoritative and clear statement of this aspect of the subject.

E. M. C.

Textbook of Agricultural Bacteriology. By F. Löhnis, Ph.D., and E. B. Fred, Ph.D. [Pp. ix + 283, with 66 figures.] (London: McGraw-Hill Book Co., 1923. Price 15s. net.)

Dr. Löhnis's *Vorlesung über Landwirtschaftliche Bakteriologie* is a textbook so well known to students and research workers that the appearance of a revised English edition by Dr. Löhnis and Dr. E. B. Fred will be especially welcomed. The book opens with a general description of bacteria and other micro-organisms and an account of their activities. Chapters on bacteriological technique are included here, and the special methods in use in dairy and soil bacteriology are dealt with in the later chapters concerning these fields of work. The book is not a laboratory manual, and consequently the authors deal, not with the details but rather with the principles underlying bacteriological methods. The second half of the book deals with bacteriology in its especial connection with the practice and problems of agriculture. The authors discuss the part taken by bacteria in the ripening of hay, in the formation of silage and farmyard manure, in the disposal of sewage, in dairy products, and in the soil. Although the book is concerned primarily with bacteria, the authors also refer to the activities and influence of other micro-organisms such as fungi, algæ, and protozoa. In some instances the importance of these other organisms is perhaps too little emphasised. Our knowledge of the part they play is at present all too small, but recent work tends to show how closely their activities are interrelated with those of the bacteria. In the soil, for example, the close connection between the activities of bacteria and protozoa has been shown by recent work at Rothamsted, where the rapid changes in numbers of bacteria and of active amœbæ that occur in a field soil have been shown to be inversely related. Until a more accurate technique is evolved for estimating the quantity of filamentous fungi in the soil their relative importance cannot be estimated, while the discovery of an abundant algal flora in field soil has introduced another factor of unknown importance into the problem. The trend of modern research indicates that in soil, at any rate, we cannot consider bacteria by themselves, but must study them in their relation to other groups of the soil population.

H. G. Thornton.

ZOOLOGY

Elementary Zoology. By Oswald H. Latter, M.A. [Pp. ix + 333, with 114 illustrations.] (London: Methuen & Co., 1923. Price 12s. net.)

Mr. Oswald Latter has been a teacher of Biology during more than thirty years; his book covers the syllabus in Zoology prescribed by the University of London and the Northern Union Joint Board for their respective Matriculation examinations. Leaving aside the question as to whether University Professors want their freshmen to have learnt a hotch-potch of Zoology before they "come up," it must be said frankly that Mr. Latter's book gives a clear, readable account of elementary Zoology and Physiology, but there is a great deal of the book, and some parts of it are rather advanced. One

cannot believe that the ordinary boy of from sixteen to eighteen could understand the book unless he was taught by a properly trained zoologist with the aid of specimens and slides. Mr. Latter is a trained zoologist, and his boys, unlike the majority, have the advantage of being taught with the aid of zoological collections. Many Universities have entrance prizes and sizarships in Biology, and a book for the purpose is badly needed. In Mr. Latter's present volume the reviewer sees the possibility of such a book; but the author's *Elementary Zoology* is too bulky, too expensive, and too advanced for this purpose. No boy unaided by a special teacher could understand Mr. Latter's book. Some of the chapters are quite originally written and would make excellent material for a smaller work for entrance examinations. The author has taken much trouble with the illustrations, which are clear and nicely produced.

J. Bronté Gatenby.

The Physiology of Reproduction. By Francis H. A. Marshall, Sc.D., D.Sc., F.R.S., with contributions by William Cramer, Ph.D., D.Sc., M.R.C.S., L.R.C.P., James Lockhead, O.B.E., M.A., M.D., F.R.C.S.E., Cresswell Shearer, M.D., Sc.D., F.R.S. Second and revised edition. [Pp. viii + 770, with 189 illustrations.] (London: Longmans, Green & Co., 1922. Price 36s. net.)

This is the only book of its kind in any language, and its usefulness to the student and research man in any physiological and embryological subject cannot be overestimated. The fact that the work is not aimed at convincing one of the correctness of any special theory or hypothesis, but is a mine of solid learning, makes it indispensable. The reviewer has read the new edition with pleasure, and treasures the volume especially among his books: no Pathological, Physiological, or Biological Department should be without it. Naturally there are many branches which the specialist will find rather bare of new leaves, but in the splendid broad view which the whole book gives he will find the excuse for overlooking the lacunæ. Certain reviewers have deplored the want of emphasis in the acceptance of some of the more modern conceptions of the cytological science; but the present reviewer would regret to see any change made. Prof. Marshall is to be congratulated on this splendid volume.

J. Bronté Gatenby.

Heredity in Poultry. By Reginald Crundall Punnett, F.R.S. [Pp. viii + 204, with 12 plates and 28 figures.] (London: Macmillan & Co., 1923. Price 10s. net.)

This little book is dedicated to William Bateson, whose experiments with poultry offered the first demonstration of Mendelian heredity in the animal kingdom. This is a cheerful saying in these days of *Drosophila*! Prof. Punnett in a modest preface tells us how Mr. Bateson came to offer him a share in the work being carried out nearly twenty years ago at Grantchester. The work begins with an elementary introduction and step by step goes on to explain all the discoveries made by Punnett, Bateson, and their co-workers at Cambridge. There are chapters on "Sex-Linked Inheritance," "Secondary Sexual Characters," "Linkage," and so forth. The book gives a complete account of all the modern work on crossing and bird breeding, and some of it will be rather difficult for the layman to understand, but Prof. Punnett's researches will provide an excellent jumping-off ground for further

work on Mendelism and Sex. The bibliography is extensive. This book, together with Marshall's *Physiology of Reproduction*, should be read carefully by all those who are interested in Heredity and Sex. The illustrations are nicely executed, several of the plates being in colour.

J. Brontë Gatenby.

Asterias. By Herbert C. Chadwick, A.L.S., L.M.B.C. [Pp. xiii + 63, with 9 plates.] (Liverpool: The University Press; London: Hodder & Stoughton, 1923. Price 4*s*. 6*d*. net.)

This monograph is the fourth of Mr. Chadwick's Echinoderm memoirs, and it keeps well up to the standard of this careful worker. All teachers welcome it, for only they really know how much it is needed in the laboratory; and not only the University student, but every naturalist, will find it a very substantial help in the study of the starfish.

The illustrations are beautiful, especially the larger drawings showing the skeleton and internal anatomy. The figures of histological detail are not quite so happy, but this is more the fault of reproduction than of the artist, and the fact that they are on rather too small a scale.

On controversial points the evidence from every point of view is carefully considered and the author's conclusions from his own dissections usually given. With regard to the much-discussed axial organ, he is inclined to support Gemmill in regarding its function as a hæmal one, and apparently does not agree with MacBride and others that the organ is the original seat of the genital cells. The clear diagram of this system on page 31 is of special value.

Only such embryology is allowed in the monograph as can be seen in the whole organism, alive or mounted, and no histology of these is undertaken. This seems wise, as the student taking Asterias as a type in a zoological course does not need to study the embryo more minutely, and, as the author states, it is easy to refer to larger works when more detail is wanted. A clear description is given of the development from the egg through the various larval stages up to metamorphosis. Here again, perhaps, the reproduction does not do justice to the drawings, which are small for the detail that is put into them and somewhat crowded on to the plate.

Mr. Chadwick is to be congratulated on the completion of so useful a piece of work, and it is to be hoped that he will add many more momoirs to the series.

Marie V. Lebour.

Manual of Entomology: With Special Reference to Economic Entomology. By Prof. H. Maxwell Lefroy. [Pp. xvi + 541, with 4 plates and 179 text-figures.] (London: E. Arnold & Co., 1923. Price 35*s*. net.)

This book differs considerably from other textbooks of Entomology, and is intended primarily for the student of the economic aspect of the subject. It is planned to teach him how to recognise the live insect in the field, to decide its sex and habits, to understand how to control injurious species, and to provide him with references which will indicate the sources where fuller information is available. There are no introductory chapters, but this appears to be accounted for by the fact that the book is based on lectures which form a part only of a full course in economic entomology. The student is consequently introduced straight away to insects order by order. Its

method of preparation is somewhat unusual, since several past or present students (eight in all) of Prof. Lefroy's department have been enrolled as collaborators in the preparation of a large part of the volume. The different chapters, apparently for this reason, are of unequal merit: there are also a considerable number of misprints, seemingly due to too superficial proof-correcting, and there are a certain number of serious mistakes affecting matters of fact.

In the preface Prof. Lefroy deplores the continual nomenclatorial changes that go on, and has endeavoured to follow a course more or less between conservatism and radicalism in this matter. He considers that every student should have a collection of insects to work with, and also specimens for examination and dissection, as he does not much believe in illustrations.

The method of arrangement of the volume has been to divide it up into sections, each section being devoted to a separate order of insects. A diagnosis of each order is given at the commencement of its section, and there follows an account of the general structure and habits based upon its typical representatives. Each of the families of the particular order concerned is dealt with in varying detail, with comments on certain of the more notable species, and often with remarks on controlling some of the noxious forms. A good deal of useful information is brought together by this method, and a very considerable amount of recent literature has been taken advantage of, which makes the book reasonably up-to-date. The four plates are clear half-tone illustrations and most of the text-figures are original.

A. D. I.

Practical Bee Anatomy: With Notes on the Embryology, Metamorphoses, and Physiology of the Honey Bee. By ANNIE D. BETTS. [Pp. 88.] (The Apis Club, Benson, Oxon, England, 1923.)

THE Apis Club Library has been initiated with the idea of providing manuals on the various aspects of the science and practice of bee culture. The present little book is the first in that series, and is intended principally as a guide to the practical anatomy of the bee. Since the first volume of Cheshire's admirable work *Bees and Beekeeping* is now out-of-date on account of the large amount of new knowledge that has accumulated, Miss Betts has had in mind a more modern treatment of the subject. With this aim in view she has included "a survey of bee anatomy from the egg stage onwards, based mainly on the work of Nelson, Snodgrass, and Zander." Her little book is extremely well done, and it everywhere shows evidence of being the work of a trained zoologist. Notwithstanding its small compass, it is very closely printed and contains practically everything of importance relative to the structure and physiology of the adult bee. The most enthusiastic worker will also find all that is required in the way of advice on dissection, methods of technique, and references to literature. It is something more than a compilation, for the authoress has evidently carried out independent investigation in writing the book, and judiciously weighs the "pros and cons" in instances where there is divergence of opinion. We have, therefore, no hesitation in recommending her book to the expert beekeeper, the entomologist, and to anyone who desires to understand thoroughly the structure of the bee. The final pages are occupied by twelve plates comprising 61 figures. These are scarcely up to the standard we should have expected from the letterpress. They are undoubtedly accurate, but would have gained in value if more time had been expended ontheir execution: as it is they are somewhat of the nature of notebook sketches.

A. D. I.

ANTHROPOLOGY

Tutankhamen and the Discovery of his Tomb. By PROF. G. ELLIOT SMITH, F.R.S. [Pp. 123, with 23 illustrations.] (London: George Routledge & Sons, 1923. Price 4*s*. 6*d*. net.)

It is usually unwise to put into book form a series of articles which have appeared in a newspaper. The newspaper article is a comment on the news of the day and therefore in its very essence ephemeral, whereas a book presupposes a more weighty and considered treatment, especially of a scientific subject. Prof. Elliot Smith has been ill-advised in yielding to the request of his friends to put his articles into book form. The temptation to do so is always great, but it should be firmly resisted. This is specially true as regards the subject of this book; for, until the excavation is complete and Mr. Howard Carter's authoritative account is published, all books on Tutankhamen can only be either speculative or a mere recapitulation of the few historical facts already known; in either case they take the "bloom off the peach" of Mr. Carter's future publication. It may be remarked that, with one exception, no Egyptologist of note has written a book on Tutankhamen; they have left Mr. Carter an absolutely clear field. Prof. Elliot Smith's book adds, and can add, nothing to our knowledge of the period; for the only department of Egyptology in which he has done original work—mummification—is not yet required. If and when the mummy of Tutankhamen is found, then Prof. Elliot Smith's great experience will be of value both to the specialist and to the general public.

M. A. MURRAY.

MEDICINE

Dental Anatomy and Physiology. By JOHN HUMPHREYS, M.D.S., F.S.A., F.G.S., F.L.S.; and A. W. WELLINGS, M.D.S., L.D.S. [Pp. viii + 323.] (London: Edward Arnold & Co., 1923. Price 16*s*. net.)

It is rare to find a book designed to meet the requirements of students working to a syllabus which is as free as the volume under review from the defects usually associated with such books.

This new dental anatomy presents its subject-matter in a broad and scientific spirit, and contains many illustrations of the value of comparative studies in the elucidation of problems of human anatomy and physiology.

In Part I, dealing with the human teeth, the text is profusely illustrated with excellent photographs. The book is thoroughly up-to-date, and debatable points requiring further investigation are presented in a lucid and undogmatic manner.

In Part II, the comparative section, the classification of animals adopted is rather crude and antiquated; but its use is perhaps justified as an approximation sufficient for the needs of those for whom it is intended.

The book can be recommended to dental students as a compact but comprehensive manual of the anatomy of teeth.

J. H. W.

Practical Physiology. By E. P. CATHCART, F.R.S., D. NOEL PATON, F.R.S., and M. S. PEMBREY, F.R.S. [Pp. xii + 344, with 206 figures.] (London: Edward Arnold, 1922. Price 18*s*. net; or in 2 vols., 10*s*. 6*d*. each.)

This is a comprehensive course of experimental physiology and chemical physiology in one volume suitable for medical students.

The section on experimental physiology occupies 175 pages and is conveniently divided into an elementary and an advanced course.

The elementary course begins with a study of the special senses as the

instruments of knowledge, thus differing from the majority of works on experimental physiology in a more philosophical if less direct method of approach.

The chapter on nerve and muscle is wisely shorn of detailed descriptions of electrical apparatus, which occupy an appendix, and the student is not confronted by a barricade of obsolete galvanic batteries before he can begin the study of contracting muscle.

An account of the polygraph would improve the chapter on the circulation, in view of the clinical interest of the implement.

Dr. Hurst contributes an exceedingly useful chapter on the investigation of the movements of the alimentary canal, and fills a conspicuous gap in experimental textbooks. Altogether, this section is eminently practical and well written.

The section on chemical physiology, while adequate in extent, is not as fresh in treatment as the previous section.

There are many minor statements open to question. For example (p. 193): ". . . this solution, like all colloidal solutions, gives a persistent froth on shaking."

The account of the acidity of gastric juice suffers from some confusion from the attempt to incorporate the modern method of expressing acidity as hydrogen-ion concentration with the old notion of total acidity, total hydrochloric acid, and free hydrochloric acid.

In the chapter on urine, it may be doubted, in view of Folin's results, if the acidity is entirely due to acid phosphates. The vague remark is once more reproduced: ". . . an amphoteric solution turning red litmus blue and blue litmus red." Surely an amphoteric solution in the region of pH 7 can better be described as turning red litmus purple and blue litmus purple, the equilibrium tint of each being the same?

. The simple and valuable urease test for urea might suitably be included in the elementary section. Incidentally, dry heat does not split urea into ammonia gas and biuret, but into ammonia and cyanic acid. Biuret is only formed by the combination of the cyanic acid with some of the urea which has escaped decomposition.

In the Rothera test for acetone, it should be mentioned that the test is also given by aceto-acetic acid. And in describing the ferric chloride test the method of eliminating fallacies due to phenol or salicylic acid by boiling the solutions should be stated.

The advanced course in chemical physiology is much better, and is up-to-date without being overloaded.

Altogether, the complete work is eminently practical and readable.

W. R. F.

MISCELLANEOUS

Elements of Optical Mineralogy. An Introduction to Microscopic Petrography. By H. N. and A. N. Winchell. Second Edition. Part I: Principles and Methods. [Pp. xv + 216, with 250 illustrations.] (New York: John Wiley & Sons; London: Chapman & Hall, 1922. Price 17*s*. 6*d*. net.)

The second edition of this book is to be welcomed, as the amount of literature on this subject available in a form suitable for students is limited. In the first edition, the subject was dealt with somewhat less completely, in fact the second edition is the first part of the projected work, Parts II and III being promised at a later date. It is well known that the birth of modern petrography was due to the labours of Sorby, but that later accurate and useful methods were devised by well-known French workers, and these are fully described in the book. The first four chapters are devoted to a descrip-

tion of crystals, their physical and chemical properties, and to methods of determining their characteristics. An elementary treatment of optics follows, and it is to be regretted that more space has not been devoted to this part of the subject, as on this are based the technical methods that are described. It might be urged that reference should be made to standard works on light, but the student will not always appreciate this necessity. The part on the microscope is also inadequate, and in places so loose in expression as to be misleading. An example is the statement that magnification is the product of the magnifying power of the objective and of the ocular, no mention being made of the effect of changes of tube-length. Most of this chapter is taken up with a description of various types of microscopes, information that could as easily be obtained from makers' catalogues. The methods of preparation of material for microscopic study, together with that devoted to the determination of refractive indices, are adequately described. The remaining portion of the book is devoted mainly to the observation of minerals in polarised light. It is recognised that this is the most important branch of such work, constituting, in fact, the main point of difference between the use of a petrographic and an ordinary microscope. The subject is treated with commendable lucidity, particularly having regard to its difficulty.

The illustrations provided are numerous, and although they are, in most cases, of the type usually seen in such circumstances, are carefully chosen and well reproduced. The remaining parts of this book may well be awaited with interest.

J. E. B.

Foundations, Abutments, and Footings. Edited by George A. Hool, S.B., and W. S. Kinne, B.S., Professors of Structural Engineering, University of Wisconsin; assisted by Horace S. Barker, S.B., Chicago. [Pp. xiv + 392, with 185 illustrations.] (New York and London: McGraw-Hill Book Co. Inc. Price 20s.)

The editors-in-chief state that this is the first of a series of six volumes, which will fully present the elementary theory of the principal kinds and types of modern engineering structures. No fewer than nineteen contributors have co-operated in the production of the volume.

It is eminently a book for the structural engineer, as distinct from the architect, and is based upon modern American practice, which, while closely resembling English methods in many respects, has many features peculiar to itself and to its own conditions. The book is divided into eight main sections, many of which have subdivisions dealing with special items.

Section i deals with soil investigation, and summarises quite clearly the general methods adopted to test and determine the nature of the soil. A subsection deals with the bearing power of soils as indicated by building codes, but as they are all related to U.S.A. conditions, they are not of much value to English students.

Excavation is the subject of the second section, and it deals in a comprehensive way with all sorts of hand-tools, power shovels, scrapers, derrick and hoist buckets, as well as rock excavation, hand and machine drilling, and the use of explosives, while subaqueous excavation and dredging concludes the section.

Section iii, dealing with foundations, is the longest section in the book. After discussing foundations in general, various writers deal at some length with pit or well foundations, coffer-dams, open and pneumatic caissons, with some illustrations, and a large part of the section is devoted to timber, concrete, and sheet piling, and the equipment required for driving. Very complete and useful information is gathered together in this section, which should be of great value to structural engineers.

Section iv is entitled "Spread Footings," and the reason for, and methods of, spreading loads on the supporting soil are concisely discussed, with examples. A subsection deals with steel and timber grillage foundations and gives an illustrative problem for both methods.

A short section on underpinning follows; while probably quite suitable for comparatively new buildings in American towns, this is quite insufficient for the many dangerous structures to be dealt with in English towns. The illustrations of shoring given are quite inadequate, and of very little use to English practitioners, who probably use the finest shoring in the world.

Section vi discusses foundations requiring special consideration, and includes such items as deep basements and machinery pits, waterproofing of structures, retaining wall, dam, and machinery foundations. The information given is sound and useful, although the illustrations—mostly photographs—do not convey much detail.

Section vii deals with bridge piers and abutments, firstly in the form of general considerations, then in further detail of ordinary bridge piers, abutments, bascule piers and cylinder and pivot piers. Much detailed information is given, together with some clear but rather small-scale illustrations.

Section viii reviews the legal provisions regarding foundations and footings as defined by the modern law of the United States, and although doubtless there is much that is common to English law, many other difficulties will be encountered in this country.

The volume concludes with two appendices, the first dealing with the bearing values of soils for foundations, and the second with formulæ for bearing power of piles, both of which contain much reliable information. A comprehensive index completes an excellent volume, which should prove of great value, not only to students, but also to qualified structural engineers.

A. S.

The Properties of Engineering Materials. By W. C. Popplewell, M.Sc., A.M.Inst.C.E., and H. Carrington, B.Sc.Vict., M.Sc.Tech., A.M.I.Mech.E., A.F.R.Ae.S. [Pp. xii + 546, with 290 illustrations and 34 plates.] (London: Methuen & Co., 1923. Price 28*s.* net.)

This work is intended primarily for engineering students, but departs somewhat from the customary text on this subject in an attempt to provide some knowledge of the strength properties of various materials. The aim of this addition is particularly good, and it should prove of great value to the practical engineer.

The book is divided into two sections, dealing first with the relation of stress to strain in various standard forms of members. It contains most of the usual information found in texts on this subject, the later chapters treating of stresses in compressive members, torsion and helical springs, thick cylinders and rotating discs.

Throughout the section, the method of failure of material and the effect of combined stress have been kept well to the fore. The results of a considerable amount of modern research work have been included, together with numerous references.

In the part dealing with disc stress, the practical man would probably find some difficulty in applying the theory given to the case of a turbine disc, where the hub, disc proper, and rim have to be treated separately, the strains at their junctions being equalised.

It is noted that several graphical methods have been given; these, being general, are of great service in practice.

The more usual English procedure for obtaining critical speeds by equating the strain energy of a shaft in its extreme position to the kinetic energy in its mid-position is not given, although to one not experienced in the form of

deflection curve, this is probably easier than by the continental method shown.

The lay-out of the work is in good teaching order, and the authors are to be congratulated on the way they have first given an "engineering" insight into a problem before passing into the mathematics. Another commendable point is the clear type and way in which the equations are set out.

The second part deals chiefly with the properties of materials and the method of testing. In the first chapter are described various testing machines, extensometers, and autographic records. The general properties and an outline of the manufacture of pig and cast iron, with a description of suitable tests, constitute Chapter XII. Steel is similarly treated in Chapter XIII, and, what is of great value in conjunction with the strength, a concise discussion on the grades of steel suitable for different purposes. Alloy steels are dealt with in the same way.

The later chapters cover the properties and tests for wire, timber, cement, sheet-lead, indiarubber, etc.

A short résumé of the work on repeated loads and a discussion on the choice of materials and factors of safety are also included. Various tests for hardness are explained, although the approximate connection between ultimate strength and hardness of certain materials does not appear to be stated.

The volume is no doubt a valuable addition to the works on the subject.

Makers of Science. By IVOR B. HART. [Pp. 320, with 120 figures.] (London: Humphrey Milford, Oxford University Press. Price 6*s*. net.)

MR. HART is known to the reviewer as the author of a textbook on Heat of rather exceptional merit, and in the present book he maintains the standard which he then set for himself. The book is intended for school use, and more particularly for use in classes where it is desired to include some knowledge of the nature and growth of scientific ideas as part of a broad general education. It is most admirably suited to this purpose, but it will make its strongest appeal to the science student who is interested in the personalities of those whose discoveries it is his pleasure to study. The scope is limited to Astronomy, Mathematics, and Physics, chiefly the latter, and covers the whole development of these subjects in Europe during historic times. The author has selected some fourteen or fifteen of the most prominent workers, and round the lives of each has written a sound and sufficient exposition of the ideas and discoveries associated with their names and periods. It may be safely said that a class which has read the book, with the guidance of a teacher in sympathy with the historical path to scientific knowledge will have acquired a really good understanding of the fundamental principles of astronomy and physics.

After a brief account of Greek science, the author discusses the lives and works of Roger Bacon, Copernicus, Kepler, and Gilbert. The account of Galileo which follows contains the inevitable story of the weights and the tower of Pisa. That this experiment was ever performed before the assembled staff of the university is doubtful, and when it was performed the weights did not fall with that simultaneous thud our author describes so graphically. The next chapter deals with René Descartes and then comes a long and interesting description of the lives and work of Newton and Boyle. The treatment of Newton's work on the spectrum is rather elementary, and the statement that "Newton came to the conclusion that the use of the principle of refraction in optical instruments was undesirable" is rather inadequate. The experiments which Newton performed and the unfortunate circumstance which led to his conclusion are so seldom described that they might well have been inserted here. Huygens's name is misspelt as Huyghens, and there is a small error in the description of one

of Boyle's experiments (p. 184). Robert Hooke appears only as Boyle's assistant and as a person continually crabbing Newton's work—surely an injustice to the author of that extraordinary work the *Micrographia*. The last third of the book deals mainly with electricity from Oersted and Ampère through Davy and Faraday to Kelvin, and is distinguished by an account of the career and discoveries of Georg Simon Ohm, who remains rather a shadowy personage to the average student of electricity.

The book is well illustrated and well written, and though it does not pretend to contain the results of any original research among historical records the reviewer has enjoyed its perusal and can recommend it without reservation to others. D. O. W.

Metals and Metallic Compounds. By ULICK R. EVANS, M.A. [Vol. I, pp. xii + 468, with 1 plate and 91 illustrations, price 21*s*. net. Vol. II, pp. xii + 396, with 19 illustrations, price 18*s*. net. Vol. III, pp. xii + 270, with 1 plate and 42 illustrations, price 14*s*. net. Vol IV, pp. xii + 350, with 1 plate and 32 illustrations, price 18*s*. net.] (London: Edward Arnold & Co., 1923.)

IN these volumes metals and metallic compounds are considered in a broad and interesting manner. The various aspects of our knowledge of metals are brought together in an ordered sequence, and cause and effect are correlated. As the author rightly points out, not only is a general knowledge of physics, chemistry, and geology essential, but it is not possible to obtain a proper understanding of the chemistry of metals without a knowledge of electro-chemistry and colloid chemistry. Metallurgists, chemists, and engineers find it difficult to keep pace with the advances in physical chemistry and physical metallurgy, and at times are almost bewildered by new developments. Progress occurs along unexpected lines, and its real value is not always fully appreciated owing to a lack of suitable assistance to study the basic principles of which a particular discovery is the outcome. Thirty-three years ago Sir William Roberts-Austen, in *An Introduction to the Study of Metallurgy*, pointed out the need of guidance to a knowledge of the principles on which the art of metallurgy is rightly practised, and in his book gave a concise statement of the knowledge which, at that time in metallurgy, depended on the application of chemistry, physics, and mechanics. What was true then applies with even greater force now: the young engineer and student in metallurgy require a sound grounding in chemistry and physics, and need to know the kind of aid the science of metals receives and may be expected to receive from these other sciences.

The work under review treats the subject of metals in a new way and makes a successful attempt to give, within a convenient space, a general and clear account of our present knowledge. It is not a textbook on metallurgy, neither should it be regarded by the engineer or metallurgist as a substitute for textbooks of chemistry and physics. The book, however, is more than a review; it is an introduction and a guide to the study of metals, for it imparts instruction, creates interest, and places the reader in a position to understand and appreciate much of the research work which has been carried out during the past few years, as well as many industrial developments. It consists of four volumes, the first treating the subject generally and the others dealing with the individual metals in the order of the Periodic Table.

Vol. I contains an "Introduction" and two parts—"The Study of the Metallic State (Metallography)" and "The Study of the Ionic State (Electrochemistry)." The Introduction supplies, briefly, that general knowledge of chemistry, physics, and geology which is required throughout the book. The metallographic portion deals with the structure of metals and alloys, and pyrometry and cooling curves, including the effects of deformation, annealing, and alloying on the properties of metals. The chapter on the structure

of alloys is very clearly written. Under the electro-chemical portion are considered ionisation in solution, the colloidal state, electro-deposition, corrosion, and radio-activity; the whole section being treated very ably and concisely.

In Vol. II the various metals of the "A" Groups are taken. These consist of the alkali metals, the metals of the alkaline earths, the various groups which include aluminium, titanium, zirconium, vanadium, tantalium, chromium, molybdenum, tungsten, and uranium, and Group VIIA, in which manganese is the only representative at present. The description of each metal is divided into three parts: the first is an account of the chemistry of the metal and its compounds, with a summary of the methods of analysis—which is merely descriptive and not sufficiently detailed for practical analytical work—the second on the terrestrial occurrence of the metal, and the third on the extraction of the metal from its ores, its uses and the uses of its compounds, which is distinctly technical in character, and although in some cases it is the longest of these sections, it in no way pretends to give an exhaustive or detailed account of industrial processes, but is a good survey of important present-day methods.

The third volume deals with the "Transition Elements"—iron, cobalt, nickel, and the platinum metals. The general properties, magnetism, and crystal-structure of these elements are considered, and then the individual metals are taken as in the previous volume. A hundred pages are devoted to the metallurgy and the structure and properties of iron and steel. Unfortunately a modern calcining kiln is not given, but the old form of Gjers kiln which is so constantly being reproduced in the modern textbooks. The paragraphs on "The Thermal Economy of the Blast-Furnace" give a very good statement of the reactions of reduction. The term "hot-blast stoves" would be more correct than "Cowper stoves." There are several kinds of hot-blast stoves in use, while a Cowper stove is only a particular variety.

The metals of the "B" Groups, viz. copper, silver, gold, zinc, cadmium, mercury, gallium, indium, thallium, germanium, tin, lead, arsenic, antimony, and bismuth, are described in the last volume. A brief account of most of these metals is given, but seventy-four pages are allotted to copper. The diagrams of converters do not represent the most modern types used in the larger copper works. The metallurgy of gold is covered in twelve pages and consists of quite brief and general accounts of the extraction of gold from placer deposits, the stamp battery and cyaniding.

Volumes I, III, and IV contain photo-micrographs—wrongly called micro-photographs—showing the structure of iron, steel, lead, cadmium, and some well-known alloys.

From the above the character and scope of the book can be judged. It tends to be encyclopædic in form, but naturally cannot give the amount of information on any particular subject that may be required by the specialist. Other works must be consulted, and in view of this numerous references are given throughout the book to scientific and technical literature.

The preparation of these volumes and the arranging of the subject-matter must have been a difficult and heavy task, and the author is to be congratulated on producing a useful and interesting book which should prove of value to the student.

E. Courtman.

Technical Analysis of Steel and Steel Works Materials. By Frank T. Sisco. [Pp. xiv + 543, with 28 illustrations.] (London: McGraw-Hill Publishing Co., 1923. Price 25s. net.)

This book embodies the various methods of analysis for steels and steel works materials used by the author in his laboratory, together with notes on various laboratory operations.

The object of the work is: (1) To give the routine analyst, the industrial chemist, and the student in metallurgical chemistry the best, simplest, and most rapid methods for the analysis of steels, special steels, and steel works materials. (2) To emphasise the necessity for speed in analytical control. (3) To provide for the steel worker an account of the steel laboratory and its problems.

The book consists of three parts, namely:

PART I.—The steel works laboratory, its design, equipment, and operations. This part has an interesting chapter on the qualification and selection of laboratory employees in which the author considers that the advantages of a college training in chemistry for the routine steel analyst are doubtful. There is a chapter on errors, one on record-keeping and costs, and two on sampling.

PART II.—The analysis of plain and alloy steels. Attention is directed to the importance of analytical control, especially for electric furnace steels, resulting in the need for rapidity and accuracy. This part includes one chapter on manipulation in steel analysis.

PART III.—The analysis of steel works material. This covers the analysis of pig iron, cast iron, ferro alloys, ores, fluxes, refractories, and slags, but, as the author points out, the sampling and analysis of coal, coke, oil, water, and gas, as well as the determination of oxides and gases in steel, have been omitted.

The methods are above criticism, for they have all been repeatedly used by the author and found to be satisfactory. They are well and completely described, with many useful hints and details of manipulation.

The book as a whole, and particularly the methods for alloy steels, should prove of value to the steel works chemist, the teacher, and the student.

E. COURTMAN.

El Arte de los Metales. Translated from the Spanish of ALVARO BARBA by R. E. DOUGLASS and E. P. MATHEWSON. [Pp. ix + 288, with 1 plate and 12 illustrations.] (New York: John Wiley & Sons; London: Chapman & Hall, 1923. Price 17*s.* 6*d.* net.)

THIS work, the title of which may be rendered broadly as "The Metallurgical Art," was first published in 1640, and was probably the first published treatise on American metallurgy. A number of reprints and translations followed, including an English and several French and German editions, some of which revealed a lack of technical knowledge on the part of the translators. The present volume is a translation, by Messrs. Ross E. Douglass and E. P. Mathewson, of a Spanish edition of 1729.

Criticism of a work of this nature in the light of modern knowledge is a difficult, if not impossible, task. A brief reference to the scope of the work would probably prove of more value to those not already familiar with it.

The book is divided up into five distinct sections, termed "books" by the author. The first "book" gives a general survey of the extent of geological knowledge available in Barba's time. In this section he shows a shrewd knowledge of the Potosi district of Bolivia, with which long residence had made him familiar, and naïvely cites numerous instances in which he was able to turn this knowledge profitably to account. Subsequent "books" deal with the amalgamation of gold and silver ores, the reduction of ores by smelting, and the refining of metals. It is in the sections on the general treatment of silver ores by amalgamation or smelting that his specialised knowledge gained in many years' experience is fully utilised. He treats the subject from an eminently practical standpoint, and many of his statements of fact hold good at the present day.

The work has acquired the status of a classic, and is freely quoted by writers of our own times, notably in reference to the development in the treatment of silver ores. The translators have contented themselves with a literal translation, adding only such notes on the text as serve to remove any ambiguities incidental to translation. The thoroughness of this annotation is a fitting tribute to their familiarity with local Spanish mining terms, ancient and modern. The result is a thoroughly readable book, of interest alike to the student of history for history's sake, and to the metallurgical student desiring an insight into the early history of the metallurgy of silver in a readily accessible form.

E. Courtman.

Mirrors, Prisms, and Lenses: A Textbook of Geometrical Optics. By James P. C. Southall. Enlarged and revised edition. [Pp. xix + 657, with 287 figures.] (New York: Macmillan Company, 1923.)

This attractively written textbook is the fruit of the author's wide experience in teaching Optics at Columbia University and elsewhere, and should appeal to a wide circle of readers on both sides of the Atlantic. Medical students, and others who are apt to be repelled by the large amount of mathematics in many treatises, will find that their needs here receive special consideration. Ophthalmic optics, in fact, takes up a large portion of the book.

It must not, however, be inferred that it is only for such students that the book is written. Though essentially an introduction to the subject, there will be found scattered throughout the volume ample evidence that the author maintains close observation on developments taking place in its various branches all over the world, and in this new edition he has gathered together into an interesting additional chapter notes on quite a number of topics suited to an elementary treatise which have recently been under discussion in various scientific journals. A special feature is the extensive collection of examples, many of them containing results fully as important as those discussed in the text, and considerable additions have been made here also. The importance of this section must excuse our criticism of example 12 on p. 27, where it is implied that the visibility of a black spot on a bright ground depends on the ordinary limit of resolution. The text also is not entirely free from small errors. In dealing with successive reflection at two plane mirrors the angle between the mirrors should be so defined as to be zero when they form one continuous reflector, not when their reflecting faces are in contact. The definition given would lead the student to draw from the general law an incorrect idea of the change of deviation as the angle between the mirrors is altered. On p. 524 we note the statement "it may be demonstrated that no optical system can have more than one pair of aplanatic points." This particular misstatement has been frequently made and is responsible for some misconceptions connected with the higher theory of instruments. Actually three such pairs are possible. The simplest illustration is afforded by a single spherical refracting surface, where one pair are the intersections of a radial line with Young's spheres, another fall on the intersection of the line with the refracting sphere itself, while the third pair coincide with the centre of curvature. The self-conjugate character of two pairs in this simplest illustration is accidental, and examples can easily be constructed in which the members of these other pairs are different points. Such minor blemishes as these, however, can hardly be said to detract seriously from the merits of the book.

Readers who are acquainted with a number of books on geometrical optics must have been strongly impressed with the grave diversity in fundamental conventions followed by different authors. Those adopted in this book have perhaps been employed as widely as any others, and the treatment

appears to be consistent; but to the reviewer it seems unfortunate to ascribe both a positive and a negative focal length to one and the same lens, and the use of the terms "primary" and "secondary" in this connection is open to the objection that in more advanced theory they bear quite different meanings.

The book is adequately illustrated—some of the original figures are particularly well conceived—and is provided with a full index.

T. Smith.

Popular Fallacies Explained and Corrected. By A. S. E. Ackermann, B.Sc. Third Edition. With an Introduction by Sir Richard Gregory, D.Sc., F.R.A.S. [Pp. xvi + 985.] (London: The Old Westminster Press, 1923. Price 12*s.* 6*d.* net.)

In his introduction to this useful work Sir Richard Gregory says: "Charles Lamb remarked that a man could not be altogether happy in the company of his schoolmaster. The reader of this book may similarly become depressingly conscious of his numerous mistakes; or, on the other hand, he may, by unseemly intrusion or insistent correction, develop into a captious critic whose presence in friendly conversational circles is best avoided. Mr. Ackermann does not desire to produce either of these types of mind, but rather to provide a work of reference which all who prefer precision to uncertainty or inaccuracy may consult with advantage." The author has succeeded very well indeed in this design; and the third edition deals with 1,350 fallacies, while the previous 1909 edition taught us wisdom regarding only 460 fallacies. Is this because the human race acquires more and more fallacies the longer it continues to exist—as some think? We quite agree that the book is not a mere list of popular errors, but is also a very interesting work of reference upon a number of curious matters. The author has collected many useful but often forgotten notes upon each point. Perhaps some very learned persons (among whom we do not include ourselves) may occasionally feel inclined to correct the corrections. For instance, in the article on Consumption (page 149) a pundit says very decisively: "People talked of consumption being brought on by a chill. To talk of any disease being due to a chill was stuff and nonsense." We seem to remember certain experiments of Pasteur on fowls which would hardly support this view; and also recollect numerous occasions when a certain common malady of the nose followed exposure to thorough chilling in railway trains and elsewhere. Medical teaching seems to have got rather beyond this stage of deriding chills. Many infections, including those of "colds in the head," appear to lie dormant for a long time, until they are brought out suddenly by something which greatly depresses the defensive armament of the body. The story has been told that the Japanese Ambassador was once present at a dinner where Mr. Gladstone was holding forth as usual on every conceivable subject. "What a clever man is your Mr. Gladstone," exclaimed the Japanese Ambassador as the great politician dogmatised first on one theme and then on another. At last the conversation turned to Japan—upon which Mr. Gladstone again enlightened his audience. "Ah," said the Ambassador, "but he knows nothing about Japan." We do not think that Mr. Ackermann will be so lightly convicted.

BOOKS RECEIVED

(*Publishers are requested to notify prices*)

Cours de Mathématiques Générales à l'Usage des Étudiants en Sciences Naturelles. Par Gustave Verriest. Première Partie. Calcul Différentiel. Géométrie Analytique à deux Dimensions. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1923. (Pp. 337.) Price 38 frs.

Four-figure Mathematical Tables. By Frank Castle, M.I.Mech.E. London: Macmillan & Co., St. Martin's Street, 1923. (Pp. 48.) Price 1*s.* net.

Cours Complet de Mathématiques Spéciales. Par J. Haag. Tome IV. Géométrie Descriptive et Trigonométrie. Pairs: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1923. (Pp. vi + 144.) Price 13 frs. net.

Exercises du Cours de Mathématiques Spéciales. Par J. Haag. Tome IV. Géométrie Descriptive et Trigonométrie. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1924. (Pp. xi + 152.) Price 13 frs.

Biomathematics. Being the Principles of Mathematics for Students of Biological Science. By W. M. Feldman, M.D., B.S., F.R.S., Physician, Eastern Dispensary. With Introduction by Sir William M. Bayliss, M.A., D.Sc., LL.D., F.R.S. London: Charles Griffin & Co., Exeter Street, Strand, W.C.2, 1923. (Pp. xix + 398.) Price 21*s.* net.

Algebras and their Arithmetics. By Leonard Eugene Dickson, Professor of Mathematics, University of Chicago. Chicago, Illinois: the University of Chicago Press. (Pp. xii + 241.) Price $2.25.

Elements of the Theory of Infinite Processes. By Lloyd L. Smail, Ph.D. New York and London: McGraw-Hill Book Company, 1923. (Pp. vii, + 339.) Price 17*s.* 6*d.*

Principles of Geometry. By H. F. Baker, Sc.D., LL.D., F.R.S. Volume III —Solid Geometry: Quadrics, Cubic Curves in Space, Cubic Surface. Cambridge: at the University Press, 1923. (Pp. xviii + 228.) Price 15*s.* net.

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Elementary Aeronautical Science. By Ivor B. Hart, B.Sc., A.F.R.Ae.S., and W. Laidler, B.Sc. Oxford: at the Clarendon Press, 1923. (Pp. vi + 288, with 213 figures.) Price 7*s*. 6*d*. net.

The Boy's Own Book of Science. By Floyd L. Darrow. New York: The Macmillan Company, 1923. (Pp. vii + 331.) Price 10*s*. 6d. net.

ERRATA

In the July number of this volume the words "5% free fatty acid," which appear on page 146, line 14, and also in the heading of the table, should read, "0·5% free fatty acid."

The title of Lt.-Commander R. T. Gould's book, reviewed on page 309 of this volume, should read "The Marine Chronometer" instead of "The Maiine Chronograph."

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SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

PURE MATHEMATICS. By F. PURYER WHITE, M.A., St. John's College, Cambridge.

History.—G. A. Gibson (*Proc. Edin. Math. Soc.*, **41**, 1923, 2–25) gives an account of the mathematical work of James Gregory, whose name is remembered in connection with the series for arc tan x. The material is chiefly taken from his letters in Rigaud's collection and in the *Commercium Epistolicum* of 1712.

G. A. Miller (*Science*, **59**, 1924, 1–7) gives a survey of American mathematics during the past seventy-five years—a presidential address to the Mathematical Section of the American Association for the Advancement of Science. The same author has also several notes dealing with Greek mathematics (*Science*, **58**, 1923, 288–90; *School Science and Mathematics*, **23**, 1923, 320–2; *School and Society*, **18**, 1923, 621–2).

Algebra.—In the general theory of linear associative algebras, defined in any given field of rationality, an important part is played by the algebras which have no divisors of zero, the *division* algebras of Dickson. In the field of all complex numbers and in the field of real numbers the problem of the determination of division algebras has been completely solved by a theorem due to Frobenius, but in any other field of rationality no non-commutative division algebras are known except a type discovered by Dickson and further investigated by Wedderburn. This type is obtained as follows: Let $f(x) = 0$ be a uniserial Abelian equation of degree n in the field F, and let its roots be a, $a_1 = \theta(a)$, $a_2 = \theta^2(a) = \theta[\theta(a)]$, . . . $a_{n-1} = \theta^{(n-1)}(a)$, $[\theta^n(a) = a]$, where θ is a polynomial of degree $n-1$ at most with coefficients in F. Take a unit i for which the characteristic equation is $f(i) = 0$, and another unit j for which $ji = \theta(i)j$ and $j^n = g$, a number of F, this being the characteristic equation for j. Then the fundamental units of Dickson's algebra, which is of order n^2, are $i^s j^t$ $(s, t = 0, 1 \ldots n-1)$. If there is no number of F whose

norm is a power of g less than the nth, then this algebra will be a division algebra. Recently Wedderburn (*Trans. Amer. Math. Soc.*, **22**, 1921, 129–35) has shown that for $n = 2$ or 3 the algebras of this type are the only non-commutative division algebras of their respective orders, and has raised the question whether there exist division algebras of a similar type corresponding to any irreducible Abelian equation, not uniserial; there would clearly have to be not a single unit j but as many as there are roots of the equation.

If $a_0 = a$, $a_1 = \theta_1(a)$, . . . $a_{n-1} = \theta_{n-1}(a)$ are the roots of the Abelian equation, then corresponding to each there is an element j_r of the algebra for which $j_r\, i = \theta_r(i) j_r$ ($r = 0, 1, \ldots n-1$); of these j_0 is the modulus of the algebra. We can now take as the fundamental units of the algebra j_0, ij_0, . . . $i^{n-1}j_0$, j_1, ij_1, . . . $i^{n-1}j_1$, . . . j_{n-1}, ij_{n-1} . . . $i^{n-1}j_{n-1}$; these can be shown to be linearly independent. The general element of the algebra is then $\sum_{0}^{n-1} g_r(i) j_r$, where the $g_r(i)$ are arbitrary polynomials, of degree $n-1$ at most, with coefficients in F. Wedderburn left undecided the question whether such algebras actually exist, but stated that he thought it probable. F. Cecioni (*Rend. Palermo*, **47**, 1923, 209–54) has recently settled the matter by obtaining examples of such algebras of the 16th order. He first investigates algebras of order n^2 of the above type which are not necessarily division algebras; and then, confining himself to the case $n = 4$, adds the condition that there shall be no divisors of zero. He has not been able to arrive at any result with regard to whether these new algebras are equivalent or not to those obtained by Dickson; but he remarks that it is possible to obtain equivalent algebras beginning with different equations $f(x) = 0$.

Major P. A. MacMahon (*Proc. Camb. Phil. Soc.*, **21**, 1923, 651–4) discusses a problem given in books of mathematical puzzles. If a measuring rod divided into n equal segments is to be used for measuring any number of segments with one operation, certain of the scale divisions will be unnecessary. Thus a rod a yard long divided into three feet may have one division wiped out without interference with the measurement of one, two, or three feet. The problem is simpler for a rod of infinite length, and the author gives the first 42 numbers of the segmental series required, which he calls the prime numbers of measurement. He has calculated the series as far as 1,000, 347 numbers arising; but as yet he gives no analytical method of dealing with the series. In another paper (*ibid.*, 642–50) the same writer discusses the partitions of infinity.

Papers by G. Andreoli (*Rend. Napoli*, **28**, 1922, 80–5) and H. Schmidt (*Leipzig Ber.*, **75**, 1923, 25–30) deal with cer-

tain classes of determinants which can be expressed as the product of rational factors.

H. W. Turnbull (*Proc. Edin. Math. Soc.*, **41**, 1923, 116–27) investigates for double binary forms Gordan's theorem that the complete system of independent concomitants of a given form is finite.

Finite Groups.—G. A. Miller (*Bull. Amer. Math. Soc.*, **29**, 1923, 394–8) gives a new abstract proof and a generalisation of the theorem that if H be any subgroup of a group G of finite order it is possible to select a set of distinct operators $s_2, s_3, \ldots s_\lambda$, so that every operator of G appears once and only once in each of the following two sets of augmented co-sets of G:

$$\mathrm{H} + \mathrm{H}s_2 + \mathrm{H}s_3 + \ldots + \mathrm{H}s_\lambda,$$
$$\mathrm{H} + s_2\mathrm{H} + s_3\mathrm{H} + \ldots + s_\lambda\mathrm{H}.$$

Analysis.—J. C. Burkill (*Proc. Camb. Phil. Soc.*, **21**, 1923, 659–63) gives a simple proof of the fundamental theorem of Denjoy integration, that the Denjoy integral has almost everywhere the integrand as derivative.

T. M. Cherry (*Proc. Camb. Phil. Soc.*, **21**, 1923, 711–29) obtains the general solution of a system of difference equations of the form $\phi_r(s+1) = f\{\phi_1(s), \phi_2(s), \ldots \phi_n(s)\}$ $(r = 1, 2, \ldots n)$, in the form $\phi_r(s) = \psi_r\,(s + c_1, c_2, \ldots c_n)$, in which the ψ are analytic functions of their n arguments, while the c are arbitrary periodic functions with period 1.

G. Belardinelli (*Rend. Palermo*, **47**, 1923, 193–208) investigates series of the forms $\sum\limits_0^\infty c_n\mathrm{P}_n(x)$ and $\sum\limits_0^\infty c_n/\mathrm{P}_n(x)$, where $\mathrm{P}_n(x) = (x - a_0)\ (x - a_1) \ldots (x - a_{n-1})$, with $\mathrm{P}_0(x) = 1$. Frobenius and Bendixson and more recently Landau have studied the cases in which the numbers a are all real and either have a unique limit point or tend to infinity; for complex values of $a_n = b_n + ic_n$ the second series has been studied by Schnee when c_n/b_n tends to zero, and by Pincherle when the integral function which has the a for zeros is of class one. Belardinelli makes the following hypotheses on the numbers $a_n = r_n\,e^{i\phi_n}$:—(1) r_n tends to infinity with n, (2) for every n, $-\frac{1}{2}\pi < -\phi < \phi_n < \phi < \frac{1}{2}\pi$, (3) the series $\sum\limits_0^\infty 1/r_n$ is divergent, and (4) the numbers a are the roots of an integral function of finite class p, greater than or equal to one. Particular cases are the factorial series $\sum\limits_0^\infty c_n n!/x(x+1) \ldots (x+n)$ and its analogue $\sum\limits_0^\infty c_n\binom{x}{n}$.

Frobenius showed that zero could be expanded as a series

of functions of the form $(x-a_0)\ (x-a_1)\ .\ .\ .\ (x-a_{n-1})$, if $\sum\limits_0^\infty(a_n)$ is convergent, but he did not give the construction of the expansion; Pincherle gave it for the series $\sum\limits_0^\infty c_n\left(\frac{x}{n}\right)$; Berlardinelli gives it for his more general case.

Two recent papers deal with Mathieu functions; J. Dougall (*Proc. Edin. Math. Soc.*, **41**, 1923, 26–48) obtains asymptotic expansions of two independent solutions of Mathieu's equation

$$\frac{d^2u}{dz^2}\ (\tfrac{1}{2}k^2c^2\ \cos\ h\ 2z - s^2)\ u = 0.$$

when the real part of z tends to positive or negative infinity; his method is the direct transformation of the series already known. E. L. Ince (*ibid.*, 82–115) studies what he calls associated Mathieu functions, which satisfy the equation

$$\frac{d^2u}{d\ z^2} + \left[a + 2\theta\cos 2z - \frac{\nu(\nu-1)}{\sin^2 z}\right] u = 0.$$

This equation turned up in the work of Abraham in 1899. In another connection Ince had been led to the integral equation

$$u(z) = \lambda\int_0^{2\pi} e^{k\cos z\cos s}\sin^\nu z\sin^\nu s\ u(s)\ ds,\ (k^2 = 4\theta),$$

and it appeared that solutions of this were solutions of the differential equation. P. Humbert, however, recently stated that this was only true when ν is an integer. Ince now proves that it holds for all real values of ν greater than $-\frac{1}{2}$, and also that the limits may be taken as 0 and π, thus avoiding the difficulty of having a branch point of $\sin^\nu s$ within the range of integration.

Geometry.—D. M. Y. Somerville (*Proc. Edin. Math. Soc.*, **41**, 1923, 49–57), whose researches on the partitioning of space were noticed in the last number, gives a more detailed description of the four space-filling tetrahedra in Euclidean space. The first may be obtained from two equal cubes with a common face by taking as vertices the centres B, C of the cubes and two corners A, D lying on a common edge; it has the two edges BC, AD equal to 2 and the other four equal to $\sqrt{3}$. The solid angles are all equal to $\omega/24$, where ω is the total solid angle at a point. Twelve such tetrahedra can be placed together to form a rhombic dodecahedron. The second type is obtained from the first by dividing it into two by a plane of symmetry through the edge AD; if E is the new vertex then AB = BD = $\sqrt{3}$, AE = DE = $\sqrt{2}$, AD = 2, BE = 1, and the solid angles at A,B,D,E are respectively $\omega/48$, $\omega/24$, $\omega/48$, $\omega/8$. (Six tetrahedra of this type can be placed to-

gether to give a cube.) From two such tetrahedra placed together with the face BDE as a common face we get a tetrahedron ABDF of the third type; for this AB = BD = BF = $\sqrt{3}$, AD = DF = 2, AF = $2\sqrt{2}$. The fourth type is obtained from the original one by joining the centre S of its circumscribed sphere to the vertices; for this AS = BS = CS = $\frac{1}{2}\sqrt{5}$, AB = AC = $\sqrt{3}$, BC = 2. For a proof that these are the only types the author refers to his earlier paper.

The generalisation to three dimensions of the pedal or Wallace-line property of the circumcircle of a triangle was made by Steiner, who showed that the locus of a point the feet of the perpendiculars from which on to the faces of a tetrahedron lie in a plane is a four-nodal cubic surface containing the edges of the tetrahedron. J. P. Gabbatt (*Proc. Camb. Phil. Soc.*, **21**, 1923, 763–71) examines the extension of this to non-Euclidean space of any number of dimensions. The same author (*Proc. Edin. Math. Soc.*, **41**, 1923, 108–15) also generalises the Tarry point of a triangle to the non-Euclidean plane.

H. Kapferer (*Jahresber. D. Math. Verein*, **32**, 1923, 32–42) gives an account of an algorithm for determining the multiplicity of the intersections of two algebraic curves. The validity of Bezout's theorem that two curves of orders m, n have exactly mn intersections clearly depends upon the concept of the multiplicity of the intersections; to define this we may use Liouville's substitution $z = ux + vy$, with undetermined u and v, to replace x. Form the y-eliminant R(z) of the two resulting equations, its roots will be of the form $u\xi + v\eta$. If then R(z) contains a factor $(z-u\xi - v\eta)$ to the power q, then we say that (ξ, η) is a q-ple intersection of the curves. All methods previously given for actually calculating the number q have this disadvantage that the successive processes have to be carried out separately for each point of intersection. Kapferer's method, which he likens to Hudde's rule for determining the multiple roots of an algebraic equation, makes the same calculations serve for all the intersections at which one at least of the curves has not a multiple point. If the two curves are $f(x,y) = 0$ and $g(x,y) = 0$, then for points at which g is not multiple we form the Jacobian of f and g, the Jacobian of this again with g, and so on; if the first not vanishing at a point of intersection is the rth, then the multiplicity of the intersection there is r. The author also gives rules for the excepted case and illustrates the method by examples.

M. del Re (*Rend. Napoli*, **29**, 1923, 79–88) considers symmetrical linear systems of plane curves with 7 base points, *i.e.* systems having the same multiplicities at all 7, and obtains

surfaces in spaces of various dimensions. They contain a configuration of 56 rational normal curves which can be used to obtain the 27 lines of a cubic surface and the 28 double tangents of a plane quartic curve. In a later paper (*ibid.*, 105–11) the author investigates a surface of order 8 in space of six dimensions, and by projecting from three points obtains a quartic surface in ordinary space with three double lines meeting in a triple point. For particular positions of the plane of projection Noether's quartic surface with a double tac-node may be obtained.

In 1893 Stackel gave an analytical proof of the theorem that if an algebraic curve in space has one and therefore all its evolutes algebraic, then its polar developable, *i.e.* the envelope of its normal planes, has all its geodesics (which include the evolutes) algebraic. A. R. Lanza (*Rend. Palermo*, **47**, 1923, 271–3) gives a proof of a more synthetic character and also proves the converse, *viz.* that if a developable has all its geodesics algebraic it is the polar developable of an algebraic curve with algebraic evolutes.

H. Mohrmann (*Rend. Palermo*, **47**, 1923, 153–86) extends to all algebraic curves with a continuous automorphic collineation group (W-curves) certain theorems discovered in 1908 by Marletta for special classes of curves. In a previous paper (*Math. Ann.*, **89**, 1923, 260–71) he had shown that every algebraic curve with not more than two singular branches is a W-curve, and that the co-ordinates of its points can be expressed as integral powers of a parameter:

$$x_k = \lambda^{n_k}(k = 0, 1 \ . \ . \ . \ r-1), \ x_r = 1.$$

The group of automorphic collineations of the r-dimensional space is then obtained from the substitution $\lambda^1 = A\lambda$ and has the form $x_k^1 = A^{n_k}x_k$, $x_r^1 = x_r$. Besides the identity ($A = 1$) there is only one involutory collineation, $A = -1$; to any point corresponds its harmonic conjugate with respect to the two origins of the singular branches, $\lambda = 0$ and $\lambda = \infty$ ($n > r$). Forming the equations of the curve in hyperplane co-ordinates, it is at once clear that there are at least ∞^1 polarities with regard to quadric varieties which transform the points of the curve into its osculating hyperplanes; the simplex determined by the two singular branches is a common polar simplex of the quadrics. Mohrmann then deals with the curves which have the further property that $n_{r-k} = n - n_k$; they have a further set of involutory collineations given by $\lambda^1 = B/\lambda$. For r even the order of such a curve must be even and apart from the rational normal curves there are such in space of any even number of dimensions greater than or equal to 4. These curves have, besides the polarities spoken of above, a fur-

ther set of reciprocities; two of these are involutory and are polarities with respect to quadrics with the simplex before referred to as a tangential simplex. If r is odd, there are two cases to be distinguished; in both there is a set of reciprocities, two of which are involutory, but for n even they are both null polarities, one transforming a point of the curve into the osculating hyperplane there, the other transforming it into the osculating hyperplane at the harmonic point; for n odd the second is a polarity with respect to a quadric. All these special W-curves have a single infinity of principal chords, *i.e.* chords which lie in the osculating hyperplanes of both their points. These chords effect an involutory $[n-r]$ correspondence between the points of the curve; for n even and r odd this splits up, however, into two rationally separated parts, a $[n - r - 1]$ and a $[1]$, the latter transforming a point into its harmonic conjugate. The ruled surface of the principal chords in this latter case consists of two parts of orders $(n - r - 1)$ $(n - n_{r-1})$ and $n - n_u$, where n_u is the smallest odd exponent in the parameter representation of the curve; in all other cases it is an irreducible surface of order $(n - r)$ $(n - n_{r-1})$. The author remarks that the sum of the orders of the two parts will not always be equal to $(n - r)$ $(n - n_{r-1})$, and draws the moral that the principle of the conservation of number is to be used with caution. A further result is that all W-curves of this special type lie on $\frac{1}{2}(r - 1)$ or $\frac{1}{2}r$ linearly independent quadrics. The remainder of the paper deals in more detail with curves in space of three dimensions; the special W-curves are asymptotic curves on the surface formed by the axes of the second set of collineations. The axes belong to a linear congruence, and thus in the representation of lines by the points of a quadric variety in 5-dimensional space the surface of the axes corresponds to a curve on an ordinary quadric with a non-vanishing discriminant. This curve again is a special W-curve and its axes form a surface represented by the original curve. Some of the results of the paper hold also for transcendental W-curves which can be expressed parametrically in a similar form, with the n_k no longer rational.

C. G. F. James (*Proc. Camb. Phil. Soc.*, **21**, 1923, 610–24) discusses a few cases of complexes of space cubic curves; and L. Godeaux (*ibid.*, 576) contributes a bibliographical note to James's earlier paper on the analytical representation of congruences of conics.

It is well known that in four-dimensional space the planes which meet four lines also meet a fifth; C. G. F. James (*Proc. Camb. Phil. Soc.*, **21**, 1923, 664–84) proves a theorem which may be regarded as an extension of this, namely, that those trisecant planes of a rational normal quartic curve which meet

a line all meet a second such curve. This theorem had apparently already been noticed incidentally by Segre; James shows that the natural place of the theorem is in space of seven dimensions.

It is perhaps worth pointing out in this connection how the quality of the mathematical papers communicated to the Cambridge Philosophical Society has improved, after a temporary slump, during the past year or so. The Society is rapidly regaining its position, if it had ever lost it, as one of the leading scientific societies in the world, and its publications must be consulted by all who wish to keep abreast with mathematical research.

In 1899 G. Humbert showed that certain singular Abelian functions can be taken for the co-ordinates of the points of a hyperelliptic quartic surface with 15 nodes. Later C. E. Traynard (*Annales École Norm. Sup.*, **24**, 1907, 77–177) examined a surface of this type by means of non-singular Abelian functions of divisor 3 and determined its curves; and L. Remy (*Bull. Soc. Math.*, **35**, 1907, 53–69) classified the different types of surface for divisors of the form $2p^2 + 1$. Two recent papers have appeared on this subject: C. E. Traynard (*Comptes rendus*, **176**, 1923, 560–2) examines the relations between the cases of singular and non-singular Abelian functions, and M. Piazzolla-Beloch (*Rend. Palermo*, **47**, 1923, 182–92) investigates systematically the curves on the surface. All algebraic curves on the surface are of even order; there are 10 conics, the curves of contact of 10 tropes, and 4 pass through each node. There are 15 pencils of twisted quartics of genus 1, each passing once through 8 nodes and cutting the singular curve (of order $4r$) in $4r$ points. For $r > 1$ there are no others, but for $r = 1$ there are 6 other pencils, each passing through 5 nodes and cutting the singular curve in 7 points, and 6 pencils through 5 nodes and cutting the singular curve each in 1 point.

G. Kowalewski (*Leipzig Berichte*, **75**, 1923, 15–24, 81–5, 86–90) has a series of papers dealing with plane transformation groups. If G be such a group, then we can in an infinite number of ways make correspond to any singly infinite system of curves another such system whose relations to the first system are invariant under the group G. Such a system is called a ***covariant trajectory system***. For the group of displacements they are the systems of isogonal trajectories, but in this case there is a peculiar feature in that the relation to the original system persists under a larger group. The new theory depends upon the existence of certain mixed differential-invariants which can be obtained by quadratures from the infinitesimal transformations of the group G. The other two

papers deal with the derivation of the curve element and the lowest differential invariant of a plane transformation group from the infinitesimal transformations and from the defining equations of the group without any integration.

J. A. Schouten (*Jahresber. D. Math. Verein.*, **32**, 1923, 91–6) gives an account of the development of the "Ricci-Kalkül," which was first invented in 1887 by Ricci following on the work of Beltrami on differential parameters, but which was neglected until the Relativity boom turned men's attention to the differential geometry of non-Euclidean space of any number of dimensions.

PHYSICS. By J. Rice, M.A., University, Liverpool.

In the notes which the writer contributed to this section of the last number of Science Progress attention was directed to the riddle which confronts Theoretical Physics, viz. how to unite the two seemingly contradictory hypotheses of an undulatory and a corpuscular nature for radiation, each one of which is nowadays employed to "explain" a certain group of the experimental evidence. Every day the position becomes more perplexing. Thus, in the *Physical Review* of May 1923, Prof. A. H. Compton of Washington University (St. Louis), and in the September number his colleague, Prof. G. E. M. Jauncey, bring forward a quantum hypothesis in a region where for long it was believed that the theory of the continuous, coherent wave provided a quite adequate foundation for mathematical development, viz. in the scattering of X-rays by light elements. Twenty years ago J. J. Thomson provided a theory for this phenomenon on classical lines and the experiments of Prof. Barkla fully supported it ; in fact, this combination of theory and experiment was the first to yield data from which could be drawn some justifiable conclusions as regards the number of electrons in a definite atom. But more recent experiments have shown that the predictions of the Thomson theory are only correct when X-rays of moderate hardness are employed. When very hard X-rays or γ-rays are used, the scattered energy is found to be decidedly less than Thomson's theoretical value, and to be concentrated strongly on the emergence side of the scattering plate. Still more recently another serious difficulty with the older theory has arisen. Thomson's reasoning, based on the view that scattering electrons radiate because of the vibrations forced on them by the primary radiation, necessarily leads to the conclusion that the scattered radiation has exactly the same wave-length as the primary. Now it has long been known that secondary γ-rays are softer than the primary rays which excite them, and Compton has

shown within the past two years that X-rays scattered from graphite (the primary rays being the K-radiation of molybdenum, and the secondary being almost undoubtedly a true scattered radiation unmixed with fluorescent, characteristic rays) have, apart from a small fraction (whose existence is even doubtful), a wave-length greater than that of the primary. To explain the actual facts Compton puts forward the hypothesis that the X-ray radiation consists of actual energy-quanta, which have such dimensions that each quantum is able to "collide" as it were and expend part of its energy and momentum upon some particular electron. This electron in turn scatters the ray in some definite direction. There is a change in the momentum of the X-ray quantum due to the change in its direction, this resulting in a recoil of the scattering electron. The energy is thus less in the scattered quantum than in the incident by the kinetic energy of the electron's recoil. The equations of energy and momentum provide two equations which are sufficient to give the increase in wave-length in terms of the angle between the incident and scattered ray. The further problem of working out the distribution of the intensity of the scattered radiation raises certain points which are treated somewhat differently by Prof. Jauncey in his paper. But either Compton's or Jauncey's conclusions are much more consistent with the facts than those of the older theory, even when that theory is extended by assuming that there may be interference between rays scattered by different parts of the electron in the case where the diameter of the electron is comparable with the wave-length of the radiation.

Now such a theory is frankly corpuscular and adds one more example to the growing list of phenomena where corpuscular ideas seem to be ousting the old, familiar notion of the continuous, coherent wave. And yet we cannot merely ignore all the phenomena of diffraction and interference. Some reconciliation must be effected, and the undulatory views must be given their due share of importance. In that direction there have appeared, within the past year or so, some very interesting speculations by M. Louis de Broglie. They will be found in the *Journal de Physique* (Nov. 1922), and in the *Comptes Rendus* of 1923. In the February number of the *Phil. Mag.* he has published a paper on a "Tentative Theory of Light Quanta." The following is a concise statement of his views.

We have to consider two aspects of all phenomena, viz. propagation of energy or mass (the two are identical on relativistic lines) and the existence of a vibratory feature in any occurrence. Every mass is an amount of energy, and is thus connected by Planck's $h\nu$ law with a vibrational frequency.

Now, if we direct our thoughts back to familiar things, we find a very striking fact in the propagation of a train of waves, viz. the existence of two wave-velocities, the velocity of the "phase-wave" and the "group-velocity." No actual train of waves in the treatment of actual oscillatory phenomena, such as sound-waves or water-waves, has the ideal unifrequentic character often assumed in theoretical treatment. Any train of waves is limited in extent, yet it can be shown that they are "equivalent" to or are the "resultant" of a number of ideal unifrequentic waves of unlimited extent and with periods lying within a narrow range of frequencies. This is a matter of the familiar Fourier analysis. Now if, as is usual, the velocities of such ideal "phase-waves" depend on their wave-length (*i.e.* if the medium is dispersive), the actual velocity of the train or "group" of waves is quite different from that of the Fourier constituents. Anyone observing a train of water-waves can often appreciate this fact in a striking manner; the middle of the train where the amplitude is greatest seems to go slower than the actual separate waves; fixing one's attention on a particular crest, it seems to advance through the train dying out as it does so. Now, clearly, if we concentrate our attention on the transport of energy, we must consider the velocity of its transport as the group-velocity; from that point of view the phase-waves or Fourier constituents are entirely a mathematical fiction. Yet when we come to optical phenomena, in which the idea of group-velocity has played a very prominent part, as every reader of the familiar text-books will know, it is on the "existence" of these ideal, monochromatic phase-waves that we rely for our explanation of interference, although we are perfectly sure that light is actually emitted in limited "pulses" or wave-trains. (The writer has stressed this point and put it first, thus distorting the order of M. de Broglie's exposition; for he thinks that this is the essence of the idea apart from the special relativistic and quantum ideas which provide the material for de Broglie's more detailed and mathematical treatment.)

Supposing now we introduce the idea of "light-quanta," as so much experiment seems to be quite conclusive in favour of their actual reality. Choosing the simplest assumption we admit that all light-quanta are identical; they differ slightly in velocity. Let their "rest mass" be m_0, so that on relativistic views the energy of a light-quantum whose velocity is v, is am_0c^2, where $a = (1 - v^2/c^2)^{-\frac{1}{2}}$. The various light-quanta have slightly different velocities, also slightly differing from the limiting Einstein velocity, c, by amounts which cannot be detected by experiment. Now each light-quantum is the seat of some internal vibratory motion whose frequency is

determined by the energy m_0c^2 from which arises the inertia of the rest-mass, m_0. This determines a frequency ν_0, given by the equation—

$$h\nu_0 = m_0c^2 \qquad (1)$$

(h is Planck's constant.)

But the energy of the light-quantum in motion is associated with a frequency ν given by—

$$h\nu = am_0c^2 \qquad (2)$$

or

$$\nu = a\nu_0 \qquad (3)$$

As v is so near c that a is large, it is consistent with the facts to assume that m_0 is at most of the order 10^{-50} gram.

Now relativity ideas suggest another frequency. If the observer were actually looking at the internal vibration in a moving body, he would observe for it, *after allowing for the Döppler effect on classical lines*, not ν_0 but ν_1 where $\nu_1 = \nu_0/a$, so that

$$\nu = a^2\nu_1 \qquad (4)$$

(This is the well-known relativity effect of an occurrence appearing to go slower on a body moving relative to the observer, than on one fixed relative to him.)

We now have two frequencies ν and ν_1, derived by relativistic considerations from a fundamental vibration ν_0, one connected with the actual total energy transported, the other connected with the actual appearance of the internal vibration to the observer in whose frame of reference the moving body has the velocity v.

Here M. de Broglie introduces the new concept of a "fictitious wave" (*onde fictive*), which plays a fundamental part in his speculations, and proves an important theorem for it. Supposing, he says, that, at a given instant, the moving body is situated at a point in a wave of frequency ν which is advancing in the same direction as the body but with a velocity c^2/v, *i.e.* c/β (where $\beta = v/c$), and at this point it happens that at this instant the internal phenomenon of the moving body is in phase with the wave, then this harmony of phase will always persist. True, the body is as it were always falling behind relative to the wave, since $\beta < 1$, but it will always find itself in a part of the wave "harmonising" with its own phase *as viewed by the "fixed" observer*, if it is in such a harmony at the initial instant. Thus at time t the body is at a distance $x = vt$ from the origin, and the phase of its internal motion *as viewed by the fixed observer* is given by $\sin 2\pi\nu_1 t$, *i.e.* $\sin 2\pi\nu_1 x/v$.

If the wave has the same phase at the initial instant, it has at x the phase $\sin 2\pi\nu\left(t-\frac{\beta x}{c}\right)$, since its velocity is assumed to be c/β. The phases are the same if

$$\frac{\nu_1}{v}=\nu\left(\frac{1}{v}-\frac{\beta}{c}\right)$$

$$i.e.\ \nu_1=\nu\,(1-\beta^2)=\nu/a^2$$

which, by (4), is true.

Thus we can associate with the transport of energy in a corpuscular form a wave whose connection is apparently a pure mathematical fiction, but really no more a fiction than the Fourier constituents of an actual vibratory motion. Indeed, the analogy goes deeper; for it is a well-known theorem of group velocity analysis that

$$\frac{1}{v}=\frac{d(\nu/u)}{d\nu},$$

where u is phase velocity (a function of ν) and v is the group velocity of a train of waves made up of Fourier constituents whose frequencies do not differ much from ν; and actually if we put ν equal to $a\,m_0c^2/h$ and $u=c/\beta$, we find that $v=\beta c$. Hence the result: the velocity of a moving body is the energy velocity of a group of waves having frequencies $\nu=m_0c^2/h\sqrt{(1-\beta^2)}$ and velocities c/β corresponding to slightly different values of β.

All this refers to rectilinear uniform motion. When we take up the case of variable velocity matters become more vague, and we can only grope forward to a new theory, which will probably gather Dynamics, Optics, and Electro-magnetism under one set of principles. Thus, if a body describes a curved path we say that there is a field of force and the potential energy at each point enables us to calculate a body's velocity as it goes through this point from its whole energy. It therefore seems natural to suppose that the phase-wave must have at any point a velocity and a frequency fixed by the value which β would have if the body were there. If we know its velocity of propagation at each point we can determine the manner in which it is propagated, and hence the paths of its *rays*. A new electro-magnetism will be necessary to give us the laws of this new kind of propagation, but it is probable that its final result will be: "the rays of the phase-wave are identical with the paths of the body which are dynamically possible." M. de Broglie indicates how Fermat's optical principle of Least Time and the principle of Least Action for a single particle may be

exhibited in a similar mathematical form in terms of the previous ideas. He also points out a very interesting feature of the orbits of an electron as determined in atomic theory by Bohr's method. It is this: suppose the electron is at a point of its trajectory and is in phase (in the manner explained above) with a phase-wave at this point; if the wave goes round the orbit and once more overtakes the electron it is still in phase with the electron if the orbit is a quantum one, but not otherwise.

We seem here to be approaching a method of fitting together the quantum ideas and the wave ideas. Consider the body to be one of the free light-quanta referred to above. The atom of light whose *whole* energy is $h\nu$ is the seat of an internal periodic phenomenon which, for a fixed observer, has at each point the same phase as a wave spreading in the same direction with a velocity very little greater than c. When a phase-wave crosses an excited atom this atom has a certain probability of emitting a light-quantum determined at each instant by the intensity (probably electric) in the wave. So when an atom emits a light-quantum a spherical phase-wave is simultaneously emitted, and crossing over neighbouring atoms of the source will excite other emissions; all these light-quanta will have their internal phenomena in phase and coherent, and will be thus connected with a definite phase-wave.

This statement is necessarily too brief to do more than adumbrate the ideas in this fascinating paper, to which the reader is referred for further information. Enough has been said, however, to show how it may be possible to link energy transport in a corpuscular form with a group of waves; how, in treating these waves, the idea, now fairly prevalent, that the Maxwell vectors are but average values of something which determines probability of light emission when an atom is situated in the wave, is introduced; and how, if these phase-waves in crossing one another interfere and so reduce this probability to zero, we get no light emission from atoms there, *i.e.* no photographic or scattering effects—*i.e.* the black bands of fringes—and this even if the number of energy quanta be very small, *i.e.* the light intensity of the interfering sources very feeble.

ORGANIC CHEMISTRY. By O. L. Brady, D.Sc., F.I.C., University College, London.

β, Nitroarylhydroxylamines and Tetranitrobenzene.—Though compounds of the type of 2:4-dinitrochlorobenzene react readily with hydrazine to give, for example, dinitrophenyl hydrazine, $(NO_2)_2C_6H_3Cl + 2NH_2.NH_2 \rightarrow (NO_2)_2C_6H_3NH.NH_2 +$

$NH_2.NH_2$, HCl, attempts to prepare the β-nitroarylhydroxylamines by an analogous method have not been successful. Borsche (*Ber.* 1923, **56**, [B], 1494, 1939), however, has succeeded in obtaining compounds of this type by the action of hydroxylamine on the polynitroaryl ethers such as 2 : 4 — and 2 : 6-dinitroanisole and 2 : 4 : 6-trinitrophenetole in alcoholic solution (compare also Michael and Browne, *J. Pr. Chem.* [2], **35**, 358).

$$(NO_2)_2C_6H_3OCH_3 + NH_2OH \rightarrow (NO_2)_2C_6H_3NHOH + CH_3OH.$$

The polynitroaryl hydroxylamines so produced are acidic in character, they form sodium salts and can be acetylated with acetic anhydride giving, for example, O-acetyl-β-2 : 4-dinitrophenylhydroxylamine $(NO_2)_2.C_6H_3NHO.COCH_3$. They are stable when dry but decompose in solution with the liberation of nitric oxide and the production of tarry compounds, on oxidation with fuming nitric acid (D = 1·54) the hydroxylamino- is converted to the nitro-group. The latter reaction is of particular interest in connection with 2 : 4 : 6-trinitrophenylhydroxylamine since it leads to the production of 1 : 2 : 4 : 6-tetranitrobenzene which consists of yellow needles melting at 125–6°. Numerous attempts have been made to prepare a tetranitro-benzenoid hydrocarbon. Drost (*Annalen*, 1899, **307**, 49) by the nitration of o-dinitrosobenzene obtained 4 : 6-dinitro-1 : 2-dinitrosobenzene m.p. 172°. Nietzki and Dietschy (*Ber.*, 1901, **34**, 55) by the action of picryl chloride on hydroxylamine hydrochloride stated they obtained a dinitro-dinitrosobenzene m.p. 133°, to which they assigned the same formula as that of Drost, and that by the action of fuming nitric acid they obtained tetranitrobenzene m.p. 116°. Will (*Ber.*, 1914, **47**, 704), however, showed that Nietzki's dinitro-dinitrosobenzene was impure, consisting of Droust's compound mixed with picric acid, and was unsuccessful in his attempts to oxidise the pure dinitro-dinitroso-derivative. Nietzki also claimed to have obtained β-2 : 4 : 6-trinitrophenylhydroxylamine (m.p. 174°), but the unusual behaviour of his product on hydrolysis suggests that he was not dealing with this compound and Borsche reinvestigating has come to the conclusion that the supposed β-2 : 4 : 6-trinitrophenylhydroxylamine was a mixture of picric acid and picramide.

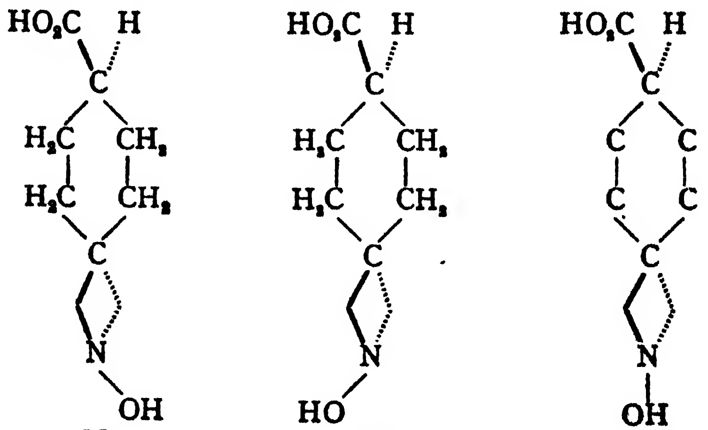
The fourth nitro-group in tetranitrobenzene is, as would be expected, readily mobile, dilute sodium hydroxide solution or nitric acid (D = 1.4) replaces it by the hydroxyl-group giving picric acid, with aqueous or alcoholic ammonia the tetranitrobenzene gives picramide, whilst aniline gives 2 : 4 : 6-trinitrodiphenylamine.

As tetranitrobenzene approximates to a balanced explosive the study of its explosive properties should be of interest,

though its unstable nature would probably make it useless for industrial purposes quite apart from the difficulty of preparation.

The Stereochemistry of Tervalent Nitrogen.—The fundamental postulate made by Hantzsch and Werner when putting forward their stereochemical hypothesis to account for the isomerism of the oximes was that the three valency directions of the doubly-linked nitrogen atom did not lie in one plane. At the time there was no proof of this assumption, and investigations of simple compounds of tervalent nitrogen tended to show that, where three different groups were attached to the nitrogen atom (*abc*N), the valency directions lay in one plane, all attempts to resolve such compounds into optical enantiomorphs being unsuccessful.

Mills and Bain (*Trans. Chem. Soc.*, 1910, 97, 1866) studied the oxime of cyclohexanone-4-carboxylic acid. If the valency directions of the nitrogen atom do not lie in one plane the synthetic oxime would consist of a mixture of two enantiomorphous forms [i] and [ii], but if they are coplanor [iii] this is not possible as the molecule would then possess a plane of symmetry at right angles to that of the paper.

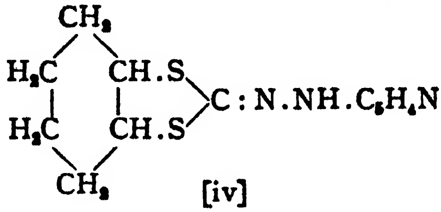


[i] [ii] [iii]

Mills and Bain separated the oxime into two optically active forms, thus indicating that the assumption made by Hantzsch and Werner was correct. The argument was, however, not quite conclusive as the optically active compounds obtained might have been the two forms of the tautomeric

$$HO.NHC\begin{cases}CH_2.CH_2\\CH.CH\end{cases}CH.CO_2H$$

and thus not contain the oximino-group. In order to remove the possibility of such tautomeric change Mills and Schindler (*Trans. Chem. Soc.*, 1923, **123**, 312) have investigated the pyridylhydrazone of cyclohexylenedithiocarbonate [iv].



[iv]

The pyridyl residue was sufficiently basic to ensure salt formation and by fractional crystallisation of the *d*-bromocamphorsulphonates they managed to obtain *d*- and *l*-forms of the hydrazone though not in a state of optical purity. It follows

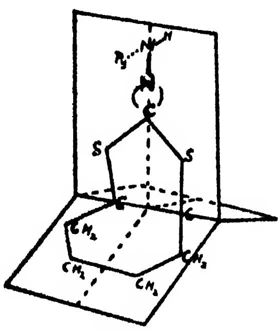


Fig. 1.

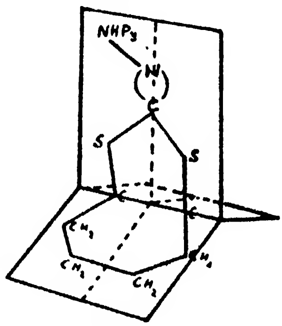


Fig. 2.

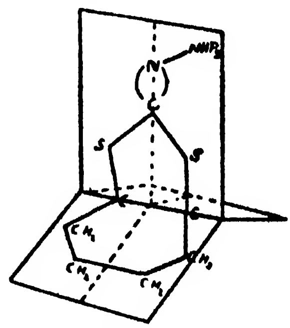
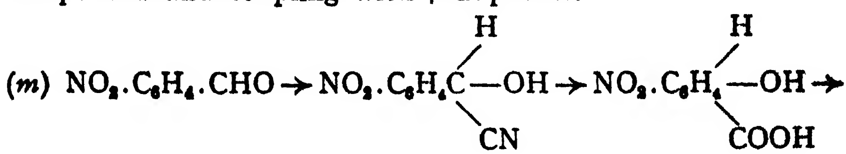


Fig. 3.

that the compound has no plane of symmetry such as it would possess were the three valency directions of the doubly-linked nitrogen atom in one plane (Fig. 1). If these valency directions do not lie in one plane it is obvious that enantiomorphous forms are possible depending upon whether the NHP*y* group projects on one or other side (Figs. 2 and 3).

A New Method of resolving Optically Active Compounds.—Willstätter (*Ber.*, 1904, **37**, 3758) seems to have been the first to suggest that asymmetric protein-like compounds might selectively absorb the optically active components of racemic dyes leading to resolution, but at that time no asymmetric dyes were known. Porter and Hirst (*J. Amer. Chem. Soc.*, 1919, **41**, 1264) prepared a number of such compounds and obtained evidence of selective absorption of one of them by wool, though only of a qualitative nature. Ingersoll and Adams (*ibid.*, 1922, **44**, 2930) state that they obtained evidence of the selective absorption by wool of dyes of the type

R.CH(CO_2H).$NHCOC_6H_4.N_2.C_6H_4N(CH_3)_2$. Porter and Ihrig (*ibid.*, 1923, **45**, 1990) have now succeeded in achieving almost complete resolution of a racemic mixture of an asymmetric dye-stuff by selective absorption. The compound employed was racemic *m*-azo-β-naphthol mandelic acid; *m*-nitromandelic acid [vii] was prepared from *m*-nitrobenzaldehyde [v] in the usual way through the cyanhydrin [vi] and was reduced to *m*-amino-mandelic acid [viii]; this amino-acid was resolved through the cinchonine salt and the *d*- and *l*-*m*-azo-β-naphthol-mandelic acids [ix] prepared by diazotising the optically active compounds and coupling with β-naphthol.



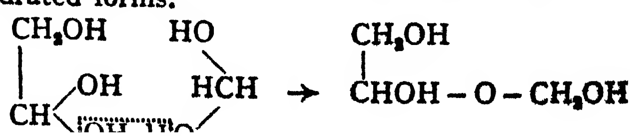
[v] [vi] [vii]

$$NH_2.C_6H_4.CH(OH).COOH \rightarrow HO.C_{10}H_6.N:N.C_6H_4.CH(OH).COOH$$

[viii] [ix]

The enantiomorphous forms, which give optically clear solutions, are identical in most of their properties but have equal and opposite rotations, $[\alpha]_D = 49° \pm 5°$. When wool was dyed in a racemic mixture of these dyes both forms were taken up, but at different rates and by using an excess of wool during 72 hours the residual dye in solution was found to consist almost exclusively of the lævorotatory form. No mention is made in the paper of the nature of the racemic mixture employed, whether it was the natural one prepared from the inactive *m*-amino-mandelic acid or an artificial one obtained by mixing equal amounts of the two active forms.

A New Carbohydrate.—In an examination of the aqueous extract of cabbage leaves Buston and Schryver (*Biochem. J.*, 1923, **17**, 470) have isolated a crystalline compound which they consider to be a simple disaccharide. Analysis and molecular weight determinations indicate the formula $C_3H_6O_4$ and the compound gives a tri-benzoyl-derivative. The authors assign to it the structure [x] and suggest that it is derived from glycollic aldehyde [xi] and formaldehyde [xii] reacting in their hydrated forms.



[xi] [xii] [x]

The compound is not, however, hydrolysed by heating with 5–10 per cent. hydrochloric acid. If any quantity of this compound is available a more complete chemical investigation should yield interesting and important results.

GEOLOGY. By Dr. G. W. Tyrrell, F.G.S., A.R.C.Sc., University, Glasgow.

Metamorphism and Metamorphic Rocks.—The conditions under which rhombic pyroxenes are formed during the thermal metamorphism of various types of sediments and igneous rocks are discussed by Dr. C. E. Tilley (*Geol. Mag.*, **60**, 1923, pp. 410–18).

Prof. J. J. Sederholm has just published another instalment of his great work on the Archæan of Finland under the title, "On Migmatites and Associated Pre-Cambrian Rocks of South-western Finland. Pt. I, The Pellinge Region" (*Bull. Comm. Geol. Finlande*, No. 58, 1923, pp. 158). He has established the existence of two Pre-Cambrian formations of basic lavas and tuffs along with sediments, each with its own suite of dykes. With the aid of these dykes Sederholm has been able to demonstrate no less than four, and possibly five, separate periods of granitic intrusion in the Pre-Cambrian. Each of these has produced the veined, dissolved, and hybridised rocks which Sederholm has named *migmatite*. The metamorphism of the rocks has been mainly chemical in origin, and is due to the interaction of granitic magma with various kinds of lava and sediments. The extraordinary perfection of exposure in the hundreds of wave-cleaned islets in the Pellinge region has enabled Sederholm to arrive at the above definite conclusions in an area of most complicated geological structures. His final theoretical conclusions are reserved for a later work.

In a re-examination of the Start area (South Devon), Dr. C. E. Tilley has found that the structure of the district may be described as an anticlinorium with its axis pitching westward (*Quart. Journ. Geol. Soc.*, **79**, pt. 2, 1923, pp. 172–204). There is a major horizon of green schists resting on a lower group of mica schists. The main metamorphism is believed to have been effected in the Pre-Cambrian. The green schists are of igneous origin, and probably represent contemporaneous lavas of basaltic composition with associated sills of dolerite. The schists in general exhibit a low grade dynamic metamorphism, and find their place in the "epi-zone" of Grubenmann's classification.

G. W. Bain writes on almandine and its significance in the contact zones of the Grenville Limestone (Quebec) (*Journ. Geol.*, **31**, 1923, pp. 617–49). The metamorphism has been produced by the intrusion of the Laurentian granite. The field

work shows a zonal arrangement of rock types, leading from the original crystalline limestone, through diopside-wollastonite skarn rocks, felspar-garnet rocks, biotite-garnet-gneiss, biotite-gneiss, to the granite itself. The changes are believed to be due to progressive pegmatisation and replacement, such that each succeeding (outward) zone is enriched in those constituents in which the preceding one is impoverished.

The serpentine rocks of Connemara, described by Dr. H. B. Cronshaw (*Geol. Mag.*, **60**, 1923, pp. 467–71), fall into two classes, the ophicalcites, and the serpentinised basic igneous rocks. In the former the serpentine is derived partly from forsterite and partly from tremolite in de-dolomitised metamorphic limestones. The second class occurs as small masses in the schists and gneisses, and has suffered considerable stress action, producing schists of the talc-tremolite-anthophyllite type.

Dr. C. E. Tilley has described the metamorphic limestones collected from the moraines at C. Denison, Adelie Land, Antarctica (*Aust. Antarctic Exp.*, 1911–14, *Sci. Repts.*, *Ser. A*, vol. 3, *Geology*, 1923, pp. 231–44). There are numerous mineralogical types which have been produced from both dominantly calcic and dominantly magnesian limestones, of varying degrees of purity, by thermal and dynamic metamorphism. While complex silicates such as epidote, tremolite, pyroxene, etc., have been produced, the maximum degree of metamorphism in calcareous sediments, indicated by the formation of wollastonite, has not been attained.

In the same publication (pp. 259–80), Dr. F. L. Stillwell describes the amphibolites and related rocks of the C. Denison moraines. Numerous types of amphibolite, eclogite, magnesium-silicate gneiss, and plagioclase-gneiss, are described, which fall into the meso- and epi-divisions of Grubenmann's classification.

Dr. Stillwell has also described a metamorphic series of great complexity comprising the rocks in the immediate neighbourhood of the great Broken Hill lode, New South Wales (*Mem. Geol. Surv., New South Wales*, No. 8, 1922, Appendix II, pp. 354–96). The original sedimentary rocks which encompassed the lode are now quartzites and sillimanite-cordierite-gneisses. Original igneous rocks which were intruded into this sedimentary series are now amphibolites, pegmatites, and augen-gneisses. There is also a younger series of epidiorite dykes. The metamorphism has taken place under conditions of high uniform pressure and high temperature, corresponding to Grubenmann's kata-zone. There are numerous shear and crush zones along which a dynamic metamorphism has been superimposed upon the main type.

Mr. E. B. Bailey (*Geol. Mag.*, **60**, 1923, pp. 317–30) has discussed

the grades of metamorphism in the south-western Highlands in relation to his structural conceptions (see SCIENCE PROGRESS, xvii, 1922, p. 212). His map shows four grades of metamorphism on an increasing scale from (pelitic) types with inconspicuous mica, then mica conspicuous, mica with albite, and finally mica with garnet. The metamorphic zones, the outcrops of which agree only very roughly with the structural outcrops, are superimposed upon one another in order of increasing crystallisation from above downwards, and their surfaces of separation are inclined mostly to the north-west. The albitic and garnetiferous types of metamorphism are contrasted; with Mr. Cunningham-Craig, Mr. Bailey agrees that garnet indicates a more thermal, albite a more hydrothermal metamorphism. When albite or garnet is enclosed in a schist undergoing shearing stress, the mineral may respond either by rodding or by rotation. Rodding occurs when the mineral is figuratively "dead," and is easily broken up into rods elongated parallel to the foliation. Rotation occurs when the mineral is "alive," and actively growing. It produces crystals with spirally-arranged lines of inclusions.

Mr. E. M. Anderson has produced yet one more reading of the Dalradian succession, based upon work in the Schichallion district of Perthshire (*Quart. Journ. Geol. Soc.*, **79**, pt. 4, 1923, 423–45). He believes that the Schichallion Quartzite, formerly taken as a single unit, is really composite, and consists of a central mica schist and marginal components which are quartzites of different characters. On the one side of the quartzite group is a graphitic schist and a succession following this in the order previously determined by the Geological Survey. On the other side is a boulder bed, and a further succession of limestones and schists. Mr. Anderson's evidence leads to the view that the last-mentioned rocks are the oldest of the series, and that the Ben Ledi Grits are the youngest. This reverses the generally accepted order; but the validity of the evidence depends largely on an assumption that the Schichallion Boulder Bed is a tillite.

Prof. T. J. Jehu, in collaboration with Mr. R. M. Craig, continues his studies of the geology of the outlying western isles of Scotland with a paper on the Barra Isles, the southern termination of the Outer Hebrides (*Trans. Roy. Soc., Edinburgh*, **53**, pt. 1, 1923, pp. 419–41). The islands consist of Archæan gneiss of igneous origin and of varying mineral characters, which shows in great perfection the development of "flinty crush rock" along zones of dislocation. A series of Cainozoic dykes of olivine-dolerite, crinanite, and camptonite forms the only other rock group within the area described.

Igneous Rocks.—L. H. Adams and E. D. Williamson have

determined the compressibility of certain minerals and rocks under hydrostatic pressures up to 12,000 megabars, corresponding to the depth of 40 kms. below the earth's surface (*Journ. Franklin Inst.*, **195**, 1923, pp. 475–529). The chief petrological result is that, eliminating the effects of porosity, the compressibility of rocks increases with basicity. From the values of the bulk modulus (reciprocal of the compressibility) and of the rigidity, the velocities of the two kinds of waves transmitted through the earth were calculated for the various types of rocks. The value usually taken for the initial velocity of the longitudinal vibrations is a little over 7 kms. per second, whereas for dunite the calculated value is 7·4 kms. per second. Hence the experiments confirm the view that basic or ultrabasic material dominates at a relatively small depth in the crust.

In the eighth paper of his present series on the physical chemistry of the crystallisation and differentiation of igneous rocks Prof. Vogt (*Journ. Geol.*, **31**, 1923, 407–19) deals with the temperature interval of crystallisation in typical examples of the contrasted groups of the anchi-monomineralic and the anchi-eutectic rocks.

Dr. H. S. Washington suggests a new test of the Wegener hypothesis of continental rift and drift by comparing the petrographical provinces on either side of the supposed great fractures, as, *e.g.* the Atlantic rift (*Journ. Wash. Acad. Sci.*, **13**, 1923, 339–47). He comes to the conclusion that the petrographical evidence is, on the whole, adverse to the Wegener hypothesis.

Prof. A. Lacroix has introduced the term ***doliomorphic*** (*Comptes Rendus*, Paris, **177**, 1923, pp. 661–5) to indicate igneous rocks in which minerals occur, such as quartz, olivine, analcite, which are abnormal in respect to the chemical composition. Thus the presence of quartz in highly micaceous rocks, or in rocks otherwise basic; the presence of olivine in rocks which have an excess of silica; or of analcite in rocks showing no normative nepheline; are examples of the ***reactional*** occurrence of these minerals in doliomorphic types.

Dr. N. L. Bowen returns to the discussion of the origin of melilite in igneous rocks (*Jour. Wash. Acad. Sci.*, **13**, 1923, 1–4) in referring to Scheumann's recent work (see SCIENCE PROGRESS, xvii, 1922, p. 364). In an earlier paper he suggested that an alkaline liquid reacted with augite to produce monticellite and melilite, and that an analcite-rich liquid was the final result of the reaction. He has since found analcite in two melilite-bearing lavas. As melilite is practically absent from deep-seated rocks it is suggested that in them the above reaction is reversed.

J. Stansfield describes extensions of the Monteregian petrographic province to the west and north-west (*Geol. Mag.*, **60**, 1923, pp. 433–53). The newly-found occurrences are of peculiar alnoitic and monchiquitic types. One of the masses (the ninth and most westerly of the Monteregian Hills) consists of a new type of plutonic rock named *okaite*, which is composed of melilite, hauyn, and biotite, as essential constituents. Nepheline may be an important accessory. This rock is genetically connected with the alnoite dykes. Bowen's reaction hypothesis to account for the monticellite-alnoite of Isle Cadieux (see SCIENCE PROGRESS, xvi, 1922, pp. 548–9) is questioned on the ground that there is field evidence of the absorption of limestone, giving rise to such silicates as melilite and monticellite.

The same author has propounded an elaborate classification of the rare alnoite group of lamprophyres (*Geol. Mag.*, **60**, 1923, pp. 550–4). Two of the numerous new names proposed by Scheumann are pressed into service, and one other, *bizardite*, is proposed for alnoites in which melilite is supplemented by nepheline. Other types of lamprophyre are considered in relation to this classification. Stansfield has evidently not seen Beger's memoir on the lamprophyres (see SCIENCE PROGRESS, **18**, 1923, pp. 207-8).

An important study of assimilation and thermal metamorphism has been carried out by Mr. H. H. Read in the Arnage district of Aberdeenshire (*Quart. Journ. Geol. Soc.*, **79**, pt. 4, 1923, pp. 446–86). The igneous rock is a norite which is intruded, probably as a sill, into various andalusite-cordierite-schists, felspathic quartzites, biotite-gneisses, and hornblende-schists of the Dalradian succession. Wholesale enclosure of these schists by the igneous rock has led to the formation of a zone of hybridised or *contaminated* rocks, full of xenoliths, and minerals such as cordierite, spinel, garnet, which are unusual or abnormal in igneous rocks. Only small kernels of unmodified norite are now left. It is shown that the contamination process depends upon reciprocal action between the norite magma and the pelitic xenoliths; the magma becomes more acid, the xenoliths more basic. Further discussion of many theoretical points is promised.

In a further paper dealing with the volcanic geology of East Fife (*Geol. Mag.*, **60**, 1923, pp. 530–42) Mr. D. Balsillie shows that the main eruption of the great vent of Largo Law must have taken place in the Lower Carboniferous, as tongues of the ash and agglomerate are interbedded with the sedimentary rocks of that age adjacent to the vent. On this observation he extends the idea of Lower Carboniferous age to the numerous small vents of East Fife which have hitherto been regarded as Permian.

In July 1921, Dr. Charcot, of Antarctic fame, accomplished a landing on the tiny islet of Rockall, which is the summit of a great submarine bank covering over 2,000 square miles in the Atlantic region between Iceland and Ireland. Prof. A. Lacroix has described the new material thus obtained in a series of papers, which the writer has summarised for British petrologists (*Comptes Rendus*, Paris, t. **173**, 1921, pp. 267–73; t. **177**, 1923, pp. 417–22, 437–40. G. W. Tyrrell, *Geol. Mag.*, **61**, 1924, pp. 19–25). Two important new points have been established in regard to Rockall geology. First, the igneous rock of Rockall is an ægirine-granite, and the unique type, *rockallite*, of which it was formerly thought to be entirely composed, is merely a basic segregation. Further, Rockall is igneous from top to bottom, and there are no sedimentary rocks towards the base of the stack as formerly thought. Rockall Bank is proved by dredging to be a submerged basaltic plateau similar to those of Skye, Mull, Antrim, Iceland, Greenland, and the Faroes, which form remnants of the widespread Thulean petrographic region.

A detailed study of the petrology of the Hawaiian Islands is being carried out by Dr. H. S. Washington (*Amer. Jour. Sci.*, **5**, 1923, pp. 465–502; **6**, 1923, pp. 100–26, 338–67); and in the third paper of the series he has been able to give a general statement of the petrology of Hawaii itself. Seventy-five per cent. of the lavas consist of andesites and basalts, including the types oligoclase-andesite, andesine-andesite, oligoclase-basalt, andesine-basalt, and labradorite-basalt. The last-named is the most abundant. There are also subordinate olivine-bearing basalts, and very olivinic types called "picrite-basalt" (= oceanite and ankaramite of Lacroix). Trachyte is also known to occur in at least two localities. The numerous chemical analyses show that there has been a remarkably regular serial progression of types which corresponds with the most probable order of eruptive activity.

The great memoir by Prof. A. Lacroix entitled *Minéralogie de Madagascar*, recently published in three quarto volumes (*Lithologie* in tom. ii, 1922, pp. 219–694; tom. iii, pp. 1–450) contains far more than is at first sight indicated by its title. It begins with a succinct sketch of the geography and geology of Madagascar (148 pp.), goes on to a detailed description of all the minerals so far found, and ends with a complete account of the petrography. It is impossible to enumerate even the various groups of rocks dealt with, much less to summarise conclusions. The schists and gneisses of the crystalline basement and the numerous intrusions thereinto, both calcic and alkalic, are described; then follows the description of a huge series of post-Liassic intrusive and volcanic rocks, including

the famous alkaline petrographic province of Ampasindava. The petrological summary which concludes the work is based upon a magnificent series of 450 chemical analyses.

Drs. E. O. Teale and W. Campbell Smith have described a series of nepheline-bearing lavas and intrusives from Portuguese East Africa (*Geol. Mag.*, **60**, 1923, pp. 226–37). The specimens include phonolites, trachytoid phonolites rich in sodalite, nephelinites, and olivine-nephelinites. Their detailed examination shows that there exist close resemblances between these rocks and many of the lavas of Kenya, Eastern Uganda, and Tanganyika Territory; and it has been found possible to correlate them with the lavas of the succession established by Prof. J. W. Gregory in Kenya. The ages of the rocks range from Cretaceous to post-Miocene. They are associated with a set of tectonic lines related to those of the East African Rift Valley.

Vulcanology.—A chemical study of the fumaroles of the Katmai region of Alaska by E. T. Allen and E. G. Zies (*Nat. Geogr. Soc., Contrib. Tech. Papers, Katmai Ser.*, No. 2, 1923, pp. 75–155) has shown that steam is the predominating constituent emitted, and that the most important remaining gases are HCl, CO_2, H_2S, N_2, HF, and occasionally CH_4. Argon occurs in the nitrogen in practically the same proportions as in the atmosphere, and probably both inert gases came from that source. The water emitted is probably of surface and not magmatic origin. The fumaroles are all aligned along fissures, and their most distinctive characters are of a secondary nature (*e.g.* the symmetrical craters formed by explosion).

In the same publication (No. 1, 1923, pp. 1–74) the origin and mode of emplacement of the great tuff deposit which floors the Valley of Ten Thousand Smokes, and which was formerly attributed to an outburst of the Katmai volcano, is dealt with in detail by C. N. Fenner. He shows that it must have originated within the valley itself, probably through fissures, which communicated with a sill or laccolith intruded into the sediments below. The deposit is regarded as due to a low-level phenomenon analogous to the *nuée ardente* of Mont Pelée, the incandescent avalanche of lava particles and gases which destroyed St. Pierre in 1902. As the lava issued from the fissures it was disrupted into fragments by rapid evolution of gases, not powerful enough for violent explosion, but sufficient to diminish internal friction to such a degree as to cause the material to flood the valley like water.

Dr. H. S. Washington has compared the *aa* (block) and *pahoehoe* (corded or ropy) types of lava from Mauna Loa in an attempt to discover the reason for the structural differences of the two forms (*Amer. Jour. Sci.*, **6**, 1923, pp. 409–23). The *aa* type is the more crystalline of the two, and its ferrous oxide

is lower relatively to ferric oxide. Further, the *aa* type occurs in much larger flows than *paehoehoe*, is less vesiculated on the whole, but carries much larger and more irregular individual vesicles.

According to Dr. Washington these facts are explained by the theory that *paehoehoe* lava issues at a higher temperature than *aa*, but with a much smaller content of gas. The gas soon escapes, and the flow quickly congeals with a minimum of crystallisation. *Aa*, on the contrary, issues at lower temperature, but is so highly gas-charged that it is initially much more fluid than *paehoehoe*. Under these conditions crystallisation begins early and proceeds with rapidity. The escape of gas becomes increasingly rapid and even violent towards the moment of consolidation, but the remaining liquid always remains gas-saturated, and so in a condition favourable to crystallisation.

By a comparison of the chemical compositions of the lava of Katla, Iceland (1918) with that of the Skaptareldhraun (Laki eruption of 1783), Lacroix shows that the extreme differences in the modes of eruption of these lavas are not due to differences in chemical composition, but to variations in the physical conditions which attended the respective eruptions (*Comptes Rendus*, Paris, **177**, 1923, pp. 369–72). The Katla outbreak took place under the ice-cap of the Myrdalsjökull, resulting in rapid cooling and imprisonment of magmatic gases, and consequent violent explosion. The Laki basalt, on the other hand, welled out unhindered from small vents situated along a great open fissure, with the minimum of explosive action.

A very detailed memoir on one of the small areas of extinct vulcanicity in South Germany has been produced by H. Reck (*Die Hegau-Vulkane, Berlin*, 1923, p. 248), in which he has clearly explained the various stages in which the outbreaks took place. The products of the first stage of activity were low tuff cones. Actual eruption of lava (basalt) took place only at a late stage, the molten stream rarely passing beyond the limits of the crater. In the eastern part of the area there are more extensive spreads of tuff and some small plugs of phonolite.

BOTANY. By E. J. Salisbury, D.Sc., F.L.S., University College, London.

Ecology, etc.—Kurz, dealing with Hydrogen-ion concentration in relation to ecological factors (*Bot. Gaz.*, Sept.), finds that the uplands are more acid than the flood-plains, whilst the acidity of flat, badly drained land was high. Dunes near Long Lake

exhibited an alkaline condition in the young phase replaced by an acid reaction in the older phases. This same feature was also shown by the dunes at Mineral Springs and Sawyer. In this respect and as regards diminution of acidity with increasing depth, the results are in conformity with those obtained by Salisbury in this country. Many of the species were observed to occur in soils exhibiting a wide range of reaction, from which Kurz concludes that acidity is not the master factor for their distribution. Observations at different seasons in the same localities show comparatively slight seasonal fluctuation of reaction.

Sager, in a privately printed paper on Soil Acidity studies (Cambridge, 1923), records determinations in Switzerland of the soil from various plant habitats. In general the soil in the shade appears to be more acid than in the open, an observation which harmonises with the known effects of coppicing of woods. The most acid soils were pH.4·6 with *Vaccinium myrtillus, V. vitis-idæa,* and *Luzula nivea,* whilst the most alkaline was 7·7 with *Bromus erectus. Sesleria cærulea* was found on soil as acid as pH.6·8 and *Amelanchier ovalis* on soil of pH.5·7.

Dr. E. Hess has just published an account of the woods of the Bernese Oberland in which the view is adopted that the distribution of *Rhododendron* above the present tree limit marks the former extent of trees. From this it is concluded that the forest limit is between 1,900 and 1,950 m. and the tree limit about 2,050 m. (6,812 ft.). The present forest limit is at 1,920–1,950 on south aspects and 1,900 m. on north aspects, but trees and stunted specimens occur to 2,050 m., to which altitude the *Rhododendron* also extends. The chief forest trees are the Beech (up to 1,570 m.) and *Picea excelsa* (1,930 m.). Locally *Pinus montana* also forms stands to the tree limit.

The phytoplankton of fifty-six ponds has been described by B. Millard Griffiths (*Jour. Ecology,* Sept. 1923). Of these *Asterionella* or *Ceratium* or both occur in thirty-eight, the pools from which they are absent being either shallow or, if deep, with a strong through current. From his observations the author concludes that the presence and amount of plankton depends on the amount of decay-products of sediment and that the character of the plankton is largely dependent on whether the decomposition is an aerobic or an anaerobic one. In the latter case the seasonal vertical circulation of the water in autumn and spring is held to account for the bimodal periodicity of some Diatoms.

The distribution of root-hairs is a factor which will have to be considered by ecologists when estimating the efficiency of varying extents and types of root system. Our knowledge

on this point is, however, meagre. Jeffrey and Torrey, in 1921, pointed out that persistent root-hairs occur in *Aster umbellatus* correlated with an absence of secondary thickening. Persistent root-hairs are also known in *Robinia*. Recently Whitaker (*Bot. Gaz.*, Sept.) has added further examples from herbaceous species of Compositæ in which it seems that the root-hairs may persist and function for several years. These may be distributed over the entire root or in extreme herbs may be restricted to the basal region (basipilose). *A. umbellatus* is an example of the former and *Aster tataricus* of the latter class. Persistent root-hairs were also found in *Solidago serotina*, *S. patula*, *S. rugosa*, and *S. graminifolia*, in *Eupatorium purpureum*, *Vernonia crinita* v. *Baldwini*, *Chrysanthemum uliginosum*, *Dahlia* spp., *Lysimachia* spp. Some species which are devoid of root-hairs in the adult condition may be basipilose in the seedling stage. The persistent root-hair is regarded as an accompaniment of reduction, or absence, of secondary thickening correlated with the assumption of the herbaceous habit.

Dziubaltowski (*Acta Soc. Bot. Poloniæ*, vol. i, no. 3) considers the steppe formations of Poland to be largely independent of soil conditions. They occur exclusively on southern exposures, where the air and soil temperature are appreciably higher than on the northern exposures and the soil humidity is considerably lower. The rapid melting of the snow on the south exposures leads to erosion which impoverishes the soil and augments the water deficiency. The steppe with *Stipa capillata* as the dominant persists under conditions unfavourable to the development of ligneous vegetation.

Genetics, etc.—It is now nearly sixty years since Darwin called attention to the fact that *Lythrum salicaria* possesses three types of flower involving three lengths of style and three lengths of stamen. Each individual, as is well known, bears flowers of only one type, and Darwin showed that more or less complete sterility results from self-pollination or even from crossing with another individual when the pollen was not derived from stamens of the same length as the style pollinated. Stout (*Amer. Jour. Bot.*, Oct. 1923) has recently made a detailed study of self-pollination in this species and finds a considerable degree of variation with respect to sterility in illegitimate self-pollination. Amongst the long-styled plants a few (18 out of 97) were feebly self-compatible, whilst the short-styled plants showed an even lower proportion (1 out of 23). Medium-styled plants, however, show a high proportion of individuals which are self-compatible in varying degrees (48 out of 112). Legitimate crosses with medium-styled plants gave pods with from 12 to 176 seeds (average

98 seeds), whilst illegitimate crosses yielded pods with from 0–117 seeds (average 33 seeds).

Schaffner (*Ecology*, vol. iv, Oct. 1923), continuing his investigations on the effect of environment on sex in hemp (cf. *Bot. Gaz.*, **71**, 1921), has made successive plantings throughout the season to test the effect of the relative lengths of daylight and darkness. The variety used showed no monœciousness, but all the plants raised during the period of long days and short nights were either carpellate or staminate. Reduction of the light intensity had no apparent effect on the sex. With the approach of the winter solstice (about 9 hours daylight as compared with 15 hours in the summer), the successive plantings showed a more and more marked tendency towards sex reversal, the staminate plants becoming carpellate and *vice versa*. As the author points out, the most striking feature is the fact that in the winter sex-reversal occurs in *both* directions. Moreover, flowers which are partly male and partly female in a variety of abnormal types are also produced. The author concludes that the attempt to explain sexual expression by hypothetical homozygous and heterozygous Mendelian factors is of little value. It will be recalled that Schaffer had formerly shown (*Amer. Jour. Bot.*, **9**, 1922) that the phenotypical expression of sex in *Arisæma* is dependent on the environmental factors of water content and nutrition, and the effect of external conditions has also been demonstrated for other plants (e.g. *Silene* spp., *Myrica gale*), and sex-reversal has been shown to occur in various animals (*e.g.* crabs, fowls, etc.).

The occurrence of two periclinal chimeras in *Nicotiana tabacum* is described by Clausen and Goodspeed (*Genetics*, March 1923). The first of these appeared in the F1 generation of a cross between the red-flowered female *macrophylla* and the white-flowering "Cuba" as male. The F1 plants were pink-flowered, but the individual in question produced a small branch of the inflorescence with white flowers. White or pink flowers when selfed gave practically identical ratios of pinks, reds, and whites. The second case originated from a cross of *purpurea* (ϕ) with Cuba and bore carmine flowers on one part of the inflorescence and pink flowers on the other. Root cuttings from the pink region produced carmine flowers, as did those from the carmine portion. The localisation of pigmentation in sections agrees with the interpretation of these plants as periclinal chimeras.

In a symposium on sterility in plants (*Am. Jour. Bot.*, Nov.) Stout reports that *Brassica pekinensis*, which showed only 10 per cent. of individuals which were highly self-compatible, when grown so as to reduce the vegetative vigour

yielded 65 per cent. of highly self-compatible individuals. Davis, in a second contribution to the same discussion, emphasises the very varied manifestations of sterility: sometimes exhibited by the failure of the gametes themselves to develop, by unhealthy development of the gametophytes, or by weakness and death of the embryo in the early phases of its development. The lethal factors due to hybridisation are responsible for the failure of all but the heterozygonus types in hybrids which breed true, the surviving offspring being held to contain balanced lethal factors. East deals with self- and cross-sterility in *Nicotiana* as conditioned by Mendelian factors apparently governing the rate of development of the pollen tube. Finally, Dorsey discusses the matter from the horticultural standpoint.

The account of the natural putative hybrids in the New Zealand flora is given by Cockayne (*New. Phyt.*, July). The number enumerated is 128, belonging to 33 families, and showing a higher proportion amongst shrubby plants than for other biological types.

Taxonomy.—A supplement to the *Journal of Botany* commencing in the June number on New Guinea plants collected by Dr. H. O. Forbes contains descriptions of new species belonging to the following genera: *Michelia* (Magnoliaceæ); *Himantandra; Cyathocalyx* (2 sp.) (Anonaceæ); *Cochlospermum* (Bixaceæ); *Ternstræmia, Adinandra* (Ternstrœmiaceæ); *Sterculea; Sloanea, Elæocarpus* (Tilaceæ); *Evodia* (3 spp.) *Canarium; Dysoxylon, Chisocheton, Dasycoleum, Amoora, Aglaia* (Meliaceæ); *Iodes* (Olacineæ); *Ilex; Hippocratea; Zizyphus; Mucuna* (2 sp.); *Cæsalpinia, Archidendron, Hansemannia; Angelesia* (Rosaceæ); *Polyosma; Terminalia; Decaspermum, Eugenia* (19 spp.); *Barringtonia; Dissochæta, Astronia* (Melastomaceæ); *Casearia* (Samydaceæ); *Polyscias, Eschweileria* (Araliaceæ). The descriptions of the foregoing are by Mr. Baker, whilst Mr. Moore adds species of the following genera of Gamopetalæ and Monochlamydeæ: *Uncaria, Mussænda Canthium, Morinda, Psychotria, Amaracarpus, Saprosma* (Rubiaceæ); *Mæsa, Ardisia, Conandrium, Discocalyx* (Myrsinaceæ); *Sideroxylon; Clitandropsis* (gen. nov.), *Alyxia, Alstonia, Ervatamia, Ichnocarpus* (2 spp.) (Apocynaceæ); *Marsdenia* (Asclepiadaceæ); *Fagræa* (3 spp.) (Loganiaceæ); *Ehretia; Solanum; Tecomanthe* (Bignoniaceæ); *Piper* (2 spp.); *Cryptocarya, Litsæa* (Lauraceæ); *Phaleria; Elytranthe* (Loranthaceæ); *Glochidion, Phyllanthus* (2 spp.), *Antidesma* (2 spp.), *Acalypha* (2 spp.), *Macaranga* (spp.); *Ficus* (4 spp.), *Parartocarpus; Casuarina, Casania.* Dr. Rendle is responsible for additional species of the following monocotyledonous genera: *Cordyline, Freycinetia, Raphidophora.* Mr. Jepp describes species of *Davallia, Hypolepis, Asplenium* (2), *Dryopteris, Polypodium,*

and *Lycopodium;* and Dr. Dixon two species of *Macromitrium.* Mr. Pugsley in the July number adds a Calamintha (*C. bætica*) to the British Flora, and incidentally another plant to the Lusitanian element in the West of England.

Howarth, dealing with *Festuca rubra* in the aggregate sense (*Jour. Linn. Soc.*, Jan. 1924), recognises the following as present in Britain: *F. heterophylla* in a restricted area in the Thames valley; *F. rubra* with two subspecies, of which one, *F. rubra gemina*, has eight varieties: and *Festuca rubra fallax*, which is the commonest Fescue of the chalk downs. *Festuca juncifolia*, a species of the coast, is distinguished from *F. rubra* by the acute leaves, extravaginal branching, and continuous sclerenchyma beneath the lower epidermis of the leaf.

Sherff (*Bot. Gaz.*, Sept.) describes ten new species of *Coreopsis*, and in this and the October number twenty-one species of *Bidens.*

Rydberg, dealing with the North American Galegeæ, distinguishes eleven subtribes, *viz.* (1) Craccanæ, (2) Millettianæ, (3) Brongniartianæ, (4) Barbierianæ, (5) Sesbanianæ, (6) Diphysanæ, (7) Corynellanæ, (8) Robinianæ, (9) Coluteaneæ, (10) Astragalanæ, and (11) Glycyrrhizanæ (*Am. Jour. Bot.*, Nov.).

Chemin contributes a Flora of the marine algæ of Luc-sur-mer to the *Annales des Sci. Naturelles* (I, v, No. 1–2), in which 208 species are enumerated, the majority (116) being Florideæ.

Morphology and Anatomy.—The flower of the Cruciferæ has been a perennial source of discussion in the past and has been again revived by Miss Saunders (*Ann. Bot.*, July 1923) in view of certain abnormalities which have appeared in *mathiola incana.* Lindley nearly a century ago propounded the hypothesis that the cruciferous fruit consisted of four carpels, of which the two valves represented one pair and the commissural ridges and replum the other. It is this view that Miss Saunders elaborates, holding that the primitive Cruciferous gynæcium consisted of two whorls of four carpels each, of which one whorl is sterile and the other fertile. One of the most cogent arguments for this view is the association of double-line sutures with commisural stigmas and of single-line sutures with stigmas corresponding in position to the loculi. The Orchidaceous ovary is similarly interpreted as possessing six carpels, of which three are infertile.

Sister Lamb (*Bot. Gaz.*, Oct.), furnishes a key to the Cycadean genera based on leaf characters. Of the species examined, *Macrozamia moorei* and *Bowenia serrulata* have stomata on both surfaces, as Nestler found for *Bowenia spectabilis. Bowenia* appears to be peculiar also in the absence of a hypoderm and of calcium oxalate crystals.

The numerical variation of florets in the capitula of various Compositæ is the subject of a paper by Szymkiewicz (*Acta, Soc. Bot. Poloniæ*, vol. i, No. 3). Usually the terminal capitulum contains the largest number of florets, whilst the number in the lateral capitula tends to augment from above downwards.

ZOOLOGY. By REGINALD JAMES LUDFORD, Ph.D., B.Sc. (Lond.), University College, London.

Chromosome Work.—J. S. Huxley has extended his work on the Amphipod, *Gammarus chevreuxi*. He finds the cross-over value between the loci of two mutant genes (red and albino), for the 4,528 offspring of 33-pair matings continued for 3 to 10 broods, was 44·69 per cent. The chromosomes are relatively small and round, or ovoid in form. In view of their small size, he concludes "that the cytological basis of crossing-over in this species is probably different in essential respects from that found in *Drosophila*, where it appears that a considerable actual length of chromosome must separate loci before they can show high cross-over values." Contrary to what is found in *Drosophila*, the percentage of crossing-over is low in the first three broods and rises later (*Brit. Jour. Expt. Biol.*, vol. i, no. 1, 1923).

Important contributions to our knowledge of the cytology of the marsupials have been made by W. E. Agar and A. W. Greenwood. The following chromosome numbers have been determined :

	Male	Female	
Macropus . . .	10 + XY	10 + XX	W. E. A.
Petauroides . .	20 + XY		W. E. A.
Phascolarctus . .	14 + XY	14 + XX	A. W. G.
Sarcophilus . .	12 + XY	12 + XX	A. W. G.
Dasyurus . . .	12 + XY		A. W. G.

The Y chromosomes are very small in each case, and the X are smaller than the autosomes. Agar points out that "probably in *Macropus*, and more convincingly in *Petauroides*, the cytological conditions to permit of 'crossing over' are present in the male."

The Golgi Apparatus.—S. D. King and J. Bronté Gatenby have succeeded in demonstrating the presence of a typical Golgi apparatus in the Coccidian, *Adelea*. It is comprised of dictyosomes or crescentic rodlets, sometimes joined together. During growth the excentric Golgi apparatus becomes larger and tends to spread out in the protozoon cell body. A form of dictyokinesis has been observed, the Golgi bodies being drawn into subequal groups around each of the daughter nuclei. The nucleus of the merozoite contains a peculiar

nucleolus, probably of the nature of a karyosome, which always lies at one end of the nucleus. The Golgi apparatus occupies a position in the cytoplasm at the opposite end of the nucleus. No centrosome was distinguishable in this protozoon, and the writers are of the opinion that the Golgi bodies are attracted by some other body in the nucleus (*Q.J.M.S.*, vol. 67, part 3, 1922).

That different malignant growths of the rat, mouse, and guinea-pig exhibit characteristic forms of the Golgi apparatus has already been described by Da Fano. He has recently extended his observations to cases where some of the same tumours are undergoing absorption. In general, he finds that the apparatus becomes fragmented at an early stage when other cell constituents are still apparently unaltered, but such fragments seem to possess a great power of resistance to degenerative conditions (*Q.J.M.S.*, vol. 67, part 3, 1923).

F. W. R. Brambell has studied the activity of the Golgi apparatus in the neurones of *Helix aspersa*. He finds that the Golgi apparatus gives rise to argentophil and oxyphil clouds in the cytoplasm. Basophil granulations, he believes, represent the tigroid bodies and are most marked in those cells in which the apparatus is most active. He concludes that " the function of the Golgi apparatus is connected with the cytoplasmic metabolism, possibly the production of protein substances " (*Jour. of Phys.*, vol. lvii, no. 6, 1923).

Tissue Cultures.—The unique opportunity afforded by tissue cultures has led W. H. Lewis to study cultures of embryonic chick cells, in the hope that new information might be gained concerning cell structure. The tissues on which observations were made included mesenchyme, endothelium, smooth muscle, ectoderm, endoderm, liver, sympathetic nerve fibres, heart muscle, and skeletal muscle. These were examined by the method of dark-ground illumination. The cover-slips containing the cultures were laid culture-side downward on to drops of Locke solution contained on ordinary slides, which were ringed with vaseline to prevent evaporation. The Locke solution usually contained some intra-vitam stain such as neutral red or janus green. Cultures set up in this way remained alive for periods, varying from a few minutes to several hours. In the cells, fat globules were seen brilliantly illuminated, and other granules, probably of the nature of degeneration products, were white. Mitochordria in the form of either threads, rods, or granules were grey, while with neutral red they appeared pink and the degeneration granules were yellow. In certain of the cells, the granules and mitochondria showed Brownian movement at room temperature ; little such movement was found in smooth muscle and skeletal muscle, and none in

heart muscle, showing that there is considerable variation in the viscosity of different cells. The karyolymph of the cells appeared dark and homogeneous and was surrounded by a thin white line, while the nucleolus appeared as a group of fine white granules. With the onset of necrosis, white granules made their appearance, increased in numbers, and persisted until the cells dissociated (*Anat. Rec.*, vol. 26, no. 1, 1923).

A further series of observations have been made by the same investigator on amniotic ectoderm growing in tissue cultures. The culture medium consisted of Locke solution containing chicken bouillon and dextrose. Epithelial cells migrated from the fragment of tissue planted in the culture medium, in the form of sheets or membranes, and cells passed out individually, becoming isolated, in the same manner as has been described by other workers. The tissues were stained with intra-vitam dyes and fixed by Lewis's iodine method. In such preparations, the mitochondria showed great diversity of form, appearing as threads, plexuses of threads, rods, or granules. Neither centriole nor centrosphere was observed in the epithelial cells (*Anat. Rec.*, vol. 26, no. 2).

T. S. P. Strangeways and H. E. H. Oakley have carried out experiments with cultures of the choroid of chick embryos in order to study the immediate changes following exposure to soft X-rays. The cultures were made in chick plasma and chick embryo extract, and were maintained at a temperature varying between 36·5° C. and 39° C. Twenty-four-hour-old sub-cultures were used, and were exposed to soft rays filtered through 2 mm. of cardboard. The alternative spark gap was 8 cm. with a rectifying spark of 2·5 cm. and a water resistance in series with the tube. The cultures were placed 24 cm. from the cathode, and the current was kept at about 1 ma. In the first series of experiments, cultures were exposed to the X-rays for varying periods and then fixed immediately in alcohol acetic. Definite changes were recognised after 30 minutes' exposure, and these changes became more pronounced as the time of exposure was increased. The earliest effects noticed were granular changes in the chromosomes during metaphase and anaphase, and fewer cells undergoing mitosis. Not until cultures had been exposed to the rays for 60 minutes were changes found in the "resting" cells. In the second series of experiments the cultures were exposed to the X-rays for varying lengths of time and were then incubated for 80 minutes before being fixed. This time was chosen as allowing sufficient time for a cell to complete the mitotic process. Also it allowed for any lag in division, or any immediate after-effects of exposure to the X-rays to become visible in the case of the "resting" cells. The result of this series of experiments

was to show similar changes as were observed in the others, but there was found to be a latent period of about 15 to 20 minutes before the changes in the cells can be definitely recognised. Also there was found a clumping or agglutination of the chromosomes in some of the cells at metaphase. Such cells do not divide, but after a period of quiescence, break up and finally disintegrate. In general, the authors conclude, the changes in the cells do not appear to differ from changes found in cells which are growing in an unfavourable or modified medium (*Proc. R. Soc.*, vol. 95, no. B669, 1923).

A most useful critical survey of the work on tissue culture has been made by H. M. Carlton, whose paper contains a bibliography of the principal contributions to the subject (*Jour. Expt. Biol.*, vol. i, no. 1, 1923).

The Embryology of the Sympathetic Nervous System.—Owing to the controversy that exists as to the origin of the sympathetic nervous system, considerable interest attaches to a paper by A. Subba Rau and P. H. Johnson, on the development of the sympathetic nervous system and suprarenal bodies in the sparrow. These investigators find that the primary sympathetic primordia originate from the spinal cord, and therefore have an ectodermal origin. These primordia are formed at inter-somitic points, their constituent cells being derived from the neural crest, dorsal root ganglia, and neural tube. "The primary ganglionic swellings extend dorso-laterally. The free ends of these extensions enlarge, and, becoming separated from the primary ganglia, give origin to the definitive sympathetic ganglia." From the primary sympathetic system are derived the secondary sympathetic chains, the chromaffin cells of the medulla of the suprarenal, the cœliac and other ganglia, and the unpaired ganglia in the cervical region. Observations made during the course of this investigation have shown that genetic continuity exists between the primary and secondary sympathetic chain, the former giving rise to the latter, contrary to the view of His, jr. (*P.Z.S.*, part 4, 1923).

Sex-reversal and the Origin of Germ-cells.—F. A. E. Crew has described a number of cases of sex-reversal in the fowl which form a consistent series illustrating the conversion of actively functioning hens into actively functioning males. He has shown that "in the hen there are successive invasions of the ovary by the sex cords derived from the peritoneum, and that the assumption of the male characters by the hen is prevented by the presence of the growing oocytes. In the absence of the inhibiting influence of the growing oocytes, the male-determining reaction becomes effective, spermatic tissue is differentiated from the invading sex-cords, and as a result of

this the characters of the individual become those of the male" (*Proc. R. S.*, vol. 95, no. B667, 1923).

A histological study of the fowls described by Crew has been made by H. B. Fell. The origin of the spermatic tissue from the peritoneal epithelium is described, and its almost identical similarity with the normal embryonic process is pointed out. In every case the development of testicular tissue was preceded by ovarian atrophy or disease (*Brit. Jour. Expt. Biol.*, vol. i, no. 1, 1923).

J. Bronté Gatenby and F. W. R. Brambell have described a similar case of sex-reversal in the fowl, in which the spermatic tissue was also produced from the peritoneal epithelium (*Jour. Genetics*, 1924).

These researches on sex-reversal have raised anew the problem of the origin of the germ-cells, as they have struck a serious blow at the Weismann germ-plasm theory. Gatenby has therefore reinvestigated, by the aid of the new cytological technique, the origin of the germ-cells in Amphibia. He states that the "peritoneal epithelium of the ovary of the adult frog contains here and there, especially during spring and early summer months, a multitude of islands of thickened areas, which are formed by metamorphosing germinal epithelial or cœlomic cells." From such cells originate the germ-cells of the frog (*Jour. R.M.S.*, part 4, 1923).

An interesting review of the more important papers on sex-reversal and intersexuality is contributed to the same number of the *Jour. R.M.S.* by F. W. R. Brambell.

Transplantation Experiments.—A number of interesting experiments on transplantation of organs have recently been carried out in the Institute of Experimental Biology of the Academy of Science, Vienna. In the first of a series of papers describing this work, H. Przibram discusses the method of autophor transplantation. He points out that with this method, the blood-vessels, nerves, and other tissues which have been cut through in the course of the planting of the graft are fitted closely together, and therefore, the conditions are favourable for a quick reunion of the tissues, their healing and return to functional activity. This method, Przibram considers, is capable of a wide application to theoretical as well as practical problems, and in view of the results obtained, in transplanting the eyes of animals, the possibility of applying the method to the human eye is not regarded as hopeless.

T. Koppányi has used this method in carrying out a large number of experiments in replanting eyes of various vertebrates. He has investigated the physiology of the replanted eyes of mammals, and also their growth. A histological study of replanted eyes has been made by W. Kolmer.

Head transplantation in water-beetles has been carried out by W. Finkler. His method consists in drawing out the head, away from the thorax, and then severing it from the body. The head is then replanted on another insect which has been treated in the same manner. The little bleeding which occurs is not harmful. On the contrary, it assists in the fixation of the head, and prevents the drying up of the wound later on. Sutures and other methods of fixing the head are unnecessary. After this operation the animals are transferred to a special moist chamber until co-ordinated movements are re-established. During the first week a " fusion tissue " is formed between the head and thorax. After one or two months the head has recovered its full functional activity, and the insect swims and eats normally.

Finkler's paper is illustrated with photographs of the transplantation operation and also with photographs of some of the animals experimented on. *Hydrophilus* with a transplanted *Dytiscus* head, and *Dytiscus* with the head of *Hydrophilus* are shown swimming.

Limb transplantation has been successfully performed by P. Weiss in *Amphibia.* He finds that the fully developed limb of the larva of *Salamandra maculosa* can be transplanted against another limb of the same side of the body or on to other parts. A small piece of the limb is sunk in the body, but the greater part is left free as in the normal condition. The " transplant " heals, grows, and undergoes metamorphosis with the rest of the body. After a time it becomes fully functional, making active movements.

Other operations of a similar character that have been successfully carried out are the replanting of the lens of the eye of fish by B. P. Wiesner, and the transplanting of the ovary of one rat into the uterus of another, which is followed by the production of offspring (*Archiv für Mikr. Anat. und Entwickl.*, 99 bd., 1 heft, 1923).

ANTHROPOLOG By A. G. Thacker, A.R.C.S., Zoological Laboratory, Cambridge.

The *Proceedings of the Prehistoric Society of East Anglia* for 1922–23 (vol. iv, pt. 1) are now to hand. The volume follows tradition, and most of the papers again refer to researches carried on in Eastern England. Of these, perhaps, the most interesting is that entitled " The Maglemose Remains of Holderness and their Baltic Counterparts," by A. Leslie Armstrong. It will be remembered that the Maglemose culture of which the type locality is at Mullerup, in Zeeland, is the oldest culture in Scandinavia, and is equivalent to the Azilian-

Tardenoisian of Western Europe. It dates from the time when the Baltic Sea merely constituted the "Ancylus Lake" of prehistoric geography, and is considerably older than the Danish kitchen-middens. The chief Maglemosian remains now described from the Holderness district of Yorkshire are two bone harpoons found respectively at Skipsea and Hornsea. Both are very well preserved. The Skipsea specimen, which is nearly five inches long, was found in 1903 under a lacustrine deposit five feet deep, and was discovered during the excavations of the bones of a large deer, apparently the Great Irish Deer *Cervus giganteus* (the writer also calls it an "elk," but *C. giganteus* was quite a different animal from the elk). The specimen, which is illustrated in the paper, was barbed on one side only, and strongly recalls the Magdalenian harpoons of the south of France, as Mr. Armstrong remarks. The Hornsea specimen, found in 1905, was much larger, being more than ten inches long. It was discovered in a similar situation beneath a lacustrine deposit. It will be readily understood that at a time when the Baltic was reduced to a lake, the flat lands of Holderness were a part of a much larger flat lake-studded area, which covered much of what is now the southern part of the North Sea. In 1923 Mr. Armstrong carried out some excavations himself at Skipsea, and unearthed a number of small flint implements, scrapers, piercers, and so on; and some of these are duly figured in the paper. Doubt has been thrown on the genuineness of the two harpoons, but quite unnecessarily, and Mr. Armstrong successfully disposes of the allegations. It is impossible at present to be sure of the racial characters of the makers of these Maglemosian implements, but Mr. Armstrong is to be congratulated on the fleeting glimpse which he gives us of these very early settlements in Yorkshire.

Another article is the presidential address by Mr. Henry Bury, which was on "Some aspects of the Hampshire Plateau gravels." This was read in London on October 10, 1923. The article deals principally with the large changes in sea-level which must have taken place in Palæolithic times, and a great many relevant data from the Hampshire, Thames, and Somme valleys are considered. He comes to the conclusion that in the Chellean there was a maximum sea-level as high as 150 feet O.D. and that in the Mousterian there was a minimum as low as *minus* 80 feet O.D. The paper will repay careful study.

The whole of the issue of the *American Journal of Physical Anthropology* for July–September 1923 (vol. vi, No. 3) is taken up with a paper by G. G. MacCurdy entitled "Human Skeletal Remains from the Highlands of Peru." The specimens concerned were collected by an expedition which went to Peru in 1914 under the auspices of Yale University and the National

Geographic Society, and they were found in caverns in the mountains north-west of Cuzco. The description is very thorough and the paper is well illustrated, but the interest of the work is in masses of detail that do not lend themselves to summarizing.

The following papers may also be noted:

In the Proceedings of the Prehistoric Society, E.A., 1922–3 (vol. iv, pt. 1): "On some Further Flint Implements of Pliocene Age discovered in Suffolk," by J. Reid Moir; "Palæolithic Types of Implements in Relation to the Pleistocene Deposits of Uganda," by E. J. Wayland; and "Ancient Flint Mines at Stoke Down, Sussex," by Major A. G. Wade. And in the *Annals of Archæology and Anthropology* (vol. x, nos. 1 and 2), May, 1923: "Were the Ancient Egyptians conversant with Tablet-weaving?" by G. M. Crowfoot and H. Ling Roth; and "The Distribution of Pompeius' Forces in the Campaign of B.C. 67." by H. A. Ormerod. And in recent numbers of *Man*: "The Study of the Chronology of Palæolithic Cultures in Relation to the Various Glacial Deposits," by M. C. Burkitt (January); and "Stone Circles in Gambia," by Northcote W. Thomas (February).

ARTICLES

METHODS OF EVOLUTION[1]

By Prof. J. E. DUERDEN, M.Sc., Ph.D., F.Z.S.
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All epoch-making discoveries have in their wake a revision of pre-existing ideas; and it is often impressed upon us with humility that our interpretations of phenomena are but tentative, determined by the state of knowledge for the time being. The confident assertiveness of one period becomes the doubt of another, and the rejection of a later; and thus knowledge grows. Particularly is this found to be the case in our interpretations of vital phenomena, which from their very complexity so far transcend human comprehension; and perhaps no problem so forcibly illustrates this as that of evolution. All accept the historical fact of descent, but as to its causes and methods all phases of assertiveness, doubt, and rejection have been passed through since Darwin first impressed it upon an unwilling world of thinkers.

Despite all this uncertainty, one cannot withstand the temptation to view one's work in the light of evolutionary theory, and to cherish the hope that it may not be altogether profitless, as forging a link in the seemingly endless chain of attempts to interpret the diversity of life. In the present state of biological inquiry one would indeed be rash to pin one's faith wholly to any one method of evolution to the exclusion of all others. There is much wisdom in the recent insistence of Prof. W. E. Ritter in *Science* that the ways of evolution are *many*, not *one*. My main purpose is to show how differently the same facts can be interpreted when viewed from the diverse aspects of to-day.

The Basis of Homology—Origin of Feathers

Mendelism, involving as it does the distinctness of the characteristics of an organism and the conception of discrete

[1] Presented before the Zoological Section of the South African Association for the Advancement of Science at its recent meeting in Bloemfontein, as an introduction to a general discussion on the Methods of Evolution.

germinal factors, is one of those fundamental principles which insistently confront us when we try to interpret zoological phenomena. We are so accustomed to think in terms of gradual transformation that it is with some reluctance we turn to discontinuity. The whole of our comparative morphology and homology is founded upon gradual transformation and involves the idea of genetic relationships, yet if we are to accept Mendelian teachings this is not the manner in which the change from one form to another has occurred; rather, each modification is discrete and discontinuous in its nature, and apart from those which have gone before and from those which may follow.

Perhaps no better concrete illustration of the two divergent views can be adduced than that afforded by the problem of the evolution of feathers. Last year I brought forward certain observations upon the origin of feathers from scales in the ostrich (*Rep. S. A. Assoc. Adv. Sc.*, vol. xix, 1922). It was there shown that where the minute scales of the leg pass into the feathered part above they bear rudimentary feathers at the time of hatching. The scale and the feather are associated as if one and the same structure, the latter an outgrowth of the former, and the epidermal and dermal layers of the two are alike and continuous.

Here we have what may be deemed to be the introduction into the animal kingdom of a really new character of prime significance—the growth of a feather from a scale. Whether the condition in the ostrich chick actually represents the first and simplest stage may be questioned, but of the relationship between the two there can be no doubt.

Two courses are open to us in our interpretation. Following the older methods of comparative morphology, we can maintain that the relationship of scale and feather affords evidence that in the course of evolution the reptilian scale has grown upwards into a filament, and by a complicated system of incisions of the epidermis, due to ingrowths of the dermis, the filament has become frayed out, and given rise to the many structural divisions of the feather—shaft, barbs, and barbules. This is the view which has hitherto been largely accepted. Moreover, it is possible to establish a dorso-ventrality for the feather, represented by the shaft and after-shaft, and not difficult to homologise it with the overlap of reptilian scales, and to find corresponding parts in the unguis and sub-unguis of claws, nails, and hoofs.

But no, says the Mendelian, a feather is a new structure; it is *sui generis*; it is an epidermal mutation, the result of a separate germinal change; its origin is quite apart from that of scales. The fact that it grows from a scale, making

use of the same epidermis and dermis, is merely incidental, and has no bearing upon its genetical nature. The overlap of scales, and claws, nails, and hoofs, are also distinct characteristics and, in so far as they differ from one another and from feathers, they are dependent upon separate factorial representation; and to establish a correspondence of parts does not imply a unity of origin. Each separate structure of the body has a separate factorial representation in the germ plasm; scales and feathers are distinct structures, and are the manifestations of distinct factors. A factor by its nature does not change; its manifestations may be influenced, as in degree, by modifying factors, but a factor giving rise to a scale could by no means be influenced so as to give rise to a feather.

It is at once obvious that the same argument can be applied to all our teachings of homology and comparative anatomy. The delight of the morphologist in the tracing of a corresponding structure through all its many transitional stages, from its one extreme to the other, is but a delusion if a genetic relationship is the underlying idea. Take the homology of the constituent parts of the vertebrate skull or of the visceral arches, through which we all struggled in our early zoological days. There was an assumption of gradual transition, yet coupled with genetic continuity, underlying all our attempts to trace the same osseous element from one stage to the other of the vertebrate series. But all this seems futile if we are to accept the factorial hypothesis and the mutative discontinuity which it involves. We return to the conception of homology taught by Cuvier and Owen, who merely held a homologue to be "the same organ in different animals under every variety of form and function." The idea of genetic unity in addition to unity of type, applied to homology since Darwin's days, must needs be abandoned as not in harmony with evolutionary processes, a conclusion which may well cause us to examine our bearings before accepting it as one of the verities of zoology.

Discontinuity of Retrogressive Changes—The Feet of the Ostrich

To illustrate again from the ostrich. It is unique among living birds in that it has but two toes, a large inner and a small outer, the third and the fourth of the pentadactyle series. On the theory of gradual transformation we should assume that this condition has slowly evolved through successive stages from a four- or even a five-toed ancestor, and is possibly transitory to still further retrogression. As regards its ancestry, Broom (*Trans. S. A. Phil. Soc.*, 1906, p. 362) remarks: "It is interesting to note that the embryo ostrich has five toes more

or less developed, and the three central ones fairly well developed. We are probably justified in inferring that the immediate ancestor of the ostrich had three functional toes, and possibly that the more remote ancestor had a functional hallux." The fact that the fourth toe is much smaller than the third may signify either that it has not increased in size concurrently with the third or, more likely, that it has undergone a retrogressive change; the latter idea being strongly supported by the fact of the partial or entire loss of the claw and loss of part of the scutellation.

Among a large number of ostriches all stages are procurable in the loss of the scales from the big toe, and can be arranged according to a definite orthogenetic sequence; also, all stages from a well-defined claw on the small toe to its complete disappearance. In an earlier paper (*Jour. Genetics*, vol. ix, 1920) these were deemed to suggest that the toes were possibly still in process of degeneration; but the facts presented below afford no support for this. They seem best regarded as the small variations, both fluctuating and germinal, always to be met with in a mixed population.

We have, however, unmistakable palæontological evidence against any evolutionary change in the toes of the ostrich within a comparatively recent geological period. From the Pliocene of the Siwalik Hills of Northern India the tarso-metatarsus of an ostrich, *Struthio asiaticus*, has been obtained, with its two distal articular surfaces in a well-preserved state. A comparison of the specimen in the British Museum which I have been able to make with the corresponding bone of the living ostrich shows the relative dimensions of the two to be exactly alike. In the words of Lydekker (*Brit. Mus. Cat. Fossil Birds*, London, 1891, p. 211): "The tarso-metatarsus cannot be distinguished from the corresponding bone of *S. camelus*." Hence we may assume that since early Pliocene times the toes of the ostrich, working on these articular surfaces, have maintained the same relative sizes.

If no alteration has taken place during these tens of thousands of years, we may well be assured that they have been fixed characteristics of the bird since the time when they first assumed their present relative proportions. Functional adaptability is probably not concerned; for, if it be held that the one-toed condition of the horse is advantageous for rapid progression, would not a broad, padded, single toe be more serviceable for the ostrich? But the two toes seem as well fixed in their present relative proportions as in any other feature of the bird. A stage in retrogression is therefore not necessarily succeeded by another. There appears to be a discreteness, a completion, about the evolutionary stage

in the foot of the ostrich which would hardly be expected of a condition which has been attained as a result of gradual, continuous transformation; for it is reasonable to ask why it should proceed to a certain stage and no farther. We have, moreover, no evidence that the two-toed state has been attained by gradual reduction from a three- or four-toed ancestry, any more than there is evidence that it will ever become one-toed.

Lamarckism—The Ankle Callosities of the Ostrich

Before leaving the ostrich I cannot refrain from referring to another feature of evolutionary significance, which is directly dependent upon the reduction of the toes. It is set forth as a remarkable illustration of the continued persistence of a non-functional structure, and of the non-heritable nature of a new adaptive character, consistently acquired through hundreds or even thousands of generations.

When half crouching, the whole weight of the ostrich is supported upon its ankle-joint behind and its two toes in front, the toes and tarso-metatarsus forming a wide angle with the ground; when fully crouching, the weight becomes partly borne by the large sternal callosity in front and the small pubic callosity behind. At the time of hatching a well-developed callosity is present over the ankle-joint, symmetrical with the long axis of the shin bone and the big toe; but the chick does not make use of this in crouching. It rests upon an adjacent inner projection of the ankle, and the skin and scales here gradually thicken and form an accessory pad.

The interpretation of the hereditary and of the acquired callosities is simple when we consider the evolutionary history of the ostrich. In its ancestral, three-toed condition the crouching bird would have three points of support in front, and the weight would be transmitted through the median part of the ankle, symmetrical with the three toes. In this case the median, hereditary callosity would be functional. When the bird lost the inner (second) of the three toes, the front axis of support was changed, and the tarso-metatarsus and ankle-joint tilted inwards, in such a manner that the bird rested upon a new projection at the ankle, and the response of the skin to the pressure and friction with the ground produced a new thickening adjacent to the old. Its gradual formation can be observed on any ostrich chick.

It is therefore manifest that in its ancestral, three-toed stage the hereditary axial pad was functional in the ostrich, but on mechanical grounds has been useless since Pliocene times, when the bird had become two-toed. Though non-functional, however, it still persists in its full development,

Since the assumption of the two-toed condition, the adaptive, accessory callosity must have been formed anew with each generation, and yet has not become transmissible.

We thus have a natural experiment, extending over thousands of generations, demonstrating that a useless character does not necessarily retrogress, while an acquired modification of a like nature does not necessarily become hereditary. Lately the alternative view, namely, that an acquired character may become transmissible, had been much to the fore, largely through the persistent advocacy of Prof. E. W. MacBride (SCIENCE PROGRESS, July 1923), who finds his support mainly from the experiments of Prof. Paul Kammerer, University of Vienna, with amphibians and ascidians. It is an advocacy which every zoologist desires to see justified by further experiments, and thus afford an explanation of much that is hitherto inexplicable in adaptive evolution. The ways of nature are many.

ORTHOGENESIS—THE LIMBS OF SERPENTIFORM LIZARDS

Impressed with the fixity of the reduction phase in the foot of the ostrich, studies have been made of certain of the many instances of degenerate limbs and limb-girdles met with in the lizards and snakes of South Africa. A brief account of the serpentiform lizards comprised under the genus *Chamæsaura* was given at the last meeting of the Association. Three species of the genus are known, restricted to South Africa, namely, *C. ænea*, *C. anguina*, and *C. macrolepis*, all found under similar conditions. The retrogression and loss of limbs are here associated with an increased length of tail, not with an increased length of body, the latter being the case in many limbless lizards (*Acontias*) and all snakes.

In *C. ænea* both the fore and hind limbs are of the normal lacertilian type, but greatly reduced in size, the former varying in length from 9 to 11 mm. and the latter from 14 to 16 mm. Within the limits of these small fluctuations the size of the limbs appears to be fixed, without transitional stages towards the larger limbs of ordinary lizards or the smaller ones of the two other species. The numerous specimens dissected show no departure from the pentadactyle type as regards the number of digits and phalanges.

In *C. anguina* also both pairs of limbs are present, but the digitate character is practically lost, each limb being styliform, with only a hint of an external separation into one or two minutely clawed digits. The fore-limbs are only 6 mm. in length and the hind 8 mm. Dissection reveals but two digits in the fore-limb, the second and third of the pentadactyle

series, each with a loss of certain of the phalanges. The hind-limb shows rather more variation. The third digit may have two or three phalanges and the second only one or two; or in the latter they may be wholly wanting, and only a vestige of the metatarsus remain. In the tarsus a single proximal cartilage occurs and one or two elements in the distal row.

Two specimens only of *C. macrolepis* have been available, and both show further reduction compared with the previous species. The fore-limb has entirely disappeared, though its girdle is present in its entirety; while the hind-limb is a mere vestigial style 6 to 7 mm. in length, terminating in an extremely minute claw. Only the middle digit remains, with two phalanges, while the metatarsus and the tarsus are represented by but a single bone.

Comparison of the number of specimens available suggests that the retrogressive stage is fixed and distinctive for each species, due allowance being made for the variations always met with in any mixed population. Taking the three species as a series, however, they present practically every stage in the degeneration of the pentadactyle limb from its normal condition to its complete absence in the case of the fore-limb of *C. macrolepis*. On the older interpretation they would undoubtedly be regarded as a continuous evolutionary series, illustrating the gradual stages according to which limb degeneration proceeds: first a diminution in size, second a loss of the first and fifth digits, then the disappearance of the second and fourth, and finally complete loss of the whole limb. On the limbs alone it might be assumed that *C. ænea* was the older form, nearest the original ancestor, that *C. anguina* has evolved from it, and that *C. macrolepis* is the final derivative, though all have survived.

From a Mendelian standpoint, however, the three stages call for a different interpretation. Each may be regarded as the manifestation of a separate and distinct germinal change in the common ancestor, fixed for the time being, and not necessarily transitional towards further loss. Each would represent a distinct phase of degenerate evolution, wholly unconnected with the two others; *anguina* need not have passed through an *ænea* stage, nor *macrolepis* have descended from *anguina*. Unlike the reduction in the toes of the ostrich, no adaptive advantage can here be involved as between one stage and another, and selection cannot have been operative. The loss of limbs, correlated with the elongation of the tail, restricts the creatures to a crawling habit, and may enable them to move more freely among the vegetation and under objects and within crevices, and thereby slightly to extend the sphere of their activities. But we can just as readily hold that any

change of habit has been necessitated by the reduction of the limbs and elongation of the body.

Taking a survey of the large family of lizards, Scincidæ, Gadow has also remarked ("Amphibia and Reptiles," *Camb. Nat. Hist.*, vol. viii, p. 560): "Every stage from the fully developed and functional pentadactyle limb to complete absence of limbs is represented. There are species within the same genus with five, four, three, or two fingers or toes. There are Skinks without fore-limbs, but with vestigial hind-limbs, and *vice versa*. The interesting point is that these reductions do not indicate relationship within the family, but have happened independently. They are impressive illustrations of convergent retrogressive evolution."

If we accept the Mendelian interpretation of separate and independent factorial changes in the ancestral germ plasm for a living transitional series such as *Chamæsaura* affords and the Scincidæ generally, are we not impelled to do the same for all the striking transitional series which palæontology affords, and which are deemed to be the best illustrations we possess of continuous orthogenetic evolution? Were the former found fossil, particularly in successively later strata, the three species would most certainly be held to be a successional genetic series in limb degeneration. May not doubt even be cast upon the accepted phylogeny, say, of the horse, as interpreted from palæontological records? If the successive digitate formulæ of *Chamæsaura* have been separately acquired without genetic significance, may not the four-, three-, and one-toed condition of the horse likewise represent independent losses, the result of distinct germinal changes? The fact that the stages are found in successively younger rock formations is significant, but has no necessary bearing upon the genetic relationships. Mendelism seems to impel us to think of the many known transitional graded series of both extinct and living forms as having arisen apart and independently, not in an orthogenetic sequence, much after the manner which Morgan has shown to be the case with regard to the graded series of wings, body colours and eye colours procurable among the mutants of *Drosophila*.

Fixity and Persistency of Vestigial Structures—Limb Girdles of Snakes

The thread snakes, *Glauconia*, and the worm snakes, *Typhlops*, as well as the Pythons and the slow-worm *Acontias*, all possess relics of the pelvic girdle in one phase or another of degeneration, and in certain instances vestiges of the hind-limbs also. Numerous specimens of each are in most cases available in South Africa, and from the study of these it has been possible

to draw certain conclusions as to the manner in which degenerative evolution takes place. Details, with illustrations, will shortly be published, but the results may be briefly indicated.

In the case of any one type it has been found that the degenerative phase is a fixed characteristic, though with considerable fluctuation, such as might reasonably be expected in vestigial structures probably of no functional value. Thus as regards *Glauconia*, over thirty specimens have already been dissected, and it has been possible to compare the girdle with that of a species in distant Venezuela. Yet practically no departure from the type has been encountered, and nothing to suggest that the degenerate stage reached is not as fixed for the genus as is any of its other characteristics. Five of the hundred or so species of *Typhlops*, widely distributed throughout tropical and sub-tropical regions, have also been examined and, with fluctuating variations in all directions, the degenerative phase of the pelvic girdle appears to be the same wherever the genus occurs.

No one questions the original ancestral nature of the limbs and limb-girdles of the lizards and snakes, and that the numerous degenerate types now met with represent different stages in their retrogressive evolution, which has probably taken place independently in a part of the germ plasm common to all. A proper interpretation of the conditions which they display may therefore help in some measure to indicate how evolution proceeds. On the principle of evolution by gradual transformation it would be difficult to conceive that the same phase could have been reached by all the individuals of a species or genus in any particular area, much less by representatives in areas far apart. There is, however, a definiteness, a completion, about each particular phase, wherever found, which negatives the idea that they are transitional from something which has gone before to something which will come after.

When the limbs have wholly disappeared, as in the case of *Typhlops* and *Acontias*, and the girdles are altogether free from any attachment to the ribs or vertebral column, it is difficult to believe that the vestiges of the girdles have any functional value ; and yet the parts persist at a fixed stage for the particular type. And one becomes impressed with the meagre justification there is for the assumption that their retrogression is in any way associated with disuse, or that useless, vestigial structures are necessarily on the road to final elimination. One seems compelled to the conclusion that the degenerate changes in any particular type are the result of definite germinal changes, complete in themselves.

PROF. DUERDEN ON EVOLUTION

By C. TATE REGAN, F.R.S.

A criticism of Prof. Duerden's article in the same number of Science Progress may perhaps be justified by the fact that it has been put forward for discussion elsewhere: it is based on papers he has already published, two of which, on the callosities and on the feathers of the Ostrich, are of a different nature from the others and may be considered first.

Prof. Duerden has observed that the sternal and pubic callosities of the Ostrich are developed before the chick is hatched; this was interpreted by him as evidence that the effects of use are inherited, a conclusion that seems not unwarranted. The callosities on the foot and ankle are also developed before hatching, but, according to Prof. Duerden, the hereditary ankle pad, functional in the ancestral three-toed Ostrich, is so no longer, the bird resting on an inner pad that develops after hatching in response to pressure and friction with the ground. He concludes that a useless character need not degenerate and a useful one need not become hereditary—a conclusion that seems justified by the facts as stated. But one would like further evidence that the inner pad does not develop except in response to pressure and friction and that the "hereditary" pad is not functional. The two pads appear to be contiguous and their surfaces seem to be in the same plane, and it is not easy to see how an ostrich crouching in the sand, and resting or rolling about, could use one pad without the other. I put this forward not to challenge Prof. Duerden's observations, but to ask him to make things clear.

Prof. Duerden's interesting studies on the feathers and scales of the Ostrich might perhaps be summarised as follows: just below the feathered part of the leg rudiments of feathers appear and attain a certain development, but finally abort, the cause of their suppression being apparently the formation of scales round their bases. From this I should not like to draw any conclusions as to the evolution of the feathers of Birds from the scales of Reptiles; indeed, some embryologists have deduced from similar facts that the scales of Birds are secondary, and not persistent Reptilian scales.

The remaining papers support the mutation hypothesis.

Prof. Duerden is concerned with what he believes to be the bearing of Mendelism on evolution, but one may question whether he is quite up to date in this matter. I recall Miss Saunders's contribution to a debate at Cardiff in 1921 : " Mendelism is not a theory of evolution, but a theory of heredity." It is true that for a number of years biology in this country suffered from Mendelian dominance, and that during that period the Mendelians made the most confident statements as to the methods of evolution. But we are given to understand that this attitude of assertive dogmatism has been to some extent modified, and that nowadays the word " evolution " is not mentioned in the best Mendelian circles. Moreover, quite recently, the distinguished Danish geneticist Johannsen has proclaimed (in *Hereditas*) that Mendelism is concerned mainly with abnormal variations and that the problem of evolution is not seriously approached through it.

But, leaving on one side the question whether Prof. Duerden interprets the teachings of Mendelism correctly, we have to consider certain facts that he regards as favourable to the mutation hypothesis. He says there is no evidence that the two-toed state of the foot of the Ostrich has been attained by gradual reduction, but here he appears to have neglected the embryological evidence, which many zoologists would regard as conclusive. As no alteration has taken place in the foot of the Ostrich since early Pliocene times, he asks why, if this condition has been attained gradually, the transformation has proceeded to a certain stage and no farther ? The answer would seem to be that the structure is perfectly adapted and that the outer toe persists because it is used. However, Prof. Duerden argues that functional adaptability is probably not concerned, for if the one-toed condition of the horse be advantageous for rapid progression, would not a single toe be more serviceable for the Ostrich ? Although this kind of argument had been put forward before, it is difficult to believe that this fantastic comparison of a quadruped with a biped is meant to be taken seriously. On similar lines one might say that the structure of the fore-limbs of Bats has no relation to adaptation for flight, because those of Birds are altogether different.

Prof. Duerden's investigations of the degeneration of the limbs and correlated elongation of the body in Reptiles with crawling or burrowing habits lead him to similar conclusions. He considers that we can hold that the change of structure necessitated the change of habits. We can, if our conception of evolution enables us also to believe that certain fishes lost their eyes and then swam into underground caves, or that some terrestrial quadrupeds, finding their limbs converted into paddles, made haste to get into the water.

THE ENDOCRINE GLANDS AND THEIR INTERNAL SECRETIONS

A Brief Historical Survey

By P. T. HERRING

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The belief is common among primitive peoples that by eating the flesh of man or animal the partaker acquires the physical, moral, and intellectual qualities which characterised them when living. Courage, the most desirable of virtues, was more particularly identified with the heart, and the custom arose of eating the heart of a courageous foe, man or animal, to acquire this virtue. Closely akin is the practice among some savages of transferring their blood one to another in the belief that they are thereby made members of the same family, endowed with the same attributes. Sir James Frazer, in *The Golden Bough*, has much to say about these customs, and regards them as part of a widely ramified system of Sympathetic or Homœopathic Magic.

The doctrine of Homœopathic Magic appears to be of great antiquity. Sir James Frazer calls attention to the passage in Ovid (*Metam.* vii, 272 *et seq.*) in which Medea restores the aged Æson to youth by a method which may be aptly compared to the intravenous injection of modern times. After many incantations and purifications, Medea pierces the neck of Æson with a dagger and pours into the wound a decoction containing, among other ingredients, the liver of a long-lived deer and the head of a crow that had outlived nine generations of men. Ovid's description of the rapidly rejuvenating effect of the altered blood as it courses through the body is most realistic, and prompts the thought that more was known about the circulation of the blood in Ovid's time than is generally supposed.

To find an " elixir vitæ " which should restore youth to the aged must have been a constant dream in man's history. From an attempt at its realisation may be dated the beginnings of the modern practice of utilising extracts of certain parts of the body for therapeutical purposes. In 1889 Brown-Séquard,

then a man of seventy-two years of age, claimed to have rejuvenated himself and many animals by the injection of extracts of the testes of other animals. Born in Mauritius, the son of an American father and a French mother, Brown-Séquard studied medicine and took his degree in Paris. Thereafter he travelled extensively and practised his profession in Mauritius, Paris, London, and America. He was for a time Professor of Physiology in Harvard University, and eventually succeeded the great French physiologist Claude Bernard as Professor of Experimental Medicine in the Collège de France at Paris.

It had long been known that the presence of the gonads, testes in the male and ovaries in the female, is necessary to the body for the development and maintenance of the sexual characteristics. Bordeu, a Court physician to Louis XV, is stated to have been the first to ascribe this influence of the gonads to an internal secretion; but Berthold in 1849 was the first to prove the fact experimentally. Brown-Séquard really drew attention to the subject, and went further in ascribing to the gonads not only the property of regulating the sexual characters, but also the power of rejuvenation of the body by means of an internal secretion.

The conception of an internal secretion as a function of certain organs of the body was mainly established by the genius of Claude Bernard. Born in 1813, Claude Bernard was one of the pioneers who made modern physiology. Among his many discoveries was that of the function of the liver, in storing sugar as glycogen and liberating it into the blood to supply the tissues as they require it. Bernard applied the term " internal secretion " to the action of the liver cells in giving up sugar to the blood. The expression " internal secretion " might be used to denote the passage of any material, waste or otherwise, from the cells of the body into the blood; but physiologists now use it in the more restricted sense to indicate the secretion by some organ into the blood of a definite chemical material which is essential to the proper maintenance of function of other organs. Brown-Séquard probably received his inspiration from the work of Claude Bernard, but his own work had not the same exactitude.

The new treatment was used extensively in the hospitals of Paris, and the " Brown-Séquard Elixir," as it was termed, had great repute for a time. Gradually, however, its popularity waned and eventually its employment became a matter of ridicule. The treatment was said to have produced an improvement in some of the numerous patients who received it, but time showed that it did not prolong the allotted span of life in any appreciable degree. Brown-Séquard, himself, died in

1894 at the age of seventy-seven, five years after his alleged rejuvenation.

Quite recently a modification of Brown-Séquard's treatment has been revived, and has attracted considerable attention by reason of frequent paragraphs in the daily papers, where it is designated the " monkey-gland treatment."

This time a Russian, Dr. Serge Veronoff, also working in the Collège de France and a surgeon to the Russian hospital in Paris, has introduced a method of transplanting testicular tissue into the bodies of elderly men. The experiments were first of all made upon sheep and goats, and it is claimed that sufficient success was met with to warrant its trial upon man. The difficulty was then encountered that grafts of testes from most animals are incapable of survival in the human body, and in order that survival may be possible it is necessary to employ grafts from human testes or from the testes of a species of animal as closely allied to man as possible. Voronoff accordingly used grafts from the testes of the chimpanzee, and reports that in many instances men so treated have undergone a marvellous transformation. Elasticity has been regained, mental and muscular vigour restored, and even sexual potency is said to have returned. Dr. Retterer, who has worked with Voronoff, claims that the transplanted grafts frequently survive for long periods, and the inference is drawn that the surviving cells furnish an internal secretion which brings about the rejuvenating effect. Opinion is still divided as to the exact cells of the testis which produce the internal secretion. Besides the ordinary cells of the tubules, whose main function is the production of the spermatozoa, there are interstitial cells, the cells of Leydig, whose function may be the formation of an internal secretion. Professor Steinach of Vienna calls the interstitial cells the " puberty gland," on the supposition that they furnish the internal secretion which controls and directs the bodily changes occurring at puberty. Voronoff and Retterer, on the other hand, believe that the ordinary glandular cell is the active agent producing not only spermatozoa but the internal secretion, and claim that it is this cell which survives in their experiments.

The advantage of the graft over the injection method of Brown-Séquard is obvious, provided that the internal secretion of the surviving cells really has a rejuvenating effect, a thesis which still remains to be proved. The grafting method has this disadvantage, that the necessary material is rare and expensive, for chimpanzees are not readily obtained, and if recourse were had to human material the practice might be fraught with grave social dangers.

Dr. Marshall, Reader in Agricultural Physiology in Cam-

bridge, in a recent article upon the subject (*Nature*, Dec. 22, 1923, p. 904), looks upon Voronoff's operation as being still in the experimental stage, possibly capable of bringing about some result, but not necessarily all that is claimed. In regard to some of the beneficial results recorded, Dr. Marshall is of the opinion that the effects of "suggestion" are not satisfactorily excluded.

Apart from the question of rejuvenation, it is now thoroughly established that the testes and ovaries do produce internal secretions, and continual work is in progress to ascertain their exact effects. The actual substances formed by the cells and secreted into the blood have not been isolated, and nothing is known of their chemical composition. Poehl in 1891 claimed to have prepared a substance of definite composition from the testes which he called "spermine," and stated that its injection into the body has a beneficial influence. Extracts of the gonads and other organs are prepared from animal material by the manufacturing chemists, and are widely advertised in medical journals. Their use is at present empirical, but good results are said to follow their administration in certain conditions.

Internal secretions are now known to play a very important part in the physiology of the body, and the organs producing them are called "endocrine" organs or glands (*ἔνδον*, within, *κρίνω*, I separate). The endocrine organs are sometimes termed "ductless glands," because they possess no ducts and their secretion passes either directly from the gland-cell into the blood, or into the blood indirectly through the lymph-stream. The term "ductless gland" is not an appropriate one in this connection, for there are ductless glands, such as the spleen and lymphatic glands, which liberate formed elements or white corpuscles into the blood and lymph, yet do not, so far as is known, produce an internal secretion. The term "endocrine" has a definite meaning, and can be applied to all cells which have the function of secreting into the blood a chemical material having a physiological action upon some other part or parts of the body.

The endocrine organs comprise such diverse structures as the thyroids, parathyroids, suprarenal capsules, pituitary body, pineal body, islets of Langerhans of the pancreas, and certain cells in the mucous membrane of parts of the alimentary canal and in the male and female gonads. Besides these there may be other tissues in the body which produce an internal secretion, but are not as yet definitely recognised as having this function.

Some of the internal secretions have been identified chemically and even synthetically prepared by the manufacturing chemist for therapeutical purposes. Others are recognised only

by the effects they produce upon certain living structures; their composition is unknown. Some can be prepared in a more or less impure state from the organs producing them by methods of extraction and precipitation. Such is the present position with respect to the recently discovered internal secretion of the islets of Langerhans of the pancreas named "insulin." In the course of time it is to be hoped that the exact chemical nature of this substance will be determined and that it may then be prepared synthetically. The expense of extracting it from the pancreas is very great owing to the small amount actually existing in any one animal.

In 1902 Dr. Bayliss (now Sir William Bayliss) and Professor Starling, then at University College, London, ascertained that the stimulus to the secretion of pancreatic fluid during digestion is an internal secretion liberated by the epithelial cells of the first part of the small intestine, which is passed into the blood and carried by it to the pancreas. The internal secretion of the epithelial cells of the intestine they termed "secretin."

This discovery opened up a new and fertile chapter in physiology. It was at once apparent that the activity of an organ may be influenced by chemical substances carried to it by the blood as well as by nerve impulses reaching it through the nervous system. Bayliss and Starling gave to these internal secretions which act as chemical messengers the name "hormone" (ὁρμάω, I stir up), a term which is now thoroughly established. A hormone is a chemical excitant, but the effects of its action are not necessarily those of increased activity. Professor Sir E. Sharpey Schafer has proposed the general term "autacoid" (αὐτός, self, ἄκος, a remedy) to denote a chemical substance formed in the body which has a drug-like action. He has also divided autacoids into two groups: one, the hormone which brings about increased activity; the other, the "chalone" (χαλάω, I make slack), which diminishes or stops activity. The expression "hormone" is, however, commonly employed to denote any internal secretion or chemical messenger, irrespective of whether its action is stimulating or inhibitory.

Hormones are supposed to be chemical substances of comparatively simple composition and of some stability. They are not destroyed by heat, and differ in this respect from enzymes or ferments. Their influence is exerted in diverse ways; some are rapid in action and are quickly destroyed or eliminated from the body, so that their effects are not too prolonged; others act slowly, particularly those influencing growth, and are cumulative in their effects upon the body.

Some autacoids are not destroyed by the process of digestion, and may therefore be administered by the mouth and at

into the blood from the alimentary canal without loss of activity. The active principle of the thyroid gland is an example of this class. Others, again, are unfortunately destroyed and cannot be so given. Insulin furnishes an example of the latter class. It is true that a certain amount of insulin may be absorbed, especially when administered in alcohol; but the amount of absorption is uncertain, and as the material is expensive and a carefully graduated dose is necessary, it is the custom to give it by hypodermic injection.

The first definite step in the successful treatment of disease by the administration of a tissue extract was made in 1892, when Dr. George R. Murray, a physician in Newcastle-upon-Tyne, gave hypodermic injections of extract of the thyroid gland of the sheep in cases of myxœdema. Very shortly afterwards Dr. Fox and Dr. Hector Mackenzie were able to demonstrate that the extract was equally effective in curing the condition if given by mouth. The rapidity and complete success with which the cure is effected is the more striking because of the extreme difference which exists between the physical and mental conditions of the myxœdematous and normal individual. It were difficult to imagine a greater contrast. The treatment was extended to that of "cretinism," and met with equal success. The use of thyroid in these conditions was not empirical, but followed upon a great deal of experimental work on animals which revealed the importance of the thyroid glands and gave some knowledge of their functions. In 1856 Schiff had ascertained that extirpation of the thyroids in dogs invariably led to fatal results. In 1873 Sir William Gull described "a cretinoid state supervening in adult life in women" to which the name "myxœdema" was subsequently applied; and in 1878 William Ord, a physician to St. Thomas's Hospital, verified the relationship of this disease to changes in the thyroid gland. Subsequent work by Kocher in Switzerland, and by Reverdin, established the identity of the symptoms of loss of thyroid in man with those produced by the experimental removal of the gland in animals. Further knowledge of the condition was revealed by the work of Victor Horsley and others in this country and by Munk in Germany.

The nature of the autacoid, hormone, or internal secretion of the thyroid has been much investigated. In 1895 Baumann, a German chemist, isolated a substance rich in iodine to which he gave the name "iodo-thyrin." More recently Kendall in Chicago has identified the active principle as a compound of iodine with oxyindole, and terms it "thyroxin." The manufacture of the autacoid in a pure form is not a matter of much therapeutical moment, for the extract of thyroid does equally well, is readily obtained, inexpensive, and stable. Its administra-

tion by mouth is so convenient and satisfactory that attempts to transplant grafts of thyroid in order to get a continuous liberation of the autacoid in the body have not been tried to any extent, though unsuccessful experiments of this nature were made before the oral treatment by extracts was established. Temporary success by grafting was claimed, but the grafts were sooner or later absorbed and their effects lost.

The autacoid of the thyroid has the property of stimulating the metabolism or chemical changes in the body, and without its influence the fire, so to speak, fails to burn. The thyroid, in its action, has been compared with the draught of a furnace, stimulating more especially oxidation in the body. An excess of thyroid secretion leads to increased metabolism and may go on to the production of the disease known as Graves's disease or exophthalmic goitre. Deficiency of the secretion has the opposite effect, and is the cause of cretinism in the child and of myxœdema in the adult. Intermediate conditions are not infrequent, and the physician finds in thyroid extract a useful and powerful remedy when properly applied.

Closely associated anatomically with the thyroid gland are the parathyroids, minute glands which were first described by Sandström in 1880. Their independence of the thyroid was recognised by Gley of Paris in 1891, but the distinction was not at first appreciated. The autacoid furnished by the parathyroids has not been isolated, but has been provisionally named "parathyrine" by Sir E. Sharpey Schafer. Its absence is said to be responsible for a condition known as "tetany," in which certain convulsive movements of the extremities occur. The symptoms are alleviated, according to MacCallum of America, by the administration of calcium salts, and the parathyroids have been thought to regulate the calcium metabolism of the body. Other workers have failed to substantiate this view, and more recently it has been shown by Professor Nöel Paton of Glasgow that the parathyroids control the metabolism of certain nitrogenous materials, and that in the absence of the parathyroid secretion a poisonous substance, guanidine, passes into the circulation and produces the symptoms of tetany. The hypodermic injection of extracts of the parathyroids has been used in the treatment of tetany, and, it is claimed, with success. Little, however, is known about the nature of the autacoid, and although parathyroid extract is prepared by some of the large manufacturing firms, its potency and the indications for its use are more or less unknown.

An example of an autacoid which has an immediate and extremely powerful action is that found in the medulla of the suprarenal glands. The cells which secrete this hormone are called chromaphil or chromaffine cells, from the property they

possess of being readily stainable by chromic acid and its salts. They are not strictly confined to the medulla of the suprarenals, but occur in other situations in associations with cells of the sympathetic ganglia.

The physiological action of extracts of the suprarenals was first discovered by Foá and Pellacani in 1879, but was more thoroughly investigated by Schafer and Oliver, working in University College in 1895. The autacoid was isolated in an impure condition by V. Fürth in Germany and by Abel in America. It was eventually prepared in a crystalline form by Takamine, a Japanese chemist working in Chicago, and by Aldrich, also in America. It has received various names, suprarenin, epinephrine, and adrenalin; the latter name, given to it by Takamine, has been adopted by the British pharmacopœia. The substance is also prepared synthetically and has other trade names.

Adrenalin, when injected subcutaneously or intravenously, stimulates all those structures which are supplied by sympathetic nerves, giving a general effect similar to that of universal stimulation of the sympathetic nervous system. Its action upon the individual organ depends therefore upon the nature of the response of that organ to sympathetic stimulation, increased activity following if the nerve is excitatory, diminished activity if the nerve is inhibitory.

Brown-Séquard was the first to ascertain experimentally that the suprarenals are essential to life, and it is now generally agreed that the cortex of the gland and not the medulla is the vital part. Different views are held as to the function of the medulla. Many physiologists, influenced by the work of Neil Stewart, an Edinburgh graduate who has taken a prominent part in the development of physiology in America, do not regard adrenalin as an internal secretion; some take the view that adrenalin is only secreted in certain states of emergency; while others regard it as a true internal secretion of the greatest importance. Whatever may be the destiny and significance of adrenalin in nature, there is no doubt that its discovery has provided Medicine with a powerful and quickly acting drug which is often of great value to the physician.

Little is known about the function of the cortex of the suprarenal body. Addison's disease, a rare condition described in 1854 by Thomas Addison, then a physician to Guy's Hospital, is associated with destructive lesions of the gland, and is characterised by great muscular weakness and a peculiar bronzing of exposed parts of the body. The use of extracts has not been attended by any great success in this condition, though temporary improvements have been noted.

The pituitary body, or hypophysis cerebri, a small gland

in the skull at the base of the brain, has a dual origin like the suprarenals, and its two parts have distinct functions. The physiological action of extracts was also discovered by Schafer and Oliver in 1895. It was soon ascertained that this activity was a property of the posterior or nervous lobe. The autacoid, or autacoids—for there is reason to believe that more than one are present—have not been isolated, but concentrated extracts, named pituitrin or infundibulin, are prepared from the pituitaries of oxen by the big chemical firms. The preparation is administered by hypodermic or intravenous injection, and is a powerful stimulant of the plain muscle of the viscera, and especially of uterine muscle. It is also an excellent stimulant of the circulation, having an action in this respect which is less powerful but more lasting than that of adrenalin. Pituitrin also has the effect, when injected into a lactating animal, of increasing the flow of milk from the mammary gland. The drug has been tried on milch cows, but fails to increase the amount of milk, and is not likely to be adopted by the dairyman as a commercial venture.

The significance of pituitrin in the animal economy is not understood, for the posterior lobe producing it, like the medulla of the suprarenal, does not appear to be essential to life. The anterior lobe, on the other hand, provides some material which is absolutely necessary. Hypertrophy, or increased activity of the anterior lobe, gives rise to giantism if occurring early in life, and to a peculiar overgrowth of the skull, hands, and feet, if it occurs in the adult. The latter condition was first recognised by Pierre Marie in 1886, and was named by him "acromegaly" (*ἄκρον*, a point, extremity, *μέγας*, large).

Diminished activity of the anterior lobe results in extreme adiposity, cessation of growth if occurring in early life, and atrophy of the gonads, with the appearance of many feminine traits in the body if the individual afflicted is a male. There is some diversity in the actual phenomena met with clinically, but these are perhaps the most characteristic. Somnolence is sometimes an accompaniment, and the fat boy in *Pickwick* is often referred to as an example of pituitary deficiency. The symptoms can be produced experimentally in animals.

There is no doubt that the anterior lobe produces an autacoid which has a general influence upon metabolism and affects the growth of bones and the development of the gonads. The nature of the autacoid is unknown. Extracts have been used in cases of deficient secretion with beneficial results in some but not in all. A material was prepared by Brailsford Robertson in California in 1916 which was alleged to be the growth-controlling principle, and was named by him "tethelin" (*τεθηλώς*, growing). Wonderful results were reported from its

employment, but these have not been substantiated by other workers. Remarkable achievements in egg-laying have also been alleged when extracts of the anterior lobe are administered to laying hens. These happenings were widely known in America, and it is significant that no commercial use has been made of them.

Within the last year attention has been directed to the discovery of an active principle in the pancreas. In 1889 V. Mering and Minkowsky ascertained that removal of the pancreas in dogs was followed by a rapid wasting of the body and the presence of large amounts of sugar in the urine, the condition being practically identical with the disease "diabetes mellitus" of the human subject. Further experimental work and pathological observations led to the conclusion that the islets of Langerhans in the pancreas are the structures responsible for an internal secretion which regulates the metabolism of sugar in the body. The islets are small and diffusely scattered through the pancreas, but in some bony fishes, as was pointed out by Dr. Rennie of Aberdeen, they are aggregated together as a distinct portion of the pancreas. Many attempts have been made to isolate the hormone and to demonstrate its presence. Some of these attempts, in the light of our present knowledge, were all but successful. The hormone was confidently predicted and was provisionally named "insulin" by Sir E. Sharpey Schafer some years ago.

It was left, however, to Dr. Banting, with the aid of Dr. Best, working in the physiological department of Toronto University under the direction of Professor Macleod, a graduate of Aberdeen University, to make the discovery in 1923. Dr. Banting graduated in Toronto in 1916, and after serving in the army and winning the M.C., returned to Toronto and made a resolute effort to throw light upon the question. His first attempts were influenced by the failures of others, and it was thought that the hormone might have escaped detection by being destroyed by the ferments which are formed by the glandular cells. Dr. Banting, therefore, took advantage of the fact that previous ligature of the pancreatic duct causes atrophy of the glandular cells but not of the islets. Ligature of the pancreatic duct was done in dogs, and after a sufficient time had elapsed the animals were killed, extracts of the remains of the pancreas rapidly made, and injected into dogs from which the whole pancreas had been removed. The extracts were found to be effective in alleviating some of the symptoms and in temporarily reducing the glycosuria. It was then ascertained that the hormone could be obtained from the whole pancreas of normal animals, and a method of extracting it in a sufficiently pure form was elaborated by Dr. Collip, one of their co-workers

in Toronto. The preparation proved to be effective in restoring human beings from diabetic coma, a condition which had hitherto been invariably fatal. The great importance of the discovery was at once apparent. News of it spread far and wide, and generous provisions were made to enable mankind to reap the full advantage. The Medical Research Council were invited to control the production and distribution of insulin in Great Britain, and have done so with a rapidity and efficiency which could not have been attained by any other means.

Insulin is now prepared by several large firms and is standardised by physiological methods. Improvements in its manufacture will take place, and in course of time it is hoped that the active principle will be isolated and prepared synthetically. At present it is extracted from the pancreas of animals, but it may also be prepared from the pancreas of those bony fishes in which the islets of Langerhans are isolated, and in which it is found to be more concentrated. The credit of the discovery belongs to Dr. Banting and Dr. Best, but the full recognition of its importance and the efficiency with which further knowledge has been attained are largely due to the genius of Professor Macleod and the enthusiasm of the large team of workers associated with him in Toronto University. The Nobel prize in Medicine was awarded recently to Dr. Banting and Professor Macleod, and has been shared by them with Dr. Best.

Insulin is not, strictly speaking, a cure for diabetes, but in some way it enables the body to make use of sugar. It has to be continually used in severe cases of diabetes and is given by hypodermic injection; but so long as it is given in proper amounts and at the right intervals, the disease does not show itself, and there is the hope that any islets present in the pancreas may recover and gradually become functional. If insulin could be administered by mouth it would occupy the same position in endocrine therapy as thyroid extract in myxœdema.

The importance of the endocrine glands in the physiology of the body is established beyond question, and further impetus to their study will be given by the latest discovery. The conception of autacoids, or active principles having a drug-like action, is a valuable one. The body contains all the material which is essential to its normal working, and when all these hormones are isolated, and their action understood, the armamentarium of the physician will be enormously strengthened.

THE BIOLOGICAL SIGNIFICANCE OF PATHOLOGICAL CHANGES

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It is interesting to define the attitude of general biology towards pathology. I believe that most biologists whose opinion might be asked concerning this discipline would answer that it treats of the abnormal, sooner or later leads to death, and therefore has no bearing upon the general problems of evolution, development, and growth. They might add that they have done very well hitherto without any knowledge of pathology whatever, that those among them who possess the nodding acquaintance with it conferred by a medical degree do not appear to be anxious to claim it in their writings, and instance Darwin, Balfour, Weismann, Driesch, and many other great men in support of their statements.

But these same biologists would unhesitatingly give well-merited countenance and praise to experiment, and point to the brilliant results achieved by experimental embryologists, as well as the high and well-founded hopes of even greater success in the future. But it may well be asked: Are not experimental embryologists essentially pathologists, in their methods at least, who investigate the reactions to an abnormal or pathological environment of their own creation? For what more is pathology than the study of the natural reactions to the abnormal environment of nature's experiments?

Our trouble is this: experimental embryologists are scientific in their methods, since they unite imagination and deduction with the observation of facts. We pathologists are but too often content with having described a fact or two. We rarely soar to a higher level than simple classification which, useful though it be, is not the highest form of science.

There are, of course, many noteworthy exceptions. Pathologists who have investigated changes of function have attained great things, and their labours have received full recognition from physiologists. But morbid anatomists rest content with descriptions of obscure lesions and rare forms of common ones and bacteriologists spend their time in separating strains of

bacteria which, although of the utmost importance from a utilitarian point of view, add nothing to general knowledge. Their writings display as a rule an entire lack of appreciation of the light they sometimes throw upon questions of general biological interest.

Although it must be admitted that our ideas are but too often bounded by systematics and epistemology, we have, unwittingly perhaps, described facts which are, or may be, of considerable general biological importance. At all events, the attitude adopted towards us by a good many biologists has not been a happy one. We have been told—by Bonnet in Germany and by Minot in America, not to mention instances nearer home—and are still being told that our facts and conclusions must conform in every respect with the laws of general biology as commonly accepted. Should this not be so, our conclusions must be wrong and our facts, if they be facts at all, grossly misinterpreted from want of general biological knowledge. We are advised to improve our minds by the study of embryology and kindred sciences. This advice is sound and I, for one, have long since adopted it to the best of my ability. But it is not, I trust, otiose or impertinent to ask if perchance the cap does not occasionally fit the head for which it is not intended. May it not be true that the general biologist, who studies healthy or normal organisms, has himself acquired a one-sided knowledge of the facts of development and growth, since he only sees the reactions to one kind of environment? This, the "natural" or physiological environment, is, luckily for us, present in the overwhelming majority of cases. But it has limits, though they be wide. What happens when these limits are transgressed? May not the organism be compelled by the unusual or pathological, but not "unnatural," environment to react by producing an unusual and pathological, but not "unnatural," modification of structure as well as of function?

I believe that I am right when I say that it is the orthodox view held by biologists that "in the adult vertebrate the capacity for regeneration is in most cases so narrowly limited that the cells of one tissue are under any known conditions incapable of giving rise to other tissues." I have here quoted Child, to whose brilliant researches we owe much of our knowledge of just these processes in lower animals. Weismann expresses the same thought in these words: "If we take as our basis the law, which holds good at any rate as regards vertebrates, that in regeneration each specific tissue can only reproduce its own specific cells . . ."

Regeneration is the peculiar province of the pathologist. It is therefore incumbent upon him to show, if he can, that

both Weismann and Child are wrong, and that instances of "heterotopic regeneration," as I propose to name it, are of frequent occurrence even in adult individuals of the species *Homo sapiens*. I have recently met with two instances of it in diseased organs, which seem to me particularly suggestive. I examined a consecutive series of thirty-six inflamed gall-bladders removed by operation from elderly individuals mostly, who had suffered from cholecystitis and cholelithiasis. In twenty-four of these the epithelium lining the fundus had produced typical mucous glands, which are not present in healthy gall-bladders, except possibly in their neck. Seventeen of these twenty-four, *i.e.* 70 per cent., contained in addition secreting glands indistinguishable in their structure and staining reactions from those of the gastric type, *i.e.* pyloric glands and Brunner's glands of the duodenum. The gastric glands were shown to be further stages of development of the mucous glands.[1] The percentage of cases in which they were present appeared to me to be much too high to be accounted for by an anomaly or abnormal predisposition of the gall-bladders in question, and I concluded that the epithelium of every gall-bladder possesses the potency and, I may add, retains it during life to undergo heterotopic differentiation or regeneration in the direction of gastric epithelium under the influence of the abnormal environment set up by a chronic inflammatory state. My second observation refers to the presence of gastric glands in the sinuses of a case of tuberculosis of the vermiform appendix and in a tuberculous ulcer of the colon. I concluded that the epithelium of the large intestine shares the potency to undergo differentiation into gastric epithelium with that of the stomach and duodenum. These heterotopic differentiations are instances of re-differentiation or regeneration, since they are always preceded by a considerable amount of proliferation and de-differentiation of the inflamed epithelium. This bare statement of the facts must suffice.

Miss Fell has quite recently published a complete histological description of analogous pathological processes in the gonads of the domestic fowl, whereby the mother of many chickens came to functionate as a fertile cock. She has established the fact that the sex-change is the result of proliferation or regeneration of the peritoneal epithelium of the ovaries, which was preceded by atrophy or disease of these organs in every case. In those in which differentiation had set in it was always in the "heterotopic" direction, the one natural in the male sex, and culminated with the production of spermatogonia and mature spermatozoa in several instances.

[1] I cannot here give even a sketch of their histology. An account of it will be found in the *J. of Path. and Bact.*, vol. xxvi, p. 399, 1923.

Biologists are fully aware of the fact that in the lower animals the prospective potencies of cells are wider than their prospective values. Driesch has said: "There are more morphogenetic possibilities in each part than the observation of the normal development can reveal. If at each point of the germ something else *can* be formed than actually is formed, why then does there happen in each case just what happens and nothing else? In these words, indeed, we may state the chief problem of our science." But Driesch insisted upon the sharp separation of *secondary restitution* whereby "a disturbance of organisation is rectified by a process foreign to the realms of normality" from *primary regulations*, "that lie at the root of true embryology." It is not clear why he should have been so emphatic in drawing this distinction, unless it be that in it he sought for and found a proof of the action of an "entelechy." But, then, can it not be the result of differences in the environment?

Had Prof. Child substituted the word "normal" for "known," I should have agreed with him. Atypical or heterotopic forms of regeneration are unknown in vertebrates in the conditions studied by biologists, but have long been known in pathology. How otherwise is the presence of gastric glands in inflamed gall-bladders and ulcers of the large intestine to be accounted for, and what other explanation covers the facts recorded by Miss Fell? These heterotopic tissues are *always* associated with pathological lesions, and have *never* been seen in normal organs. In 70 per cent. of gall-bladders with pathological mucous glands differentiation of the cells of these glands had proceeded in the direction of gastric epithelium. This percentage is much too high to suggest a primary abnormality of the gall-bladders or an unnatural predisposition of their epithelium in this direction. It indicates as clearly as possible a natural disposition or potency which remains in abeyance during health, but is readily evoked by a pathological environment. Not only are the prospective potencies of the cells of the embryo greater than their prospective fate, but they are not always lost in differentiation and development, since they occasionally become evident many years later in response to a new environment set up by a pathological lesion.

These are the most striking instances of heterotopic regeneration I know of in vertebrates. Changes of a similar kind are common and familiar to pathologists. Squamous epithelium has been recorded in pathological states in nearly every internal mucous membrane not lined by this form of epithelium; *e.g.* the accessory air-sinuses and salivary glands, the mammary glands, the ependyma of the central nervous system, the urinary passages, the uterus and the epididymis, the bronchi and lungs,

the thyroid, the gall-bladder, and the pancreas. I have omitted from this list cases that are, or might be, due to substitution or overgrowth by neighbouring epithelium and to anomalies of development. Those that remain are genuine instances of heterotopic regeneration.

Heterotopic regeneration is most often seen and easily investigated in the most ubiquitous of all tissues, namely, areolar connective tissue. Bone has been found in and around areas of necrosis and calcification in nearly every organ and cubic inch of the body. It is always produced by the cells of the supporting areolar tissue, that have proliferated to form a barrier of granulation tissue around the calcified "foreign body." The connective-tissue corpuscles are stimulated by the local excess of calcium salts that are dissolved by their own activity to react in a manner which is physiological in endochondral ossification of bones. Instead of remaining fibroblasts, they are forced by the new or pathological environment to assume the structure and functions of osteoblasts and bone corpuscles.

All these pathological reactions indicate that the power of cells to undergo regeneration into a different specific type is a much more widely distributed phenomenon in the animal kingdom than is generally supposed. The higher the animal, the less does it become, but many instances of its action are to be observed even in man. I have recorded no instances above that exceed the potencies of the primary germinal layers to which the tissues belong.

We may well ask with Driesch: "If at each point of the germ something else can be formed than actually is formed, why then does there happen in each case just what happens and nothing else?" Two alternative explanations appear to be possible. In one of these the future structure that cells are finally to assume is predetermined in a morphological sense, since it is present in some part of the cell, usually supposed to be the nucleus, throughout the whole of its developmental history. This is the theory of *preformation*, which postulates, in its logical form, that development is a gradual unfolding or becoming manifest of structural characters that were always present in some form or other. Structures a, b, c, etc., are present in the nucleus of the zygote; during the course of development they are shifted to their permanent position in parts A, B, C, etc., of the body. If development is normal, they will ultimately reach their normal positions; should it be abnormal, they are shifted to abnormal situations or possibly damaged or killed, when other stronger or healthier characters may take their place. The body as a whole does not increase in complexity of composition, it increases merely in bulk by

division, reproduction, and growth of the structures contained in the nucleus of the fertilised ovum. The individual cells become progressively more simple in composition as development proceeds. This theory has been formulated to include the phenomena of regeneration, as we shall see directly, and is thus covered by Driesch's question.

The other possible explanation postulates that the final structural characters are not present in the fertilised ovum. They gradually emerge or are evolved from simpler structures as something new. Thus, development and differentiation depend upon a gradual increase in complexity of the body as a whole. The final specific structure of a cell is not predetermined from the beginning; it is the visible expression of one of many alternate possibilities or "potencies." This is the theory of *epigenesis*.

The instances of heterotopic regeneration described in this paper appear to me to afford an excellent means of testing these theories. Should that of preformation hold, then how is it possible for gastric glands to be produced late in life in the gall-bladder, seeing that, in the terms of the theory, the epithelial cells of this organ contain nothing but gall-bladder characters? The phenomena of regeneration have been a stumbling-block for this theory from the very beginning, and compelled Weismann, who worked it out fully, to postulate the presence of accessory nuclear specific structures or "determinants," as he called them, which are present in a dormant state in all adult cells in every part of the body in which they are required by the theory, are of just such a nature and degree of complexity as is wanted in every given case, and thus bring about the most complicated regenerations we know of, including even that of the original "germ-plasm" itself. But this is nothing more than a mere restatement of the fact that cells in normal conditions retain their physiological structure, whereas they exchange it for others in abnormal ones, and appears to me to reduce the whole theory of preformation to an absurdity. I see, however, that Prof. Hobson discusses Weismann's theory at some length in his Gifford Lectures. His biological section is of peculiar interest and value, since it reflects the views of a well-informed layman in matters biological, who is a great authority on other branches of science. He approves of Weismann's theory as a "conceptual scheme," which, as he himself points out, it is not, and never was intended to be, but does not compare it with other alternative theories, thereby suggesting that these are not worthy of philosophical consideration. I cannot, unfortunately for me, share Prof. Hobson's admiration of purely conceptual schemes. Biology is very far removed indeed from an abstract or even an exact science, and it is not

only useless, but positively harmful, to try to explain it by conceptual theories in its present state. Weismann's theory or, to be exact, that of preformation whose offspring it is, does not add to the facts of regeneration to my mind, but merely paraphrases them. I am not sure if this is not the object of, or at least the end commonly attained by, conceptual schemes, except in pure mathematics, of which, unfortunately, I know nothing.

Since preformation does not meet the requirements of pathologists, we will see what epigenesis can do for us. We have seen that the structures described above are instances of regeneration. We must now emphasise the fact that they are all built up of typical physiological cells and tissues, whose structure is identical with that of the same types when studied in their natural surroundings. Their functions even appear to be typical, if we can judge them by their microchemical reactions. The only anomaly they present is one of position; they are found where they ought not to be.

It is a truism to state that a regeneration demands an antecedent lesion. Our heterotopic tissues are therefore only found in diseased parts of the body, although they themselves show no signs of disease. They simply din the fact into our ears, a fact which has not received the attention it deserves, even from pathologists, that all pathological reactions, structural and functional, are the only possible reactions to the abnormal or pathological environment. They are perfectly natural and physiological in the changed or abnormal conditions that happen to prevail. If we call the normal environment N, and the normal structure of the tissue n, then n is the resultant of N and of the original potencies of the cells of the tissue. If the environment changes to X, Y, or Z, the resultant structure will inevitably change to x, y, or z, provided, of course, the tissue be not killed.

We thus come to see that normality is not a clearly defined category as Driesch would have us believe. It is simply an accident, one of many possibilities. The correct mental attitude to adopt towards it is not the common one of taking it for granted, but to ask: Why is it that development and growth so generally go right, and why is it that they so comparatively rarely go wrong?

It is only by studying the effects produced when growth goes wrong that we can hope to get an idea of the varied potencies that are contained in the cells. It is here that the heterotopic regenerations seen in adult animals appear to me to be of inestimable value. Not only do they demonstrate the fact that structure is not predetermined, but that it is simply the physiological reaction to the given environment. We cannot

help seeing how readily it changes with the latter; it is the visible expression of one of many alternative possibilities. Our best chance of understanding large and extensive intra-uterine malformations and, through them, the mechanism of normal development, is through these comparatively simple and clear post-embryonic heterotopic regenerations.

I have laid stress above upon the fact that the cells of these anomalies are perfectly normal or physiological. Why, then, are gastric glands produced by the epithelium of the gall-bladder? Is it because it is able to run riot because of increase of blood-supply or possible inhibition of nervous control, etc., due to the "upset" of the inflamed gall-bladder? I suspect that the reasons are far simpler and more physiological. These gall-bladders always contain infected, purulent, and semi-decomposed material. May it not be that "gastric" secretion, comparable with that of the stomach, tends to neutralise the toxins liberated by the discharge and to inhibit bacterial action? Should such a secretion be demanded by the abnormal environment, I can see no reason why the tissues will not react by producing it if they can. Our heterotopic regenerations would thus be the natural morphological expression of a physiological reaction to a change of environment. Heterotopic bone-formation around calcified foreign bodies is capable of explanation upon similar lines, since the bone surrounds and replaces the highly insoluble dead substance by living calcified tissue, and thus neutralises its irritating action. It is certainly a suggestive fact that Maximow found in his experiments that the bone is eventually completely reabsorbed after the calcified necrotic tissue has been dissolved and replaced by it.

Not only are the facts of heterotopic regeneration explained by the theory of epigenesis, but they appear to me to give strong additional support to it. The cells of fully differentiated tissues undergo de-differentiation succeeded by regeneration into something new. But a fresh fact emerges here: Morphogenesis is not always the end of all things. The original prospective potencies of the cells are not always lost for ever when their ultimate values or characteristic structure have been established at the end of development. The normal structure of a finished organ or tissue is nothing more than a *Funktion des Ortes*, a reaction to the normal environment, since it changes even late in life with a pathological alteration of the latter.

And this again suggests that Child is right when he maintains that the least "stable" and therefore least "formed" and visible parts of the cell are most completely alive, in the sense of being the most active agents in metabolism, and that a far-seeing layman in biology, Mr. Morley Roberts, has come

very near to the truth when he asserts that a stable structure like the nucleus is only a storehouse of the tools required by the "living" protoplasm of the cell for the performance of its metabolic activities.

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TAR, SMOKE, AND COAL GAS AS FACTORS INIMICAL TO VEGETATION

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Introduction

The writer has been led to the study of the varied problems grouped under the above title, as the result of following an independent line of investigation in plant physiology which has come into contact with these questions in a very surprising manner that will be indicated below. These problems seem to be very little discussed in British journals, either scientific or technical, and therefore some account of the more interesting results of work, carried out mainly in other countries, seems to be justified. At a later stage the question will be asked whether the problem is of practical significance. It is certainly of scientific interest to the chemist, to the gas engineer, and to the botanist ; the conclusion reached later is that it may well prove to have practical interest for the horticulturist and for the public authority who is trying to provide avenues of shade trees in populous thoroughfares.

Tar

The reactions between tar and the surrounding vegetation will obviously vary with the nature of the tar and its method of use. It is an everyday experience that the vapours from a cauldron of heated, bubbling tar may produce local damage to vegetation, volatile phenols being probably largely responsible (Haselhoff and Lindau [10], and Gatin [6]). Similarly the damage done to potatoes growing near a factory where carbolineum is being used to impregnate wood is easily understood (Sorauer [26]). But the question whether the vapour or dust from a tarred or asphalted surface may do harm to vegetation is more debatable and of greater practical interest. The subject has been very well reviewed by Gatin [6] recently, this author having special opportunities to study the problem in view of his duties as secretary of the commission appointed by the Prefect of the province of the Seine in 1911, which was

charged with the duties of investigating the damages alleged to be arising from this cause along the beautiful avenues traversing the great public parks of Paris.

Gatin concludes that in the avenues where traffic is heavy and the bituminous dust lies thickly upon the neighbouring plants, then under hot sunny conditions damage to the foliage of the more sensitive species of plants is certainly produced and may reach very serious proportions. He points to the absence of similar reports of damage from England, to judge from the results of an inquiry instituted by the *Surveyor* [35], and attributes this to the facts (1) that a more refined tar, the product of carbonisation at higher temperatures, is used upon road surfaces in England; and (2) that we enjoy a lesser share of warm and sunny weather. An interesting question arises as to how the dust from the tar produces its deleterious effect upon vegetation. As the effect is sometimes prominent upon the opposite side of the leaf to that on which the dust has settled, Gatin evidently inclines to the view held by Mirande [14] before him that vapours from the tar may be the harmful agent. This subject may be worthy of further investigation in view of the toxic action of minute traces of constituents of coal gas, reported in a later section.

Finally brief reference must be made to another way in which a tarred surface may affect vegetation. Such a surface provides a continuous and relatively impregnable surface crust above the soil. In cities the result will be that if leakage of coal gas or other noxious vapours should occur, these gases may be carried long distances through the soil if liberated beneath a tarred surface. This point is raised in the discussion in the *Surveyor* [35]. Stone [28] also draws attention to the long distance (2,000 feet and more) that gas may penetrate through gravelly soils, and Wehmer [34], in reporting a case of gas poisoning referred to below, points out the effect of the hard packed street surface in keeping the gas circulating around the roots of the plants.

Illuminating Gas

The effects of various forms of illuminating gas upon vegetation have been studied considerably, especially in Germany and America, where the problem has forced itself upon the attention of the investigators as the results of practical cases where unhealthy vegetation seems attributable to this cause. Unfortunately the nature of the illuminant is not always stated. It may be carburetted water gas, ordinary coal gas, or oil gas. Whatever the nature of the illuminant, if sufficiently concentrated its effect upon the plant is naturally

unhealthy. Such toxic action may arise from a variety of causes; there are plenty of substances present in any illuminating gas which in adequate concentration will be toxic. Very brief consideration will first be given to observations dealing with cases of plant poisoning of this kind, as obviously the results might be anticipated and have little practical importance. In a subsequent section the very different circumstances connected with the poisoning of plants by small traces of gas in the atmosphere will be more thoroughly considered.

The first observations are mainly German (Girardin [7], etc.), and so also the first experimental investigations. Kny [12] killed maple, elm, and euonymus trees by big doses of gas, carried to the trees by pipes laid through the soil with burners allowing it to escape in the close vicinity of the tree.

Späth and Meyer [27] passed about 1 cubic metre of gas daily through some 18 cubic metres of soil. They found lime trees relatively resistant, but *Platanus*, Silver Poplar, and *Ailanthus*, amongst others, were killed by this treatment. Böhm [1] grew slips of willow in water through which the gas was passed; he found that the roots and the cuttings remained very short and soon died. Wehmer [34] reports the death of thirteen elm trees in a street, killed by a leakage from a gas pipe, the injuries produced varying in this case with the distance from the pipe. Shonnard [23, 24] found that, after exposure to a flow of gas of 1·07 cubic feet per hour for eight days, a lemon tree used for the experiments was in distress with sap exuding from the stem. In all these cases, where large quantities of gas are used, there are conflicting views as to the active toxic agent, but this is to be expected. The mere displacement of the air around the tissues of the plant may well be responsible, producing asphyxiation effects, as is suggested by Kosaroff [13] in his experiments with hydrogen and with carbon dioxide. Wehmer [29–34], in a recent series of papers, appears at times to attribute the toxic effect action to benzol or its homologues, to sulphur compound, and to hydrocyanic acid gas. Very probably different factors are involved in the different cases briefly reviewed above, but no practical end seems to be served by a closer analysis. A very different case is presented by the phenomena now to be reviewed, where gas poisoning results from the presence of minute traces of illuminating gas in the atmosphere around the plant.

The Toxic Action of Traces of Illuminating Gas

These observations have recently been briefly reviewed by the writer elsewhere [20] and are now only very shortly

recapitulated. The earliest observations date from the extremely interesting German work (Neljubow [15 and 16], Singer [25], Richter [21], etc.) by which the toxic effect of the impurities of laboratory air were slowly traced to their source. These experiments firmly established (1) the significance of small leakages of gas in reference to the toxic effect produced; (2) the fact that the unsaturated hydrocarbons in the gas were responsible for the poisonous action; (3) that certain living plant organs were especially sensitive. These German workers noticed the striking difference in the growth of etiolated plants, placed for convenience in darkness in the laboratory, as compared with similar plants growing in the purer air of a greenhouse, or cut off from the laboratory air by growth in the laboratory below a bell-jar, inverted over water. An etiolated plant such as a pea or broad bean is normally a curious, spindly-like structure, but grown in gas-contaminated air it is even more abnormal, as is illustrated in the accompanying photograph. Curved instead of erect, the stem is swollen and its growth in length has practically ceased.

Since these early German observations, the thorough work of Crocker and his school [4, 5, 8, 9, 11], reported more in detail elsewhere (Priestley [20]), has shown the wider significance of this poisonous action produced by traces of gas. The effect is more pronounced on growing plants, dormant plant tissue being little, if at all, affected. Carnation flower buds are very sensitive, very young and nearly-open flower buds especially so, buds at an intermediate stage less sensitive.

Green foliage shoots are relatively unaffected, but the leaves show epinastic curvature, very pronounced in some plants, as *Ricinus* (Harvey [8]), Tomato (Doubt [5]), etc.

Etiolated shoots are extraordinarily sensitive, and much work has been done upon them by Crocker and Knight [4] directed to elucidate the toxic factors at work. Roots are also very sensitive. Crocker and his school confirm completely Neljubow's conclusion that the unsaturated hydrocarbons present in the gas are the toxic agents responsible for poisoning by the gas in these very dilute concentrations. They show definitely that sensitive structures, like etiolated pea seedlings or carnation flower buds, will react to concentrations of pure ethylene in air, equivalent to one part of ethylene in one million, and to concentrations of coal gas in air introducing equivalent quantities of ethylene.

The different effect of various illuminating gases can now be understood. Oil gas, frequently employed in earlier days in Germany, contains very high concentration of unsaturated hydrocarbons; the water gas so much employed in the United

Bean Seedlings Grown in Dark.

Seedlings on right normal, those on left grown in an atmosphere slightly contaminated with coal gas. Note horizontal direction of growth, and stunted and swollen stems of seedlings in gas-contaminated atmosphere.

States is charged with considerable amounts of these substances to give it direct illuminating power; whilst in this country the coal gas is usually relatively poorly supplied with these substances. Crocker's experiments were largely carried out with a Chicago supply containing about 4 per cent. of ethylene; the English gas supply used in small traces to produce the effect shown in the photograph contains less than 2 per cent.

Smoke

Knight and Crocker's experiments [11] also show that the smoke given off from burning cigarettes or even from smouldering pure cellulose can prove toxic to etiolated seedlings if drawn into a confined space of air in which the plants are growing. But the results here can also be traced to the presence of these same unsaturated hydrocarbons, and experiments upon the flue gases from industrial and domestic chimneys show that normally the vitiated air from these sources contains too little ethylene or other unsaturated hydrocarbon to be a serious source of danger to plants. Smoke-laden air can, of course, be inimical to vegetation in countless other ways (Cohen [2], Crowther and Ruston [3 and 22]), but the special effects we have been considering in connection with illuminating gas do not appear likely to be produced by smoke-contaminated air.

Although the experimental work done in Germany and America leaves no doubt that these unsaturated hydrocarbons are responsible for the damage done by traces of illuminating gas, and although very full details of the structural changes produced were now available, no explanation was forthcoming as yet as to the causes of the toxic effects. Working in quite a different field, the writer has obtained some results which seem to throw considerable light on this apparently obscure problem.

The Reasons for the Toxicity of Unsaturated Hydrocarbons

Contact was established with the writer's work through the observations that etiolated stems and normal roots are both very sensitive to traces of gas, whilst normal shoots grown in the light are not. The writer had recently learnt to associate etiolated stems with roots in reference to one structural peculiarity that is missing in the normal stem grown in the light. The original tissues of root and shoot are all formed by the activities of a little mass of tissue at the apex of the growing axis. Behind this apical meristem the tissues absorb water and swell in size, at the same time alterations proceeding in their cell membranes and contents. In the root a character-

istic and constant feature of the tissue changes just behind the apex is the development of a cylinder of cells with especial structural peculiarities, known as the endodermis. This layer has recently formed the subject of close study by the writer (Priestley and North [18], Priestley [17]), and he has learnt to appreciate its considerable physiological significance in restraining free diffusion of substances between the tissue on either side of it. In the normal stem, growing in the light, no such functional endodermis with specially modified walls is present, and sap diffuses freely from the vascular system in the central region of the plant to its surface. The superficial tissues of such a stem are therefore growing freely, but in the pea or broad bean, if such a plant is placed within the dark, the abnormally elongated etiolated shoot is found to develop an endodermis just behind the growing apex (Priestley and Ewing [19]). As a consequence, the sap from the vascular strands is kept within this endodermal cylinder and less growth takes place of the superficial tissue of the stem, greater growth in length taking place instead.

When these etiolated shoots are placed in the presence of traces of coal gas, the shoots cease to grow in length but develop in girth instead. This result immediately suggested that under the action of the unsaturated hydrocarbons the endodermis ceased to form as a functional layer, and examination confirmed this supposition. Similarly in roots the "gassed" roots swell up just behind the tip, and in the swollen region breaks can be found in the normally continuous endodermis.

There are other results of the toxic action of illuminating gas yet to be explained, and the subject is still under investigation. The writer is not without hopes that they may find their solution, in part, in the similar action of the unsaturated hydrocarbon in inhibiting cork-formation, in part from the effect of these substances in altering the distribution of unsaturated fatty acids in the walls of the meristem. In every case the direct effect of the unsaturated hydrocarbon seems to be that the presence of its unsaturated linkage "crowds out" an unsaturated acid from its normal position in the plant membrane.

These unsaturated fatty acids appear to play an important part in the construction of the endodermis, in the deposition of the suberin lamella of the cork cell, and in the gradual differentiation of many plant meristems.

Gas Poisoning and Horticultural Practice

It will now be clear that concentration of gas may occasionally be present in the air around a plant which will exceed

he very low concentration necessary to produce the toxic effects briefly referred to in the last section. In greenhouse practice, where the air space is limited and ventilation restricted, leaky gas pipes may provide dangerous quantities of unsaturated hydrocarbons in the air. In urban conditions, with wide surface areas covered by pavement and asphalt, coal gas leaking into the soil may accumulate until toxic effects are produced upon the roots of trees. American experience seems to suggest that such action may require consideration where trees are grown for shade in public thoroughfares (Doubt), and the trees experience shows most suitable for the purpose may be relatively resistant to the gas.

But many other factors are always at work, in urban conditions, to oppose the healthy growth of the plant, and the practical problem seems to be to ascertain, in any case where injury is experienced, whether this can be indubitably traced to the poisonous action of gas. The concentrations of gas shown to be effective are far below the concentrations which can be detected by smell. Doubt [5] in some American experiments states that frequent trials failed to detect the odour of concentrations lower than 1 part in 400 of air ; on the other hand, with this same gas supply (containing 4 per cent. of ethylene) concentrations of 1 per 1,000 were very harmful to many plants. The gas escape may therefore not be detected save by the effect produced upon the plant, and as, when the original damage has been done, the plants are very susceptible to fungus disease and when examined show clear signs of the effects produced by such secondary causes, damages originally due to the continuous action of traces of coal gas may be ascribed on examination entirely to the secondary factors subsequently at work. Remedial measures directed to remove these secondary factors may leave the root of the trouble untouched. Doubt's suggestion to florists is that where harmful contamination by coal gas is suspected, vigorous plants of tomato or the castor-oil bean or other suitable plant should be placed in various places in the house and left there for twenty-four to forty-eight hours. Within this period at ordinary temperature the plants will respond to the presence of traces of the gas, the most characteristic response being the marked epinastic movements, but with concentrations still below detection by odour, the plants named will drop their lower leaves abnormally early.

To these suggestions of Doubt the following additions can be made, in the light of the writer's experiments. If the effects of gas poisoning are examined at an early stage, the structural effects produced by the contamination should be quite characteristic in nature. In the roots, swellings associated

with gaps in the endodermal cylinder ; in stems, lesions in the cork accompanied by cell proliferation due to the excessive distension of thin lamellæ lacking their normal deposit of suberin ; loose proliferated tissue at the lenticel, and, generally throughout the plant, fatty deposits, normally present in definite internal structural features, missing or reduced in extent, and the fatty substances accumulated in the neighbourhood of the surface instead. Such a plant is more liable to fail to heal any external injury or to develop lesions as the result of the strains upon the covering tissues produced by the expansion of the growing tissues within; it is therefore an easy prey to fungus diseases entering the host plant through a wound.

In cases where physiological experiment is not excluded by practical requirements, the atmosphere might well be tested by growing etiolated pea shoots or young pea or bean roots and keeping them under examination. Forty-eight hours will normally provide unmistakable evidence of the presence of toxic concentration of the gas, and the reactions of the etiolated shoots in particular are exceedingly characteristic and exceedingly unlikely in greenhouse practice to arise from any other cause.

Crocker and Knight [4] in their paper upon the carnation have an interesting note on the influence upon the domestic atmosphere of the spread of electric lighting. The authors state that they are personally acquainted with homes where it had been impossible to keep carnation blooms fresh in the room for periods of more than a day. With the replacement of gas by electric light this difficulty has entirely disappeared and the blooms can be kept fresh for many days. In England, with a smaller supply of unsaturated hydrocarbon in the illuminant, this is not, perhaps, such a common experience, but the incident is worth recording, as it may supply some reader with a possible explanation of his difficulties in maintaining the blooms required for table decoration at a suitable standard of vigour.

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POPULAR SCIENCE

SCHOOL AND UNIVERSITY SCIENCE

Presidential Address to the Science Masters' Association, January 3, 1924

By PROFESSOR ARTHUR SMITHELLS, C.M.G., F.R.S.

During the whole of my career as a University professor I have been deeply, and perhaps too meddlesomely, interested in the school teaching of science. I appreciate more than I can say the honour of being called to occupy this chair for the year that has carried me to the ranks of the *emeriti* ; I feel it to be a great honour, bringing also the comfort of an act of absolution. My own teaching task has, of course, compelled an interest in yours, but I have been far more concerned to see how you fared with a much more important duty than dealing with those of your pupils (in the case of state-aided secondary schools, I understand, less than 3 per cent.) who proceed to Universities. You are the great missionary force of science ; it is on you that largely depend the establishment and dissemination of natural science in the general education of the multitude. The task has been and still is one of immense difficulty. It has been a new campaign, much belated, and fraught with quite special obstacles. It was not merely to add an extra to education ; it was an effort to make a profound change in an old-time curriculum, so as to bring it into some sort of correspondence with the state of a changed world. For, be it for good or ill, the tumultuous flood of the new knowledge—that of natural science—with its attendant inventions, has transformed the world. I am not thinking by any means solely of the mastery of things material, in fact I am thinking least of it. I think of the far more serious implications of science with thought and, through that, with matters lying at the foundations of civilization and human happiness.

It has been the task of yourselves and your predecessors to take a very difficult part in what has inevitably been an aggressive movement, an attempt that necessarily involved displacement—the displacement of something very reluctantly

abandoned. Mr. Galsworthy has lately complained that science with its inventions came before the world was fit for it. We shall all admit at least that it came quickly, and in some ways more quickly than has been comfortable even for those of us who are in the profession.

The campaign for the introduction of science teaching into schools was only just gathering force when I became a University teacher. It has proceeded steadily and has extended remarkably. It is, I suppose, generally considered that science teaching is now, broadly speaking, established as a normal part of school education, after a long period of waiting in the shades of the modern side. Science, we are continually being told, has gained the day and entered into her inheritance. Look, people say, at the schools, at the great public schools, listen to the great headmasters. Look at the activity of the State in several of its departments, and then the new Universities, the Technical Institutions, and perhaps above all the old Universities themselves! Well, we admit all that; it is obvious. But what I, for one, do not admit—and I believe I shall have many of you with me—is that, notwithstanding much outward and visible sign, there is anything like as much inward and spiritual grace as many people would have us believe. There is, I mean, much less real conviction and change of heart, and without that we cannot reckon that much has been gained. What is really recognized to-day as never before, is the might of science, rather than its right, or still less, if I may continue to rhyme, its light. A considerable part of the thinking world is above all afraid of it, and I admire Mr. Galsworthy for saying so plainly what is concealed beneath much of the lip-service and concession that we have lately received from others of the class called humanists, who, though rarely participators, are deeply interested spectators of science. He announces in an underlined dictum that "destructive science has gone ahead out of all proportion," and he speaks of "science more hopeful of perfecting poison-gas than of abating coal-smoke or cancer." Yet "scientists (chemists, inventors, engineers) have," he says, "for the first time the future of the human race within their grasp." I do not propose here to discuss Mr. Galsworthy's pronunciamento, but I do commend it to your attention, as an illustration of a view of science which you must take into most serious consideration. You will be struck, I am sure, by the degree to which the threatening aspect of science seems to predominate, in Mr. Galsworthy's view, over all its graces and its services.

There is another somewhat gloomy comment on the present position of science for which I will myself take responsibility. It is that a generation of science teaching has produced a very

disappointingly small dissemination of scientific knowledge of an available kind among our people. I take responsibility for that statement, for I doubt whether anyone could have had better opportunity of testing its truth. It is not long since I was in charge of a school of life-saving, where we dealt with three million men from all classes of society, and where a little real knowledge of elementary science might mean, and often did mean to them, the difference between life and death. The rudiments of such knowledge were not to be found in one man among a hundred. The remnants that existed were of no avail. I think we must reluctantly admit on both counts that our science, so far, has edified and instructed far less than was reasonably to be expected from a movement that has been carried out for so long and with such vigour.

I hasten to say, however, that I am not downhearted and am not inviting you to any kind of despondency. I should be sorry, too, not to do justice to the great help our cause has received from many outside science whose sympathy is whole-hearted. There is indeed much to encourage us, and perhaps sometimes we are impatient and not grateful enough for small mercies. If, for example, the Civil Service Commissioners have not yet done all we might wish, it is a great thing to have it agreed as a start that science is a subject every educated man should know a little about, even if he knows nothing of it out and out.

I believe that the present time affords an opportunity for commending science such as has not occurred since, in the early nineteenth century, Davy and Faraday were expounding the great new knowledge of that day to fascinated audiences in the Royal Institution. The revolutionary discoveries of the still young twentieth century are surely such as have never been known before. I do not allude to inventions with their bewildering conquests in power and speed, or to the healing art with its triumphs of rescue and immunity, still less to the destructive arts in their annihilating might. I allude to discoveries in the older vast astronomy of the heavens and in the new minute astronomy—the electronomy—of the atom. These are things about which every thinking man must wish to know something, and as Sir William Bragg is showing to-day, again in the Royal Institution, they are things which can be taught, in a measure, even to children. He is, of course, a master in the work itself and in its exposition. But we can all do something, and I feel confident that even a little knowledge of the wonders which the most recent science has revealed cannot fail to stir the emotions of any human being endowed with common capacity for thought and feeling. These discoveries exhibit the spirit of science in its fullest glory. They must

surely help men to see something of the inner temple in which she dwells, and keep them from the impiety of letting any suspicion fall upon Science herself because of the misdeeds of those who abuse her gifts.

For my own part, I can honestly say that nothing gives me greater hope for the future of our common task than my knowledge of this very Association. I have watched it from its birth with the most eager interest, and by your kindness have been permitted to attend a number of its annual meetings. During the past year I have seen something of its inner working. I think I know what you are doing, and I am certain that if you continue strictly on the path you have so far followed, you have it in your power to put science teaching in a very different posture and to put it right. You are associated in a way that, if not unique, is rarely to be found to-day among any class of workers, namely, for the single and direct purpose of helping one another to do better work. You have proceeded strongly and steadily, and I should like to pay my tribute to the body of science masters of the great public schools who founded this Association, who have stood by it and worked for it, who have now extended its ambit so widely, and who, it is to be hoped, themselves and their successors, will continue to abide by you and stand high in your councils.

I feel that, compared with what you can do for yourselves, it is very little that the most interested outsider can do. Yet I daresay you might fairly think that after my long experience, especially in inheriting some of your pupils and participating in the training of science teachers, I ought to be able to say something at least of a little interest from the University side. In making the attempt, I should like to start with the statement that of all the good things you have done, I reckon none so important or so good as your declaration that school science should primarily take the form of Science for All. I need not enlarge to you upon the ideas that underlie that declaration, and I should find it difficult to put them in brief form to the rest of the world. Besides, it has been admirably done by others, and notably so by our faithful friend Sir Richard Gregory, in his British Association address of 1922. Perhaps one might say, in a phrase, that you are determined that science shall be studied in more effective relation with life as it is and things as they are in the great everyday world, and not as it is dealt with by professionals in seminaries. Science, in a word, must be humanized. Your action was in part a protest against the lingering assumption that a science master stands outside the body of teachers engaged in what is called general education and that he is mainly charged with the duty of preparing a few boys for subsequent professional study.

I know very well that, though your declaration was made *nemine contradicente*, there are some—it may be many—among your number who do not in their hearts subscribe to it. And I know there are outside, among those who exert considerable power as critics and adjudicators of your work, some who are more than sceptical. I should like therefore to put my advocacy wholeheartedly, for what it is worth, on the side of Science for All.

Perhaps I may say first, speaking I know for many of my kind, as a University professor, that what we want to find in the pupils we receive from you is, above all, the real live self-acting interest in science of the amateur. We want boys who have the zest of the true naturalist—using the term in its full extension to the physical sciences—with the habit of doing what they do with thoroughness both of thought and craftsmanship. We want also, besides literacy and an available knowledge of languages, a reasonable breadth of elementary scientific knowledge. The rapid growth of science is making it ever more and more difficult for us to include the elements of some of the branches of science which would be of the utmost service. It was little botany and zoology that one could learn at the University in my day if desirous of specialising in the physical sciences. Yet that little, which has been of inestimable value to me, is now usually left untouched by the honours chemist. The lack of a little knowledge is a dangerous thing, if you are a specialist—a little knowledge of many subjects and not only those of science. I have discovered no relation between the mere quantity of chemical information a pupil brings from school, and his subsequent progress in that science. I am tempted almost to say that I do not even know the type of the ratio. But I do know what it means to have pupils come with even a little, well taught, and with an eager appetite for more.

I know very well the fears that attend the attempt to teach Science for All, and I think I know the difficulties. Something akin has indeed been tried before, for Huxley's physiography was, I believe, in intention much the same. It failed, as I think the present-day attempt with an omnivorous geography is likely to fail; it was spread too thin, it lacked the reality of experimental science, and it was not at any point handled in a way to inculcate the method of science. I do not, of course, speak of what Huxley intended, but of what I saw of the usual attempts to teach physiography.

No doubt the severest criticism of Science for All will come from a section of teachers of whom I have always stood in awe. We who so love science as to have taken the vow of poverty and become teachers, instead of perfecting poison-gas or invent-

ing high explosives, should never forget that we are exceptional people in our enjoyment of abstract knowledge. Among us the mathematically gifted stand supreme in that respect. They are, personally, I know, as amiable and sinful as the rest of us, but they are apt, from unconsciousness of their special gifts, to favour a somewhat ascetic view of the more easy-going sciences, and I believe they will be genuinely and justifiably suspicious of any threatened abatement in what there is of a desirable sternness in our present scientific discipline.

Of the supreme difficulties to which all good teaching is liable under the sway of the examination Frankenstein that we have created for ourselves, I will say little, for you know them only too well. Speaking as a University man, I would urge you not to relax your effort to see that, so long as there are examinational tests of school science, the teachers themselves shall have their proper share in devising and applying them. I think great harm has been done by Universities in this direction, by allowing this most important work to fall so largely into the hands of people who have no first-hand knowledge of school science teaching. Certainly University teachers would not for a moment tolerate corresponding conditions for themselves and their own pupils.

It is quite certain that, before you have developed what you really have in mind as Science for All, you will encounter great difficulties in the problem itself and within your own ranks. You will experience many failures, and you know, of course, that faults in a new way of doing things are more conspicuous and are visited much more seriously than far greater faults to which we have become habituated in the older ways.

I wish now to draw your attention to a matter which I think is of paramount importance in relation to your work. I think the greatest difficulty for a science master in entering upon his duties in school is to divest himself of what I may call the University or professional outlook on science. I doubt whether there is any period of life at which a man has forgotten so completely what it was to be a beginner as during the earliest years of manhood. His own assimilative powers have perhaps just reached their fullest development, and he is exulting in the conquest of the greatest heights of accumulated knowledge and in the exhilarating purity of the intellectual air. That would not matter a fraction of what it does matter if a child's natural method of learning were continued in kind throughout the University years. But that, I think, is very rarely the case.

I hope it will not weary you if I remind you of the conditions under which we work in most Universities. A University professor is necessarily a trainer of specialists, and is confronted by the ever increasingly difficult task of putting his pupils

into the way of using a vast body of knowledge for professional purposes. I do not ignore other necessary qualifications of a professor or other types of students, but Universities were in the beginning, and are now, characterized especially by being centres of professional training. It is this fact that determines the content and, in great degree, the method of their teaching. It leads to a series of convenient formalities of procedure of a highly artificial kind. In science it leads at once to a segregation of subjects and an unnatural delimitation of frontiers, leaving us with a set of separated members of the whole corpus of natural knowledge. Within each separated science we have next to contrive how, in the time at our disposal, to deal with the vast and ever-increasing accumulation of facts and principles. We seem almost compelled to drag our subject from its affiliations with history and the rest of human knowledge, from its association with the life and things of every day; to make an abstract, or, as we say, a "pure" science of it. We devise a sort of logic of condensed presentation, make categories, fix arbitrary limits of information, and so end with a kind of canonical version of physics, chemistry, botany, or whatever the science may be, an orthodoxy whose grip is aided by the iron hand of the examiner, and by the written word of syllabuses and textbooks. Thus we methodize our teaching with little heed of the fact that we are almost inverting the natural way of learning. For we really know well enough that in the natural way of learning about natural phenomena, we have our topics intertwined with more than one of our formal branches of science at once, and not only so, they stand in direct linkage with our life and history and with all the rest of knowledge. That is, of course, why people were interested in them to begin with.

I hope I make myself plain, for there is not time to amplify illustrations. To take just one, we might choose the conversational scientific topic which crops up first on the encounter of any two Englishmen. It is, of course, the variable composition of the atmosphere. They do not announce it quite in that way. It is the water, above all, that is the centre of interest—whether there is, or has been lately, or is likely to be soon, much or little of it there in the gaseous or liquid form. The proportion of oxygen will, of course, vary accordingly; of that the man in the street knows or cares nothing. But in Universities it is the constancy, not the inconstancy, of the composition of the air that we insist upon; water vapour becomes a mere element of pressure, and the quantity of oxygen, according to the spoken and written word, is as invariably 21 per cent. as the Battle of Hastings is 1066. It is true, of course, that the substance of water vapour is recognized as

being there, but rather as an intrusion and an inconvenient extra affecting hygroscopic surfaces and some chemical actions. On the hygrometry day, if there is one, atmospheric water is formally called up for judgment; it receives justice and is recommitted to its life sentence of pressure, leaving the oxygen in possession of its unearned increment.

Try for yourselves how many fresh graduates, even those who will tell you the tension of aqueous vapour at 15° C., carry in their minds any idea of its counting as a volume, or can give any statement of the volume in summer or winter air. Will they not be astonished to hear that the variability of the amount of oxygen in the air attributable to the varying volume of water vapour alone so affects the intrinsic luminosity of a gas flame that it has, or had, to be allowed for in the photometric valuation? What heed has University science paid to atmospheric fog or to the existence of haze in unsaturated air? How many of Aitken's dust-counters are distributed among teachers of science, and how many have shown the simple experiments devised more than a generation ago by Aitken to show the formation of fogs and their intensification under town conditions? Until ions and radio-activity appeared on the scene and Mr. C. T. R. Wilson developed his beautiful work, there was little fog, at least of the kind I mean, in University courses.

Still sticking to the air and the things in it that matter much to common men, let us ask how many science students have made a plate culture of the germs of the air? It is a simple enough affair, yet of immense consequence, that can be taught to tyros, but it has been tucked away on some University shelf of specialist bacteriology.

To realize what I mean you may best go to India and see what the graduates of science are teaching in a country where sanitation and agriculture are the supreme interests of a precarious material existence. I had a class of forty such teachers in the Punjab for a fortnight, and among other things determined that, before they returned to their remote ministries in the province, they should learn a little about the transmission of malaria. I rejoice in any opportunity of doing a hand's turn towards letting humanity know what has been placed at its disposal by the magnificent work of one of your late Presidents, Sir Ronald Ross. The malaria officer eagerly lent his services and took the class out to seek the source of trouble. I have a photograph of him somewhere at the edge of a pond, showing a mosquito larva to an astonished group of M.Sc.'s and B.Sc.'s who had been searching eagerly for winged mosquitoes beneath the surface of the water. Yet these men knew and conscientiously taught such things as the tests for bismuth

and the formula of silicofluoric acid. The science of health and of endless actualities among which ordinary men spend their daily lives is in fact usually so dissevered, inverted, or altogether crowded out under University conditions, that it may never be reassembled even in the minds of those who are, by conventional standards, quite learned men and have leapt the highest academic hurdles.

I do not propose to discuss here whether what I have described as the University way is the best way for Universities. I only want to emphasize the fact that it is the usual way. The theory of Universities is that people go there anxious to learn and ready to endure for learning's sake. It is supposed that somehow they will get out of the teacher the best he has to give, even if he makes it difficult for anyone to do so. I am astonished now to think of what I suffered without complaint in my time at the hands of some University professors.

It is, however, not the University teacher or his functions that I wish to discuss. But what I do wish to stress, with all the emphasis I can give, is the unnatural form which natural knowledge acquires under conventional University conditions, because I think that the greatest difficulties of the science teacher in school have had and still have therein their real source.

I have said that the theory of a University is that it is a place to which students will come determined to learn, if necessary at some considerable cost. Turning now to schools, I am, of course, not going to say that a school is a place to which boys are sent, determined to avoid learning, if necessary at some considerable cost. Such days, if ever they existed, we have, I am assured, left far behind. No, but it would perhaps be fair to say that a school is a place where a boy is prepared to learn only on certain terms. Here again I dismiss as obsolescent, if not extinct, the habitual use of *vis a tergo*, and think of the present-day human boy as I have known him. I believe he is ready to learn on the simple terms of being interested, and I believe that in general his interest can be evoked. Of course there is a fraction, by no means negligible, whose interest will survive conditions of considerable adversity, and there may be some who have as it were no ear for the harmonies of science—but I am thinking, as school teachers must think, of the whole and the average.

It is this condition above all that must compel the teacher in schools to an outlook on science fundamentally different from that of the University teacher if he is to discharge his main duty—his duty to the many and not to the few. And it is largely because I see the realization of this condition underlying your Science for All that it invokes my blessing.

It happens, as I have already said, that my life as a University teacher began just about the time when the campaign for the introduction of science as a normal part of the school curriculum was gaining force, and if I were asked to say briefly what advance school science-teaching has made within my own recollection, I should say first, of course, that it had increased marvellously in quantity, and, secondly, that science teachers had realized that the results of the school teaching of science on the University model were so disappointing as to call for a thorough-going reconsideration.

When I first knew science-teaching in schools, it was what might be truly and adequately described as University science and water. There were laboratories exactly on the University model, lectures in lecture rooms, and as many of the University appurtenances as the school could afford. Boys were taught systematic chemistry at a tender age and entered for examinations on syllabuses prepared solely by University dons. A beneficent State intervened through an organization of examining machinery known briefly as South Kensington, whilst a make-believe University, consisting solely of examining appliances, did a brisk business in dispensing degrees for science teachers. I speak generally and not without clear recognition that in this desert there were oases, where, as is ever the case, one found individual teachers whose springs of original sinlessness no system could inhibit. I am not one of those who can look back placidly or gratefully on bad beginnings, because after all they were beginnings of something eventually to become good; it is a very easy form of charity. I cannot forget what I suffered myself, in these early days, as the inheritor of pupils who brought with them not only a travesty of professional science, but a great reluctance to be put into a better way. I can recall one instance where a student, who had been at more than one school, after sampling a first-year University course really much beyond his feeble capacity, assured me that he had already done chemistry three times. He was like other students I have had who, when asked to enter upon rational qualitative analysis, said they had already learned to "take solutions through the chart."

The real woe, however, lies in the thought of so much misdirected and futile effort. I should treat all this as of the past but for two reasons. One is that the difficulty is still with us, inasmuch as a great effort must still be made by the man fresh from a University if he is to recover the natural outlook of the beginner and break away from the artificialities of the University ways, and I am obliged to say that I think this is still often not achieved.

The other reason I put forward to justify the stress I have

put upon this particular matter arises from the recent changes which conduce to specialization in separate sciences during the last two years of school life, and, of course, at the old Universities entrance scholarship conditions for lads of nineteen give sanction to this practice. I know that this has not been done without a great deal of consideration, and I claim no further right than to say that, so far as my own judgment and experience guide me, I am led to look upon it with many misgivings. The broad reason will be plain from what I have already said. But I know that this view is bound up with obstinacy in a belief that an experienced specialist in a great main branch of science in a University, if he has really the qualities of a philosopher, inevitably develops a strong individuality of view and treatment, and that, if he has, or thinks he has, the zest and gifts of a teacher, he will insist upon expounding his philosophy to newcomers on the fundamentals in their play among the simpler material of his subject. It is natural that new students, who after all are very young, should resent on their own part and on behalf of their earlier teachers what may well seem an invitation to begin again on what they have already been taught. I have never blamed such pupils, and have often reconsidered my ways, but in the end have always reverted to them.

Believe me, I should have shrunk from this confession were it not for the knowledge that there are other University teachers of incomparably greater importance, including men far from the stage of *emeritus*, who hold the same view as tenaciously. It is not pleasant to proclaim the belief, as you will well understand. I know the reply; it is given in a well-known document. It is that a commoner practice than the one I have described is to put the teaching of the younger and inexperienced University students in the hands of the younger and inexperienced members of the staff. If it is true, it is equally true, in my opinion, that it is wrong. It exhibits the existence in Universities of an evil that still lingers throughout the educational system. And there I will leave the question and draw my remarks to a conclusion.

I do not think I have ever watched school science without being imbued with two feelings—one a strong desire to try my hand at it, the other a sure conviction that I should have been driven to despair if I had attempted it under what have been average school conditions. To deal with a language in a class-room, where the element of handicraft is almost entirely absent; where we can without difficulty or disadvantage get into action and can equally cease fire almost at the word of command; where the human boy with his ungainliness has but limited scope for physical destruction and cannot

leave the path of knowledge strewn with the material débris of teaching appliances—that is one task. A few books collected, a black-board dusted, and all is ready for another start. The task is not so easy even there as my words might suggest, but it is profoundly different from that of a science master in a laboratory. If science is to be taught as it only can be taught properly, in association with initiation into a delicate handicraft, with allowance for individual skill, individual incidents and accidents, individual questionings; if it is to have the allowance of time that will keep it free from the element of breathlessness, the necessities of the teacher justify an economic demand that I am sure has rarely been recognized and still more rarely met. I have said nothing of the pace of science and of the transforming discoveries, which make it an imperative necessity that the teacher shall keep his ears and eyes open to all that is going on in the great workshops of new knowledge. And I have said nothing of the peculiar difficulties that obstruct the science teacher in maintaining and developing his own zest and his participation in scientific investigation. I can only say that I am astonished how much good work has been done under such conditions.

In what I have said to-night I have endeavoured to draw your attention to one or two considerations which I thought might be worth your attention, and I have expressed myself quite candidly. Ever since I sat down to write this address, I have wished that I had chosen rather to talk to you about some particular topic of science to which I had given attention. I feared, amongst other things, that by contributing to the flow of educational rhetoric which sets in at this season of the year, I might help to mask the character of your Association, which is so eminently bent upon doing things and discussing them quietly among yourselves. If in anything I have said I have seemed in the least degree lacking in sympathetic appreciation of the difficulties with which you have to contend, I can only hope you will believe that it arises from a lack of skill in words and not from want of heart or understanding.

NOTES

The Story of Insulin and the Nobel Prizemen in Medicine for 1923 (Prof. J. Playfair McMurrick, University, Toronto).

THE story of the preparation of insulin as a therapeutic agent is an interesting one. The results obtained by a long succession of investigators, among whom Mering and Minkowski, Laguesse, Schulze and Diamare may be especially mentioned, had made it tolerably certain that the Islands of Langerhans of the pancreas played an important rôle in carbohydrate metabolism, and this idea had received confirmation from the observations of Opie and Ssobolew, published in 1900 and 1901, these revealing pathological conditions in the islands in cases of diabetes mellitus. There followed attempts to control glycosuria by feeding pancreas or by the administration of pancreatic extracts, but such attempts yielded no definite results, partly because of the lack of suitable methods, such as for the determination of the sugar content of the blood, and for that of the respiratory quotient, whereby an accurate idea of the amount of carbohydrate metabolism might be obtained, and, partly, because the exocrine ferments of the pancreas had a destructive action upon the secretion of the islands.

As far back as 1878 Pawlow had observed that ligation of the pancreatic duct in the rabbit was followed by a degeneration of the acinar tissue of the gland, and in 1889 Mering and Minkowski showed that extirpation of the gland was followed by a fatal diabetes, while ligation of the duct produced no appreciable glycosuria. A clue to the significance of these results was offered in 1900 by W. Schulze and Ssobolew, who showed in the guinea-pig and rabbit respectively that while the acinar tissue of the gland underwent degeneration on ligation of the duct, the islands remained unaltered. These results pointed to the conclusion that the islands possessed an endocrine function, an idea first set forth by Laguesse (1893), and that their secretion played an important rôle in carbohydrate metabolism. A corollary of the results was that ligation of the duct offered a method for obtaining an extract of the island tissue uncontaminated by the secretions of the acinar cells and in 1906 De Witt made attempts by this means to obtain such an extract. Unfortunately she tested her extracts only

in vitro, and while the tests as regards sugar were always positive, they also showed more or less action of the exocrine ferments, probably due to incomplete degeneration of the acinar tissue.

So, in general, the question stood when, in November 1920, Dr. Banting, in reading an article in a surgical journal, came across an account of the effects produced by ligation of the pancreatic duct, and the same idea occurred to him as had previously occurred to Mrs. De Witt. Early in 1921 he consulted Professor Macleod as to the feasibility of the idea, and in May of that year he resigned his positions in the University of Western Ontario and came to Toronto. Professor Macleod placed such accommodation as was available at his disposal, gave him an outline of the methods it seemed advisable to follow, and assigned to him, as Assistant, Mr. C. H. Best, at that time Fellow in Physiology at the University of Toronto. Throughout the ensuing summer Dr. Banting and Mr. Best carried on their experiments with unremitting perseverance and enthusiasm, and, as is known to all, their efforts were crowned with success.

At first their endeavours were directed towards obtaining an uncontaminated extract of the islands by ligating the pancreatic duct, and allowing time for the degeneration of the acinar tissue, the action of the extract being tested upon dogs made diabetic by total pancreatectomy. Record was kept of the amount of blood sugar and of that excreted with the urine, and it was found that the intravenous administration of the extract produced a rapid and invariable fall in the amount of blood sugar in the diabetic animals. So far so good, the physiological action of the island secretion had been demonstrated; but the method of obtaining it was tedious and difficult, and the question arose whether there might not be some other way of circumventing the destructive action upon it of the proteolytic ferments of the pancreas.

It was known that no active trypsin was to be found in extracts of the pancreas of fœtal oxen until the fourth month of development, and, furthermore, that the development of the island tissue preceded the differentiation of the acinar cells (Laguesse). Banting and Best proceeded next to make use of these facts by preparing extracts of the pancreas taken from fetuses less than 40 cm. in length; these extracts, rested as before, were found to produce a marked reduction of the blood sugar. They were first made with Ringer's solution, but later acidulated 95 per cent. alcohol was used, Zulzer having shown that alcohol extracts of even the adult pancreas were potent. This solvent apparently fails to extract the proteolytic enzymes while taking up the secretion of the island tissue,

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and a way was thus opened for the ready preparation of an island extract from the adult ox pancreas. The extract so prepared was thoroughly tested on diabetic animals, and the results obtained were so favourable that it was decided that the time had come for the administration of the extract to diabetic patients. This was done in the case of a boy of fourteen, suffering from a severe type of diabetes mellitus, and the result proved highly satisfactory, the percentage of blood sugar being quickly reduced by about 25 per cent.

Unfortunately the extract was found to produce a certain amount of local irritation, and to overcome this difficulty the services of Dr. J. B. Collip, of the University of Alberta, who, at the time, was carrying on a research in the laboratory of Pathological Chemistry of the University of Toronto, were enlisted, and by fractional precipitation with alcohol he succeeded in removing the irritant substances and in obtaining a much purer product, for which the name *insulin* was adopted. Thus, early in 1922, the possibility of obtaining a utilisable extract of the insular secretion and its high value as a therapeutic agent had been demonstrated, and the first chapter of the story is completed.

But much yet remained to be done. It was necessary, in the first place, to devise a method of standardising the dosage of insulin, since it had been found that an overdose might produce serious or even fatal results by too great a reduction of the blood sugar. These results were found to supervene in rabbits when the percentage of blood sugar fell to 0·045, and the unit dose of insulin was at first fixed at that amount, which within four hours will reduce the percentage of blood sugar to 0·045 in a rabbit weighing about 2 kilograms. It was found, however, that such a unit was rather large for satisfactory use clinically, and a clinical unit was subsequently established at one-third the strength of the original physiological unit.

In the second place, it was necessary to plan for the production of insulin on a somewhat extensive scale, so great was the demand for it. This was undertaken by the Connaught Antitoxine Laboratories of the University of Toronto, the work being placed under the direction of Mr. Best, who has successfully overcome the difficulties that were encountered when production on a large scale was attempted. These difficulties made manifest the necessity for some means of safeguarding the commercial production and standardisation of insulin, and to this end those chiefly concerned in the researches that led to its production took out patents in Canada, the United States, Great Britain and other countries, fully covering the methods of preparation, these patents being subsequently conveyed to the University of Toronto, which assumed the function of grant-

ing to approved applicants the right to manufacture insulin, and to sell it at a reasonable cost. Licences have been granted to a number of manufacturers in the United States, and, to safeguard production in Great Britain, the University in turn has conveyed the British patents to the Medical Research Council, which thus becomes the licensing body for Great Britain.

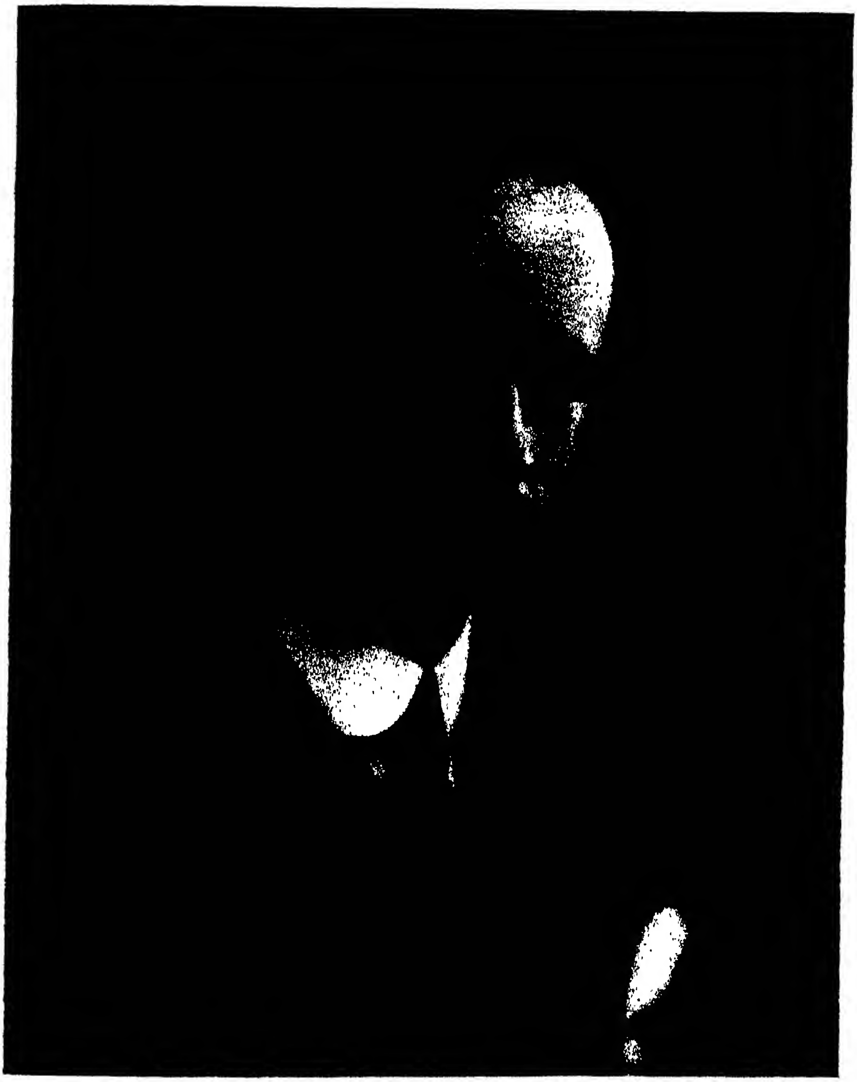
The preparation of insulin opened up a way for the investigation of many interesting physiological and clinical problems, such as the composition and mode of action of insulin, its effects on the respiratory quotient, its effect on the excretion of acetone bodies, on fat metabolism, etc., and to the investigation of these questions Professor Macleod devoted his energies, with the assistance on the physiological side of Mr. E. C. Noble, Dr. J. Hepburn, Dr. J. K. Latchford, and Dr. G. S. Eadie, and on the clinical side of Dr. W. R. Campbell and Dr. A. A. Fletcher. A discussion of the results of these investigations lies outside the scope of this article, and they are mentioned merely to emphasise the team work that has been involved in these insulin researches. They constitute a brilliant example of the value of competently directed co-operative work in the solution of scientific problems.

Professor John James Rickard Macleod is a son of the manse, and was born at Cluny, Dunkeld, Scotland, September 6, 1876. He was educated at the Grammar School, Aberdeen, and entered Marischal College of the University of Aberdeen in 1893, graduating therefrom in 1898 with the degree of M.B. and Ch.B., and with an appointment to the Anderson Travelling Fellowship. This Fellowship enabled him to spend the year after graduation in work under Professor Siegfried in the Physiological Institute at Leipzig.

On his return to England he entered upon a teaching career, being appointed Demonstrator of Physiology in the London Hospital Medical School, a position he held until 1903, holding also during the last two years of this period the Mackinnon Research Scholarship of the Royal Society. In 1902 he successfully fulfilled the requirements for the degree of D.P.H. of the University of Cambridge.

His first published paper was the result of his work at Leipzig, and was on the phosphorus content of muscle. This paper was published in 1899, and from that time onward his research activities have found expression in a long series of papers, all of a high standard of excellence. During his tenure of the Mackinnon Scholarship he was engaged, together with Professor Leonard Hill, in an experimental study of Caisson Disease and Diver's Palsy, the results of which were set forth in the *Journal of Hygiene* in 1904.

In the autumn of 1903 he accepted and entered upon the



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duties of the Professorship of Physiology in the Western Reserve University, Cleveland, Ohio, where he remained until 1908, accepting in that year a call to the University of Toronto, with which institution he is still connected as Professor of Physiology and Vice-Dean of the Faculty of Medicine. Shortly after assuming his duties at the Western Reserve University, he took up the studies of carbohydrate metabolism and glycosuria, his first paper on the subject having been published in 1905, and followed in succeeding years by numerous contributions, which led up to the publication in 1913 of a monograph on *Diabetes: Its Pathological Physiology*. On assuming his duties at the University of Toronto he turned to the investigation of problems connected with the physiology of respiration, and published during the succeeding years a number of papers dealing with these questions. The year 1918 also saw the publication of his timely and highly appreciated treatise on *Physiology and Biochemistry in Modern Medicine*. Recognition of his standing as a physiologist was shown in his election in 1919 to a Fellowship in the Royal Society of Canada, and to the presidency of the American Physiological Society in 1920.

On the coming of Dr. Banting to Toronto with his problem Professor Macleod, realising that the possibilities of success had greatly increased with the recent elaboration of methods for determining accurately the sugar content of the blood, willingly placed at Dr. Banting's disposal the facilities of his laboratory, and discussed with him the methods to be employed. Engagements previously made rendered necessary Professor Macleod's absence from Toronto during the summer, and on his return he found that Dr. Banting and Mr. Best were far along the high-road to success, and he at once devoted his abilities for organisation to bring about the biochemical and clinical co-operation necessary for complete success, at the same time turning all the members of his staff that could be spared for the purpose to the investigation of problems correlated with the main question. It was largely owing to his directing and executive ability that the commercial preparation of insulin was made possible and safeguarded.

The important part taken by Professor Macleod in the preparation of insulin received recognition in 1923 by his election to a Fellowship of the Royal Society, by his selection as Cameron Prizeman of the University of Edinburgh, by his being granted the honorary degree of D.Sc. by the University of Toronto, and by the award to him and Dr. Banting conjointly of the Nobel Prize for Medicine. Professor Macleod generously turned over to Dr. J. B. Collip one-half of his share of this prize, as a recognition of the important aid rendered by Dr. Collip in the purification of insulin for use as a therapeutic agent.

Dr. Frederick Grant Banting, the sharer in the Nobel Prize for Medicine for 1923, was born at Alliston, Ontario, November 14, 1891, his parents being of that yeoman stock that has made the western peninsula of the Province of Ontario one of the finest agricultural districts of the Dominion. Dr. Banting received his early education in the Alliston Public School, passing thence to the High School, and in 1910 he matriculated in the Faculty of Arts of the University of Toronto. He remained in the Faculty of Arts only one year, transferring in the autumn of 1912 to the Faculty of Medicine, from which he received the degree of M.B. in 1916.

In the meantime the Great War had broken out, and in March 1915 Banting joined the army, in which he remained for four and a half years, serving in Canada, England and France. He was wounded at Cambrai, and, as a recognition of his valuable services, was awarded the Military Cross. During his service in England he took advantage of the opportunities offered for continuing his medical studies, and in 1918 successfully underwent the examinations for the L.R.C.P. and M.R.C.S.

After demobilisation he returned to Canada, and was for a time Resident Surgeon in the Hospital for Sick Children, Toronto, serving at the same time as Demonstrator in Anatomy in the University of Toronto. In 1920 he removed to London, Ontario, with the intention of engaging in surgical practice, and while there employed his leisure time by acting as Assistant in Physiology and Demonstrator of Anatomy in the Medical Department of the University of Western Ontario. It was while holding these positions that he came to the decision to devote himself to the study of the functional activities of the Islands of Langerhans, and early in 1921 he returned to Toronto and, with Mr. C. H. Best as an assistant, he began studies in the Physiological Laboratory of the University with results that are now known to all.

In thus acting, Dr. Banting sacrificed for an idea, temporarily at least, his prospects of success as a surgical practitioner, and, being without private means, he was obliged to eke out a livelihood while devoting himself to his chosen investigation. Some opportunity he found for this in a Lectureship in Pharmacology, which he held during the session 1921–22, and in the following year in a Demonstratorship in Medicine, both in the University of Toronto. With the successful outcome of his studies these necessities passed, for the University of Toronto appointed him to a Professorship of Medical Research, for which the Provincial Government set aside an annual sum of $10,000, half of which was to form the stipend of the Professorship, while the other half was to be divided equally between

an assistant, a position now held by Mr. C. H. Best, and the maintenance of a research laboratory. Further, the Dominion Government, in recognition of Dr. Banting's services to medicine, voted him a life annuity of $7,000. Such public recognition of scientific achievement constitutes a hopeful indication of interest in scientific research.

Several other honours have been conferred on Dr. Banting during the past year. The University of Toronto granted him the degree of M.D. in 1922, and that of D.Sc. in 1923. Queen's University in the latter year conferred upon him the degree of LL.D., he was elected to Fellowship in the American College of Physicians, and was the recipient of the Charles Mickle Fellowship, an honour annually awarded "to that member of the medical profession, who is considered by the Council of the Faculty of Medicine of the University of Toronto to have done most during the preceding ten years to advance sound knowledge of a practical kind in medical art or science." Dr. Banting is the third holder of this Fellowship, his predecessors having been Professor Pawlow and Dr. Harvey Cushing. Conjointly with Professor Macleod he was awarded the Nobel Prize for Medicine for 1923, and has generously assigned one half of his share of that prize to Mr. C. H. Best, whose able assistance contributed much to the successful outcome of the search for insulin.

Anopheles plumbeus (R.R.)

The South-eastern Union of Scientific Societies has issued a reprint from its *Transactions* giving an account of the work of its Mosquito Investigation Committee on *Anopheles plumbeus*, with a Preface by the Chairman, Dr. Clarence Tierney. It is stated that work upon English mosquitoes was started by the Union in 1917 in consequence of the action by the (now) Ministry of Health, which suggested that the return of cases of malaria to Britain might prove dangerous to the home population, and which asked the Union to study the distribution of British Anophelines. I think, however, that this idea was really started by the War Office itself; and in fact Sir Alfred Keogh, G.C.B., then Director of the Medical Services, appointed me Consultant in Malaria at the War Office very early in 1917 expressly in view of this possibility. However this may be, the Union has done sterling good work. The pamphlet referred to considers chiefly *A. plumbeus*, which has been proved by the Liverpool School of Tropical Medicine to be a malaria carrier and which breeds almost exclusively in rot-holes in trees. It is not likely to be dangerous now, but may have been so when the British were a jungle-living race. Let us hope that the politicians will not reduce us to the same condition again.

Pollution of Rivers and Fisheries

The Standing Committee of Rivers Pollution and Fisheries of the Ministry of Agriculture have issued a very interesting "non-technical report" of their work (Stationery Office, 1924, 3*d*. net). The matter is of great importance to fishermen, whose sport is apt to be much affected by poisonous

effluents into our rivers. This is a serious matter, for, as the report says: "It has often been stated that the inland fisheries' interests are confined to a comparatively few well-to-do persons, who as often as not have a direct financial interest in the condition of their fishing waters. In the Committee's opinion this is an altogether erroneous idea, since certain members of the Committee alone represent Angling Associations with a membership of well over a hundred thousand, chiefly working men, and in all there must be at least half a million anglers in the country. The appeals of this number of people for the preservation of the means of their recreation and of those people who, while not being anglers, gain recreation, pleasure, and amusement from the rivers, by swimming, boating, and other means, should not be lightly ignored."

The causes of injurious pollution are classified as follows:

"(1) That caused by the introduction into the water of substances (*e.g.* sewage) which deprive the water of its dissolved oxygen which fish require for the purpose of breathing. If the withdrawal of oxygen goes on to any great extent, fish are suffocated unless they can avoid such areas of pollution.

"(2) That caused by the presence in the water of substances which irritate and poison the fish. Waste products from certain industrial processes come under this heading, and the number of possible poisons is very large.

"(3) That caused mechanically by silting up the river-bed and overlaying weeds and fish spawn, thus preventing reproduction; or, by injuring the eyes and gill membranes of fish, thus setting up inflammatory conditions leading to cataract or blindness on the one hand, or 'gill fever,' which often results in the death of the fish."

We fear that progress in preventing such pollution is not very fast.

A Plea for the Establishment of a Bio-geo-chemical Institute (J. Johnstone, D.Sc.).

The Report of the Port Erin Biological Station for 1923 (Liverpool University Press) contains a short article by M. W. J. Vernadsky, pleading for the institution of a laboratory which will be a link between Biology and Geo-Chemistry. M. Vernadsky's full appeal is made in the *Revue Générale des Sciences* of January 30, 1923, and January 15 and 30, 1924. The extraordinary influence of life in the moulding of the superficial part of the earth's crust is emphasised, and our equally extraordinary ignorance of the quantitative chemical composition of living matter is also stressed. We learn, with some surprise, that there is hardly a single modern complete, quantitative analysis of the *whole* body of an animal or plant, marine or terrestrial. Far less do we know the mass of living matter in general, to say nothing of the masses of the various categories of organisms inhabiting even restricted regions of land or sea. Problems of extraordinary general interest call for the knowledge of such data, while special problems in geo-chemistry, such as the destruction or alteration of rocks and minerals by living organisms, cannot be touched without very much more data than we have at present. M. Vernadsky points out that the little data we do possess has been obtained by biological and agricultural institutes with quite different objects. He presses for the creation of an institute which will deal with the organisation of the kind of research which he outlines in his large memoir.

Brainless Britain.

We are very glad to hear that our excellent contemporary *Discovery* is taking on a new lease of life from the beginning of this year—there was some danger of the cessation of it. It is much more interesting than most

of our magazines, with their eternal politics. One of the most curious phenomena to be observed in this country is the almost universal indifference to science, although science has given the people many more benefits than they have ever received from their politics and their frequently spurious philosophy. Sir Arthur Shipley, G.B.E., has some very pertinent remarks on this subject in the January number. "A typical example of this occurred," he says, "but a few months ago. Westminster Hall, with its restored roof, was reopened by the King, many speeches were made and many articles were written dwelling on the history of that noblest of Halls, and on the many dramatic episodes that had taken place in that stupendous building, yet, so far as I know, not a single speaker and not a single writer referred to the gifted entomologist who had made the restoration of the roof and the preservation of its old timbers possible. Historians are notoriously bereft of a sense of proportion, and whilst in many articles they dwelt upon the great scenes that had been enacted in the hall, not one of them ever remarked on what was perhaps the most dramatic and most important scene of all, the finding by a distinguished Professor of Entomology of the London School of Science of the larvæ of the beetles that had for decades been eating up the woodwork in the roof, and his still more remarkable application of a chemical compound which proved fatal to the beetle and all its works and made the beams whole."

The British Association

The next meeting of the British Association is advertised to take place at Toronto from the 6th to the 13th of August of this year. Forms for attendance should be applied for to the British Association Office, Burlington House, W.1. The following are Presidents of the Sections:

A. Mathematics and Physics. Sir William Bragg, K.B.E., F.R.S.
B. Chemistry. Sir Robert Robertson, K.B.E., F.R.S.
C. Geology. Prof. W. W. Watts, F.R.S.
D. Zoology. Prof. G. Elliot Smith, F.R.S.
E. Geography. Prof. J. W. Gregory, F.R.S.
F. Economic Science and Statistics. Sir William Ashley.
G. Engineering. Prof. G. W. O. Howe.
H. Anthropology. Dr. F. C. Shrubsall.
I. Physiology. Dr. H. H. Dale, C.B.E., F.R.S.
J. Psychology. Prof. W. McDougall, F.R.S.
K. Botany. Prof. V. H. Blackman, F.R.S.
L. Educational Science. Principal Ernest Barker.
M. Agriculture. Sir John Russell, F.R.S.

Sir David Bruce, K.C.B., F.R.S., is to be President.

Notes and News

The Honours list published on the occasion of the New Year showed but little recognition of the work of scientific men. Knighthoods were conferred on Mr. H. T. Mottram, H.M., Chief Inspector of Mines, Board of Trade, and on Mr. H. Murray, Assistant Forestry Commissioner of England and Wales, while Professor W. R. Dunstan, until lately Director of the Imperial Institute, was made K.C.M.G. There were also a number of awards to medical men.

The Council of the Royal Society has decided to recommend for election to the Society the following candidates; Dr. T. N. Annandale, Director of the Zoological Survey of India; Mr. J. E. Barnard, Past President of the Royal Microscopical Society; Dr. J. F. Gemmill, Professor of Natural History, University College, Dundee; Dr. M. H. Gordon, Bacteriologist to St. Bartholomew's Hospital; Dr. P. Groom, Professor of the Technology of

Woods and Fibres, Imperial College of Science; Mr. C. K. Ingold, Lecturer in Organic Chemistry, Imperial College of Science; Mr. P. F. Kendall, late Professor of Geology, Leeds; Dr. L.V. King, Macdonald Professor of Physics, McGill University, Montreal; Mr. L. J. Mordell, Reader in Pure Mathematics, University of Manchester; Dr. T. Slater Price, Director Photographic Research Association; Mr. C. V. Raman, Palit Professor of Physics, Calcutta; Mr. L. T. Rogers, formerly Professor of Mathematics, University of Leeds; Dr. A. Russell, Principal, Faraday House Electrical Engineering Society; Dr. C. Spearman, Grote Professor of Psychology, University College, London; Mr. F. Twyman, Managing Director, Messrs. Adam Hilger & Co., London.

The Royal Society has now made the appointments to the three research professorships which it has been able to establish as a result of the generosity of Sir Alfred Yarrow. The Society has been able to set a very high standard for the appointments, since the holders are Prof. A. Fowler, Major G. I. Taylor, and Prof. O. W. Richardson.

The gold medal of the Royal Astronomical Society for 1924 has been awarded to Prof. A. S. Eddington for his work on star-streaming, on the internal constitution of stars, and on the generalised theory of relativity.

The Symons gold medal of the Royal Meteorological Society has been awarded to Dr. Takematsu Okada, Director of the Central Meteorological Observatory, Tokyo, Japan.

Mr. J. Barcroft has been elected Fullerian professor of physiology at the Royal Institution in succession to Sir Arthur Keith. M. le Duc de Broglie, Dr. C. L. Guillaume, and Profs. Debye, Einstein, Groth and von Laue have been elected honorary members of the Institution.

The Wollaston medal of the Geological Society has been awarded to Dr. A. Smith Woodward, the Murchison medal to Dr. W. Gibson, and the Lyell medal to Mr. W. W. King.

Among the names of the men of science who died during the last quarter were those of Canon T. G. Bonney, geologist; Dr. F. Clowes, chemist; Dr. A. Gleichen, optician; Lieut.-Col. H. H. Godwin-Austin; Prof. H. J. Hamburger of Groningen, physiologist; Prof. J. Harkness of McGill University, mathematician; Dr. O. Klotz, Director of the Dominion Observatory, Ottawa; Dr. J. Loeb, physiologist; Brigadier-General G. E. Pereira, geographer; C. P. Steinmetz, electrical engineer; Sir Frederick Treves; Sir John Tweedy, ophthalmic surgeon; Canon T. Wood, naturalist.

As a result of a reorganisation of the research department of the Air Ministry, it is understood that Lieut.-Col. Tizzard has been appointed Director of scientific research and Air-Commodore F. C. Halahan, Director of technical development. Previously there had been a single director of research who was responsible both for the scientific work carried out at Teddington, Farnborough, the Imperial College of Science, and other places, and also for the application of the results of such research and of the data obtained by practical flying experience to the technical development of aircraft, aero-engines, and accessories.

The French Physical Society celebrated its fiftieth anniversary last December by an exhibition in the Grand Palais, Paris, and by a series of anniversary lectures delivered during the week December 8–14. The importance of the occasion and of the Society were fully recognised by the Government. On December 13 President Millerand took the chair at the meeting held in the Sorbonne and received from Prof. Lorentz the addresses which had been brought by delegates from all over the world. The fiftieth anniversary of the Physical Society of London which occurs in March this year will also be celebrated in a fitting manner. In particular there will be a banquet, at which the Duke of York has promised to preside.

The third attempt to reach the summit of Mount Everest will be made towards the end of May next. The expedition will again be in charge of

Brigadier-General C. G. Bruce, with Major E. F. Norton as second in command. The climbing party will consist of Major Norton, Captain Bruce, and Messrs. Mallory, Sommervell, Odell, Beetham, Irvine, and Hazard, the four last mentioned being new members of the party. The route from Darjeeling will be that followed in 1922, and the base camp is to be established in the Rongbok Valley. Oxygen apparatus modified in the light of the experience obtained in 1922 will be carried. In his articles in the *Times* Brigadier-General Bruce states that the expedition has now the personnel, equipment, and knowledge necessary to ensure success, and needs only a late monsoon to achieve it.

A committee convened by the Royal Society has made preparations for the celebration of the centenary of the birth of Lord Kelvin, which occurs on June 26. In view of the fact that a World Conference on Electric Power will be held at the British Empire Exhibition during the first fortnight in July, it has been decided to hold the actual celebrations on July 10 and 11. There will be an exhibition of Kelvin apparatus, a dinner, and a meeting at which addresses will be presented and a memorial oration delivered by Sir J. J. Thomson.

The Imperial Economic Conference last year considered a report from a committee appointed by the Secretary of State for the Colonies, to consider the affairs of the Imperial Institute, which was, it appears, in financial difficulties. As a result of the proceedings it has been decided to close the Public Galleries of the Institute and to lease them for use as a War Museum for a rental of £8,000, which will be added to the revenues of the Institute. This will continue its existence at South Kensington as a clearing-house of information equipped with laboratories for the preliminary analysis and investigation of raw materials. For this purpose it will be amalgamated with the Imperial Mineral Resources Bureau and will be under the control of the Department for Overseas Trade. In view of these changes Prof. W. R. Dunstan has resigned his post as Director of the Institute and, pending the reorganisation, Mr. H. M. Lidderdale has been appointed Acting Director.

A small portion of the science collection which was removed last year from the Western Galleries of the Science Museum at South Kensington is now on view in the new Science Museum. This building is, however, still in a very incomplete state, and it would seem probable that another year at least must elapse before the whole collection is again available for public

It will be remembered that an experimental explosion was caused at Oldenbroek in Holland in October 1922 in order that further information might be obtained concerning the propagation of intense sounds and, more especially, for the investigation of the silent zone which separates the two areas within which such sounds may be heard. This experiment was not satisfactory in that few observations could be taken on the western side of the explosion on account of the proximity of the sea. The experiment is therefore to be repeated at a station in Central France on three different days in May. The results should be of particular interest, because there is some doubt as to whether a silent zone is formed at all in Europe during the summer months.

The Americans have been very slow to take up vaccination properly and we are therefore glad to read the following remarks made by Dr. Victor Heiser of New York, made before the American Medical Association Meeting in June 1923 (*Jour. Amer. Med. Association*, vol. lxxxi, September 29, 1923):

"One hundred and twenty-seven years ago Jenner made knowledge available which, if effectively applied, would have brought smallpox under complete control. But its adoption was not universal, and millions have died from smallpox since then. The anti-vaccinationists of the past have largely contributed to such a result, but that is behind us and cannot be remedied.

The future, however, is still before us. Shall a few fanatics, with their unproved allegations, be allowed to spread disfiguration, blindness, and death to innocent victims of their folly? Should we suffer a person to burn down his house because he believes it non-inflammable, if his action menaces the houses of others? Certainly not. Yet we remain complacent when persons promote conditions that destroy lives and happiness.

"Those who are indifferent and who fail to have their children vaccinated are also contributors to the continuance of smallpox—that disgrace of civilisation. The catastrophe that recently occurred in the Philippine Islands, when more than 50,000 persons lost their lives from smallpox, should be a warning to the people of the United States. The conflagration in the Philippines was made possible by a huge group of unvaccinated children, and the heat of it was intense enough to affect those who were semi-fireproofed because their vaccination was of too long standing."

The World's Health for January (published by the League of Red Cross Societies) contains an interesting article by Dr. M. D. Mackenzie on malaria in Russia. It appears that the disease has been spreading since 1922 in a very virulent form within a vast area bounded by Moscow and Nijni-Novgorod in the north, by the western frontier of Russia in the west, by the Black Sea and the Caucasus in the south, by the Urals and Turkestan in the east. The disease continues almost through the winter into the next spring; the author attributes this to the fact that the *Anopheles* mosquitoes which carry the infection are allowed to breed in tubs of drinking water constantly kept in the heated houses of the peasantry. Generally, of course, these mosquitoes breed in pools in the open air and therefore cease to be much propagated as soon as the chills of autumn commence, thus limiting new infections to summer and early autumn. Quinine obtainable by the peasants seems to be much adulterated, but the British Red Cross is issuing large amounts of it in solution, distributed by a "network of malaria outposts which are gradually being established in the Buzuluk district, an area of about the size of Wales—but this area is negligible when compared with infected areas of Russia as a whole." The people seem to be too ignorant or stupid to deal with their own water-tubs—which could easily be freed from larvæ by a few drops of oil on the surface. We suppose that the peasantry, although they have been "liberated" by the Bolsheviks, have not reached such a stage of civilisation as to perform this little operation once a week. This fact gives a picture of the difference which exists in this world between real and imaginary advance—between science and sham politics. The adult mosquitoes could also be easily destroyed by fumigation of the interiors of the houses or huts. The Red Cross is to be complimented upon this piece of work.

The Fourteenth Annual Exhibition of the Physical and Optical Societies was held at the Imperial College of Science on January 2 and 3. The number of exhibits was rather larger than usual and the attendance quite satisfactory. One thing, however, had not improved, namely, the efficiency of the demonstrators. It would seem fairly obvious that an exhibit at an exhibition of this kind is useless unless someone is present who is both *competent and willing* to explain the special points of the apparatus in his care and who also has at least some approximate idea concerning its price. In actual fact, however, many of the exhibits fell into one of the three classes: (1) those at which no demonstrator at all could be detected; (2) those in which he was too busy with his friends to bother about the unknown visitor; (3) those in which a technical expert was present who knew nothing about the business side of the work of his firm. These remarks are by no means limited to the exhibits of the smaller or less well known concerns; they apply to some of the best-known firms represented at the Exhibition. The customary lectures were given by Mr. H. B. Grylls, who described the Heape and Grylls Rapid

Cinema Machine, and Sir Richard Paget, who dealt with the artificial production of vowel sounds, a subject on which we hope to have an article in the July number of SCIENCE PROGRESS.

The Grylls and Heape Cinema machine is designed to take photographs at rates varying from 500 to 5,000 per second. In principle it is the reverse of the focal plane camera, for the slit is fixed, while lens and film move rapidly past it. The film is held by air pressure against the rim of a large flywheel; the lens, or rather lenses, are fixed near the periphery of a disc geared to the flywheel. The exposure for each picture may be as small as a sixty-five thousandth part of a second, the necessary illumination being obtained by the use of searchlights or by burning large quantities of flash powder (50–70 lb. during a single experiment which lasts only a small fraction of a second). The apparatus is being used by the Admiralty for the investigation of the impact of projectiles on armour plate.

The fifth and last Report of the B.A. Committee on *Colloid Chemistry and its General and Industrial Applications* has recently been issued (H.M. Stationery Office, Imperial House, Kingsway. Price 2*s.* 6*d.* net.) It contains six papers and an index to the whole set of five reports. In the first paper Dr. A. Ferguson discusses the methods available for measuring the surface tension between a liquid and its vapour, interfacial surface tensions, and the angle of contact. In particular he discusses the conditions under which the drop-weight method may be expected to give reliable results. He also gives a useful bibliography to the recent literature dealing with surface tension. A short paper by Mr. C. A. Mitchell contains some interesting details relating to the manufacture of inks, while Mr. E. Wheeler deals with the manufacture and properties of artificial silk. The amount of scattered information which has been collated for these reports is so immense that it is hopeless to attempt to abstract them. It is only possible to congratulate the Committee on a worthy ending to a monumental task.

We have received three pamphlets issued by the Department of Scientific and Industrial Research dealing with the work of the Food Investigation Board: a General Report of the Board for 1922 (price 1*s.* 6*d.*); Special Report No. 14, on *The Thermal Properties of Ethyl Chloride*, by C. F. Jenkin, C.B.E., M.Inst.C.E., and D. N. Shorthose, M.A. (price 1*s.* 6*d.*), and Special Report No. 16, on *Canned Fruit*, by William D. Savage, M.D., and R. F. Hunwicke, B.Sc. (1*s.* 3*d.* net. All from H.M. Stationery Office as above). The General Report contains some interesting results relating to the preservation of eggs by cold storage. It is found that as long as the eggs are frozen and the temperature is not reduced beyond $-5°$ C. they will, when thawed, revert to their original state, but if the temperature falls below this point a rapid, irreversible change occurs. These experiments completely upset the accepted theory of the freezing of tissues and show the need for much more experimental work. Prof. C. F. Jenkins's report contains an account of the experimental methods used to determine the thermal constants of ethyl chloride. The results are summarised in the temperature-entropy and the total heat-entropy charts which accompany the report. The work was undertaken in order that this substance might be used in refrigerating machines. The writers do not appear to express any definite opinion as to its suitability for the purpose suggested; the difficulties they had to overcome would, however, seem to indicate that it is not likely to be used on a commercial scale.

The Mineral Resources Committee of the Imperial Institute has added to its list of monographs on mineral resources a volume on *Cobalt Ores* by Edward Halse, A.R.S.M., and another, *Vanadium Ores*, by members of the Staff of the Institute (John Murray, price 3*s.* 6*d.* and 5*s.* net respectively). These reports are compiled on the comprehensive lines established by the earlier volumes and that on Cobalt contains a reference to the remarkable

magnetic alloys which have been produced by the use of the element and which have made it possible to reduce the weight of ignition magnetos to one-third or less of its previous value. (The actual data given on p. 14 would seem, however, to require revision or, at least, further explanation.) It is also stated that a cobalt-chromium alloy (cochrome) may be drawn into wires which are in some respects superior to nichrome when used for electric heating elements.

A full summary of the proceedings of the second Empire Forestry Conference, which was held in Canada from July 25 to September 7, is to be found in the December issue of the *Empire Forestry Journal* (published by the Empire Forestry Association, Imperial Institute, London, price 4*s*. net), which will henceforth be the medium for the publication of official and technical information collected by the Standing Committee on Empire Forestry. Considerable concern is expressed regarding the rapid exhaustion of the pulpwood forests in Canada, which threatens grave peril to the pulp and paper industry in that country within the next ten or fifteen years. The annual value of the products of this industry exceeds £30,000,000. It is one of Canada's principal industries and the need for the conservation and development of the forest resources is most urgent. One most important aspect of this question is the prevention of forest fires. Some 90 per cent. of these are due to human agencies. During the last five years about 29,000 such fires were reported, resulting in a loss of property to the value of 72 million dollars. The chief source of these fires is the debris or slash left by settlers in their clearing operations or by loggers, and the proper disposal of the slash is one of the most urgent and difficult problems confronting the Forest Department. It is hoped that aircraft may play an important part in the early location of fires, and the Canadian Government has purchased a number of hydroplanes specially designed for the purpose.

The Physical Society of London has issued a second edition of the *Report on Radiation and the Quantum Theory*, written by J. H. Jeans (London: The Fleetway Press. Price 7*s*. 6d. net.). The author states that "such omissions as have been made have been prompted by the feeling that the Quantum Theory need no longer be considered on the defensive; the additions, which are much more numerous than the omissions, represent merely the filling in of the old framework which has been necessitated by the rapid growth of the theory." A casual glance at the references, bearing in mind that the first edition of the Report was published in 1914, shows that the additions are very numerous indeed. Bohr's theory dates only from 1913, so that almost the whole of the admirable summary of the applications of that theory to the spectra of the elements (chapter iv) is new. The greatest interest will, however, be excited by the last section, which contains suggestions towards a physical basis for the theory. The components of the electric force which appear in Maxwell's equations are identified with some function of the probabilities of jumps in the velocity of an electron which forms part of an atomic system. It is shown that this hypothesis would include classical theory as a special case appropriate to radiation of great wave-length. On the other hand, it would no longer permit us to think of energy as "being localised in free space, and instead of thinking of a spherical wave as carrying with it a store of energy in each element of its wave-front, to be yielded up to whatever matter it encounters we (must) rather think of it as carrying with it a potentiality of yielding up energy to such matter as it encounters in a suitable state to receive this energy. The amount of energy it may yield up remains always equal to $h\nu$, but the chance of its yielding up this amount falls off as $1/r^2$." The conservation of energy remains, but only as a statistical law not necessarily true in every individual case, but with the same probability of truth as the second law of thermodynamics. The last paragraph in the *Report* is by no means the least interesting. The author

states that "The supposed discontinuity may itself prove to be unreal; the 'jumps' may ultimately be found to consist of a succession of continuous steps. If there is a real difficulty in imagining discontinuity to be the ultimate rule of nature, we can escape this difficulty by imagining a sort of sub-universe such that the apparently discontinuous processes of the quantum-theory result from a rapid succession of continuous changes in this sub-universe." Physicists of the older school will not fail to appreciate the importance of such remarks.

Lieut.-Col. John and Lady Helen Murray gave an At Home on February 19, at which Mrs. Wetherell read an interesting account of the progress of the David Livingstone Film Expedition now following the course of Livingstone's journeys in Africa, with a view to making a great instructional film regarding them. The expedition started towards the end of last year and is now following Livingstone's travels step by step. Viscount Ullswater, G.C.B., presided and speeches were made by Earl Buxton, the Bishop of London, Sir Francis Younghusband and others, and Mr. John Murray showed a number of very interesting Livingstone relics.

The *Monist* for January contains an admirably written paper by Harry Elmer Barnes on "Theories of the Origin of the State," in which the views of the ancients from Socrates to Seneca are carefully recorded and analysed. It seems curious to us that in attempting to explain aggregation of dwellings none of these writers cited the necessity for mutual protection against enemies. Probably war has really done as much to advance civilisation in some directions as to retard it in others.

We publish an amusing letter from Mr. E. H. Hankin on his experiences connected with medical research in India. So far as we know he has never received the smallest recognition from his intelligent countrymen for his invaluable method of preventing cholera by putting potassium permanganate in wells. Luckily for him, however, he has hitherto escaped any serious punishment for this—such as Mr. Haffkine received in connection with his invention of the vaccines against cholera and plague which have saved so many lives in that country. He should not have been let off so easily. It is really high time for men to discover some form of crucifixion for their medical benefactors so that people may be left to die in piece by millions if they choose. Colonel King also gives some pleasant examples of the manner in which medical benefactors should be treated. Why not light the fires in Smithfield again?

CORRESPONDENCE

To the Editor of SCIENCE PROGRESS

DR. KAMMERER IN CAMBRIDGE

FROM PROF. E. W. MACBRIDE, F.R.S.

DEAR SIR,—Will you allow me space to make a brief reply to the letter of Mr. Thacker which appears in your issue of January? In that letter Mr. Thacker takes me to task for attributing to him the belief that "germinal variations" are causeless.

I am extremely sorry if in my letter to SCIENCE PROGRESS of October 1923 I misunderstood and misrepresented Mr. Thacker. I am very glad to learn that he does not regard the phrase "germinal variation" as a final explanation of hereditary change, and that he admits that the causes of germinal variation may be external as well as internal.

This attitude I regard as moderate and reasonable. But I think that Mr. Thacker errs in attributing his own position to the majority of Mendelians. It is the experience of both Dr. Kammerer and myself that when these gentlemen are called on to explain an hereditary change they fall back on the "dropping of a factor" or the "appearance of a factor which the stock was carrying," and consider their explanation complete. The climax of absurdity was reached at the Liverpool meeting of the British Association when one Zoologist, describing the occurrence of "intersexes" amongst pigs in one of the Pacific islands, explained their occurrence by assuming the existence of a "factor for intersexuality."

May I remind Mr. Thacker that Weismann himself, when hard pressed, admitted a Lamarckian origin—in the last resort—for his variations? This is a point which is constantly overlooked by his followers. Weismann's conception of the nuclear constitution of the germ-cells was that it consisted of a number of complexes which he termed "ids." Each "id" was theoretically able to carry through the development of the egg. Variation took place through the random shuffling of the "ids" in the production of polar bodies. The "ids" resembled each other closely, but not completely, and the differences between the "ids" were traced back to *the unicellular ancestors of the race in which they had been acquired by a Lamarckian reaction to the environment of these simple organisms.* Therefore, according to Weismann, unicellular animals could acquire in a Lamarckian manner new qualities, whilst multicellular animals could not!! I commend this explanation to Mr. Thacker's better judgment.

I agree with Mr. Thacker that Dr. Kammerer's work ought to be repeated and confirmed. That, incidentally, is what Mr. Boulenger and I have been endeavouring to do in the Zoological Gardens of London for the last four years. We are working on the colour reactions of *Salamandra maculosa*, and so far as we have gone our observations confirm Dr. Kammerer's work. We have only succeeded, however, in rearing the first generations to maturity. With luck this spring we hope to get them to breed, and then in a few years more we ought to be able to report decisively on the matter. This will give your readers some idea of the difficulty of dealing with questions of this kind. Mr. Thacker refers to the experiments of my

old friend Mr. Fox on Ciona as justifying the hesitation which he feels in ing Kammerer's results. I think that by this time he will have that Mr. Fox's work gives no reason for this hesitation. Mr. Fox was unable to get the reaction of Ciona to the experiment of amputating the siphon which Kammerer had described; but Kammerer's observations were a mere repetition and extension of Mingazinni's carried out twenty-five years previously. Of course Mingazinni did not test the inheritability of the acquired character, but neither did Mr. Fox. Perhaps I may be allowed to add that it was Mr. Fox himself who in 1914 first drew my attention to the importance of Kammerer's work.

Finally, may I remind Mr. Thacker that at least three other observers have obtained similar results to those reached by Kammerer whilst working on widely different animals—viz. Heslop Harrison on moths and Hymenoptera, Durkhem on the pupæ of the white butterfly, and finally Pavlov on the inheritance of a conditioned reflex (*i.e.* a habit) in white mice.

Yours truly,

January 22, 1924. E. W. MacBride.

SANITARY WORK IN INDIA

From E. H. Hankin, Sc.D

Dear Sir,—Your difficulties in India, it appears to me, were only partly due to the usual opposition to new discoveries. There was also opposition owing to the fact that the equanimity of the medical authorities at the time had been somewhat disturbed by Haffkine's and my bacteriological work. Despite its lesser importance, my work probably exceeded Haffkine's in its power of ruffling official feathers, as his work, at the utmost, merely accused his official superiors of sanitary sins of omission, while mine pointed to their sins of commission, which I repeatedly advertised with much indiscreet enthusiasm. Hence the following account of my experiences may perhaps be of interest to you.

When I arrived in India in 1892, it was quite unknown to me that, in any department of Government, for the sake of discipline, it is necessary for junior officials to show respect for the policy of their superiors. Should they have valid reason for believing their superiors to be in the wrong, it is advisable that they should tactfully draw their attention to the facts of the case, leaving to them the onus of deciding what should be done. For instance, a high sanitary official once told me that it would be quite contrary to official etiquette for an official—even if so highly placed as he was—to make a recommendation that contradicted an existing Government order. For instance, to quote the example he gave me, if a Government order existed that plague-infected houses should be disinfected and if you discover that plague is carried by rat-fleas and that disinfectants merely make such fleas liable to take refuge on human beings and bite them and so increase the disease, you must not recommend that disinfectants should be dropped, but only suggest that anti-rat-flea measures should be added to the existing procedure.

Tact and official reticence are not needed for carrying out researches on microbes, and my mental equipment at the time did not extend beyond what was requisite for my scientific work. My annual reports, consequently, which should have been nothing more than a statistical summary of work done, contained somewhat racy and incisive criticisms of official apathy about cholera, and even, rushing in where my official superiors thought it wiser not to tread, I made many practical suggestions for dealing with the disease, some of which are nowadays commonplace and some of which are probably obsolete. The then Director-General of the Indian Medical Service, on one occasion, implored me not to write such long reports—a

request that I disregarded—while, at the time, one of them was being reprinted and, I believe, widely circulated by Government. Not content with criticising my superiors in my official annual reports, I actually—and without asking permission—published a book entitled *Cholera in Indian Cantonments and how to deal with it: written for the use of Cantonment Magistrates, Medical Officers, and others interested in the Question.* In its first paragraph the opinion was expressed that the idea of a "cholera miasma," the then pet belief of all my official superiors, was "a view lineally descended from the old idea that an epidemic was caused by an effluvium from a comet's tail, and that it is a view that has never acquired the definiteness of a scientific conception." The book was full of statements that directly or implicitly put the authorities in the wrong in their mode of dealing with cholera. For instance, the water-supply to troops was then supposed to be adequately protected from cholera infection because it was always filtered through the Macnamara filter, an old-fashioned apparatus of sand and charcoal. This is described in my book as "an excellent breeding-place" for the cholera microbe; also it is stated that the cholera microbe had been found by me in a Macnamara filter that had been used throughout a particular outbreak no less than six weeks after the filter had been last used. "Of course by this time," my account continued, "it had lost its infectious power; but the observation is of interest, as I am sure that no cholera microbe could survive so long in a culture tube in my laboratory, at that time of the year." But the stock argument then relied on by the medical authorities for disbelieving that cholera could be due to infected water was the remarkable fact that when the disease broke out at pilgrim festivals on the banks of the Ganges, though the infection invariably reached the water, the disease never broke out in villages downstream on the banks of the sacred river, though the inhabitants habitually drank its water, unless indeed the disease was carried to the village by returning pilgrims. This argument was completely upset by my discovery that the Ganges water acts as an antiseptic to the cholera microbe, probably owing to the presence of traces of hydrogen peroxide produced by the action of sunlight.

My reports showed no hesitation in criticising my own official duties. A great part of my work at the time was the testing of water-supplies. "Why," was asked in one of my annual reports, "should one test well-water either chemically or bacteriologically when one can at once render it fit to drink by disinfecting the well?" My employment of permanganate in purifying well-water resulted in Government granting the sum of a hundred rupees for continuing the test of the method.[1] A high medical authority was heard to say that he had opposed this grant by every means in his power. The hundred rupees appears to have been a profitable investment, for, in 1914, when supplies of permanganate had been interrupted owing to the war, I was asked to find a substitute, as the interruption was a serious matter, for it was calculated that in that year, in the United Provinces alone, the use of permanganate had resulted in the saving of 36,000 lives. The only other official mention of permanganate ever made to me was by a certain Lieutenant-Governor when I made a request to be afforded facilities for getting statistics for testing the value of the method. He replied, "It is quite unnecessary, Dr. Hankin, for each of my subordinates is required by government order to have a supply, and if he is short of permanganate when cholera breaks out he has to give his reasons in writing and is liable to be punished."

Misled by a false theory, the authorities failed to take various elementary sanitary precautions in dealing with cholera. In one instance isolated

[1] The method was first tested by me in the Native State of Balrampur, at some personal risk. See "Bhowani, the Cholera Goddess," *Nineteenth Century* for October 1896.

cases were occurring among troops, although the bazaar, where cholera was raging, was strictly out of bounds. It was found by me that the sweeper had a clean broom for sweeping out barracks, etc., and another broom that he reserved for sweeping in dirty places such as latrines and cookhouses.

After several small discoveries of this kind had been made by me, I was put on a committee for investigating an exceptionally severe outbreak of cholera that had decimated the East Lancashire Regiment in Lucknow in 1894. Some of the members of the committee expected me to write a report of the stereotyped variety with details about the sandy soil, the prevailing winds, the climate, etc., the consideration of which points I persuaded them to defer till other more important matters had been examined. The first of these was the fact that E Company had escaped without a single case. We examined its colour-sergeant. "Had your company the same food and water supply as the others?" he was asked. "Yes," he replied, and one of the members of the committee looked at me out of the corner of his eyes with a suggestion only of *Schadenfreude*. Then, by good luck, the question was put by me to the sergeant as to how he could be so confident that his men had the same water to drink as the others. His reply was "Well, sir, I ought to know as I boiled it myself." Needless to say, this precaution had not been taken in the case of the other companies. The answer practically settled the question. Every detail of the evidence agreed with the view that the outbreak was derived from infected water, and we found no need to dwell on the sandy soil and the prevailing winds.

A committee was afterwards appointed, of which I was made an "additional" member, to revise the regulations for dealing with outbreaks of cholera among troops. Old regulations about marching to "cholera camps" at right angles to the prevailing wind, etc., were abolished. They were replaced by others based on bacteriological knowledge. Since then no severe outbreaks of cholera have occurred among British soldiers in India. The deserted sites for cholera camps, still to be found near every cantonment, are left empty and ready to be used as emergency landing-grounds for aeroplanes, should such a need ever arise.

When my term of service of five years drew towards an end there was some delay in reappointing me. Rumours reached me to the effect that the medical authorities wanted to replace me by a fresh bacteriologist, one more up to date and perhaps less likely to disturb official routine. Under the circumstances described above, a desire for change on their part is no great matter for surprise. But my bacteriological work was at an end. My medico-legal and chemical work increased enormously. The colony-counting test for municipal water-supplies had been introduced by me and soon was the only work connected with microbes still carried out in the Agra Laboratory.

In the meantime Haffkine had been introducing his cholera vaccin, and he followed this up by his yet more brilliant and more useful discovery of a mode of inoculation against plague.

This work of Haffkine's probably made the medical authorities awake to the possibility that Government would think they had been weighed in the balance and found wanting and, accordingly, might take sanitary work out of their hands. Hence, it may be suggested, it was, for a short time, their policy to depreciate the value of bacteriological work. When they heard of what you were doing with malaria, they were already suffering from a kind of mental anaphylactic shock, and hence they tried, with bad judgment, to nip your work in the bud. It was only later that the authorities of the Indian Medical Service started a bacteriological department, manned by their own officers, who have since tackled Indian bacteriological problems with valuable results.

Yours sincerely,

January 18, 1924. E. H. HANKIN.

ESSAYS

THE PENALTIES OF RESEARCH (Col. W. G. King, C.I.E., I.M.S.)

PROVERBS are the inspissated products of communal experience; a few are common to several nationalities, yet modifications betray the characteristics of respective peoples. One of these showing a frequent concurrence in views, though used in various forms, is that "the child is father of the man." As thus stated, there is an assumption that instincts exhibited in childhood will predominate in after-life; the terse statement leaves no hope of either their obliteration or modification. In the nursery the cry in glee or in anger—in either case in token of superiority over the rest of the rabble —"It's mine, I found it first," bespeaks the instinct of acquisitiveness, if not of inquisitiveness, as an incentive impulse. *A priori* reasoning would dictate that these primary instincts must have been possessed in an unusual degree, by men recognised as having placed milestones marking the progress of science. Thus it might be surmised that the child who, undeterred by the parental rod, ascertained the organisation of a clock, would be found in after-life engaged in eager effort to fit atoms and electrons into the scheme of Creation; or that the adolescent inquirer as to the internal machinery of a cat, would become an authority in zoology or in the art of healing. Instances are not wanting in the history of men of Science which prove that such tendencies have been as the bud to the flower; and biographers search for such proofs that their heroes were prodigies in infancy. But a comparative study of the lives of recognised "authorities," as contrasted with the average worker, would make it probable that their pertinacity in striving for success in adult life has followed control of these primary instincts, by the finer qualities of mental power elicited or strengthened by the rod of adversity, and by experience gained in differentiating right from wrong, in the course of meeting adverse criticism from their compeers. Especially has strenuous action been inspired, in spite of difficulties, by the belief that the advancement of Science and humanity are interwoven; for "tribulation worketh patience; and patience, experience; and experience, hope." Such opinions do not, however, accord with the popular conception of the fully developed man of Science; he is depicted as a being with supernormal brain power—born, not made; that he should encounter opposition from his fellow workers so acute as to be a factor in mind-moulding would seem absurd. Yet, from early times, "envy, hatred, malice, and all uncharitableness," *and*—at times—the stake, have haunted the Research-worker. In modern days, such crude accompaniments of advance are camouflaged under the term "criticism"—healthy and unhealthy; but the nursery cry—"It's mine, I found it first," persists. Indeed, the only man of whom it is recorded that he disclaimed a discovery with which it was possible for him to be credited was Adam; he was too noble to deprive his partner of the honour.

Communal psychology connoting a particular type of mentality may not be effective in guiding personal action. This becomes more readily demonstrable the smaller the community. If there be but one bauble for the nursery rabble, proprietary rights become of special import—even to the

excitement of pugnacity. Under such conditions, "patience" and "experience" have selective influence in the production of authorities.

Restrictions are found in a marked degree in the medical profession. Here there exists a body of men whose communal and personal actions are subject to caste rules and prejudices which date back to Hippocrates, and are as strict as those which to-day govern the Hindu.[1] In more or less modified form, the oath prescribed by him is still administered by certain bodies when conferring degrees; and whether the medical man of the present day has so formally pledged his honour or not, the moment he becomes a member of the medical profession he remains within the pale of a caste subject to traditions, the ignoring of which leads to either social or actual excommunication. The oath of Hippocrates, whilst seeking the commonweal of the profession by rules that would safeguard its honour and devote its members to the cause of humanity, demanded that the interests of the patient should be paramount, and that accretions of professional knowledge be not exploited for the benefit of the discoverer. In a paper recently read before the Royal Society of Medicine by W. H. S. Jones, M.A., the bearing of medical ethics on the rights of discovery is thus stated: "Nowadays all discoveries in Surgery and Medicine must be made public property. New surgical methods or an improved vaccine must not be patented." There is here referred to a fact very little known to the general public and where known its wide influence is not usually appreciated; namely, that whilst to the Research-worker in other branches of Science it is possible that personal efforts may bring him not only fame but money-producing inventions protected by patent, this is forbidden to the medical man not by the laws of the country but by the caste and ethics of his profession. The discoverer in Medical Science has the satisfaction of knowing that years of toil (which in other work would have brought him and his children a competence) have been of benefit solely to his fellow-beings, and, as for himself, there remains the possibility of a niche in the Temple of Asklepios. The niche, however, is neither protected by the Tenth Commandment nor by a patent, and until he has met in wordy combat such of his compeers as echo the nursery cry "I found it first," or attempt to minimise the importance of his discovery by pointing to the historical fact that I-em-Hetep (3500 B.C.) came to the same conclusion but, unfortunately, had no leisure to prove it, he may not be granted even a niche.

The well-known cases of Vesalius, Harvey, Pierre Brissot, and Ambrose Paré afford instances, in early days, when opposition by the profession as a body was of the unhealthy variety. Differences between individuals on professional opinions sufficed to bring about a duel in Jamaica (1750). A riot by respective student advocates of diverse doctrines occurred in Göttingen (1802), which was of sufficient asperity to require cavalry to terminate proceedings. In the present day, when a direct assault upon the claims of a discoverer has failed, he may still be required to run the gauntlet of dogmatic contradictions culled from "principles" or hypotheses laid down by accepted sages. This offshoot of hero-worship is a marked peculiarity of medical men, which finds an analogy in the respect paid to the teacher under the laws of Manu as well as the oath of Hippocrates.

Again, there is the ever-recurring danger of discouragement of young men at the hands of senior experts in the particular work where they threaten intrusion; here the cold water of dogmatic assertion has frequently quenched finally the fire of enthusiasm.[2] In the case of Semmelweiss, who, under the

[1] The "panchayet" has its analogue in the General Medical Council.

[2] Contemptuous treatment of investigation is not confined to one profession. Pure Science has shown itself absurdly intolerant to study of that still imperfectly understood "force" which Crookes entitled "psychic"; yet, that its existence is capable of demonstration, there

strain of opposition, ultimately became insane, the striking result of his empirical advocacy of asepsis in midwifery was treated by medical men "either with indifference or self-satisfied superiority or with the hostility of insulted dignity"—an attitude which probably cost the lives of many hundreds of women pending Lister's great work.

So far have been outlined difficulties which trammel the Research-Worker in medicine, at the hands of his compeers. The resulting uninviting picture of human traits is, however, relieved in practice from dark shades by the fact that, whilst wrangling *ad libitum* is permissible within the profession, the defence of its members or of its traditions from attack by outsiders is loyally of the *nemo me impune lacessit* variety. There remain, however, difficulties which beset research of greater import than those alluded to, and for which outsiders are responsible. These are removable by that great body—the public—the nation—for whose benefit discoveries (unprotected by patents) for the relief of suffering and disease prevention have been freely given by medical men. It has been forgotten that the education of a medical man represents capital sunk with the object of utilising the knowledge acquired for the mundane purpose of earning his daily bread by methods limited by caste ethics; and that research represents, in time, labour, and material, sacrifices of the wherewithal for that necessary nutriment—irrespective of that not infrequent risk of contracting the disease over which a victory is attempted.

Opposition frequently emanates from lay authorities in charge of public funds who pride themselves on being *practical*, and regard research on behalf of either Curative or Preventive Medicine and the application of their teaching for communities under their administration as expensive "fads," which can safely be ignored in their budgets. It was in this spirit the War Office of the period caused troops in the Crimean War to attempt existence on a diet which Professor Christison showed was of less nutritious value than that used, in time of peace, in garrisons in Great Britain; and that the Government of India, against the strenuous protests of the late Surgeon-General Cornish, C.I.E. (when Sanitary Commissioner for the Government of Madras), ordered a standard diet of one pound uncleaned grain per head for adults of a famine-stricken population. Wolseley's conception of the importance of Preventive Medicine in ruling that his Sanitary Adviser should be at the base of operations had a like origin. Pirogoff, in the Crimea, and Lieutenant-Colonel Carter, C.B., I.M.S., in Mesopotamia, had each a "bad time" in combating authorities of like mentality. It was the practical genius of a body in charge of public funds for the upkeep of a large camp, during the late war, which caused it to spend, by insertion at the head of a sound water-carriage sewerage system, chloride of lime at the rate of over £800 per annum, and to launch the mixture on modern sewage filters where microbic action was essential. They ultimately scrapped the expensive filters and sewage disposal area (which justifiably refused to yield vegetation), and spent thousands of pounds on a substituted scheme for pumping the sewage to a new point of discharge. The man who solved the simple problem had the usual reward of a "bad time."

The practical man, as an individual, may have bigoted views which he may defend till shelled out of his stronghold; but the politician wittingly defends crooked views held by others, with the camouflage of sincerity. Only under crushing difficulties, such as great national calamities, is a communal

can be no doubt whatsoever. In the presence of the inflexible scepticism of his compeers in Science, a man who could be trusted to identify Thallium, and to have brain enough to be President of the British Association, abandoned research on the subject, "that he might work where prejudice did not assail him."

mentality of a higher type elicited. This was evident during the recent war. The politicians of that day suddenly discovered that research in Chemistry was of value to the nation, that the physicist could provide data for offence and defence, and that allied Curative and Preventive Medicine formed the backbone of the fighting efficiency of our Navy and Army; they also realised that the men who gave their experience born of infinite toil required a living wage, and that materials for investigation cost money. They advanced even so far as to understand that the discoverer of facts of value to the nation deserved a more durable token of gratitude than the thanks of the bureaucrat, in the form of drafts on the funds of the public served. Hence, an Inventions Board. Amongst other awards have been £20,000 for an explosive bullet; £7,000 for a patent-bullet; £2,500 for a process of "silvering" a mirror; £500 for improvements on roll and depth recorders, etc.

Realisation of the importance of the application of Science, in time of war, gave hope that the benefits on the health and economic conditions of the country would not fade from memory, in peace. But politicians who were able to provide millions of pounds for unemployment without arranging for any form of labour test, and met a disease in cattle by a slaughter policy at a cost of further millions and, by way of economy, "axed" the few thousand pounds for the mastery of which research had been sanctioned, showed their appreciation of the value of Medical Research to the country by placing at disposal, in 1922–3, the sum of £130,000 to cover the requirements of (*a*) clinical medicine; (*b*) mental disorders; (*c*) industrial fatigue; and (*d*) pathological inquiries. The Report on the work attempted states that investigations could not have been undertaken, unless the sum supplied by Government had been augmented from other sources—private and quasi-public. Not only was this the case as to funds but, according to Lord Salisbury, there were utilised in other departments the services of scientists "willing to work for the Government at far less than would be the remuneration of their great talents, and are willing, through Government, to place their knowledge at the service of the community." *Nature*, in a leading article, stigmatises this as a "deplorable statement," "which baldly claims the mercenary advantages of a one-sided arrangement." It certainly is a statement that should suffice to make a member of the late Inventions Board blush for his country.

It is apparent, therefore, that the average medical man who devotes himself to Research does so at the present time in the knowledge that, in the "mercenary advantages of the one-sided" arrangement of financial authorities, he may find "experience" in philanthropy and impecuniosity, which may develop "hope" of the variety that maketh the heart sick. It is at this critical stage in development that the Simian instinct of pugnacity attains the refined quality of pertinacity, which, as above suggested, has been exhibited in a special degree by acknowledged authorities, whose work has been inspired by the belief that the advancement of science and humanity are interwoven. This characteristic is certainly demonstrable in the case of Jenner, Lister, and Ross, whose labours, in the last one hundred years, have placed within the power of man the defeat of small-pox, sepsis, and malaria, and thus have saved from death and suffering, not the people of a single country, but the masses of the world at large.

Jenner, in a letter to his friend Dr. Gardner (*Life of Jenner*, by Baron, vol. i, p. 267), says: "My experiments move on, but I have all to do single-handed—not the least assistance from the quarter where I had the most right to expect it! Bodily labour I disregard, but pressures of the mind grow too heavy for me. Added to all my other cares, I am touched hard with the reigning epidemic—impecuniosity." In another letter he states (p. 140): "Whilst the vaccine discovery was progressive, the joy I felt at the prospect of being an instrument destined to take away from the world

one of its greatest calamities . . . was often so excessive that, in pursuing my favourite subject among the meadows, I have sometimes found myself in a kind of reverie. It is pleasant to me to recollect that these reflections always ended in devout acknowledgments to that Being from whom this and all other mercies flow." Lister, from his first contact with surgical duties in wards, was filled with profound pity for the ghastly mortality he witnessed. He declared, at a dinner of the Royal Society when receiving the Copley Medal, that "perhaps his chief merit lay in the fact that he worked for years together with exceedingly little encouragement from his professional brethren. There were, however, two great exceptions, his father-in-law, James Syme, and his students." In his Memorial Discourse concerning Lister, in 1912, the Rev. Wallace Williamson, stated (*History of Medicine*, p. 636, by Garrison), "His was the grave and thoughtful courtesy which bespoke the Christian gentleman and the earnest lover of his kind. . . . That such a man, dowered with God's gift of genius, should rise to lofty heights and achieve great things was inevitable." Ross, in his *Memoirs*, recently published, shows that the underlying motive of his effort to solve the problem of malaria was the hope of saving the people of India from the suffering, poverty, and death due to that unconquered disease, which he witnessed on first joining the Indian Medical Service. He found scope for his poetic tendencies to record much of his daily thoughts and actions in metre. This is how matters struck him in 1890 (*Philosophies*, p. 21) as to Indian fevers:

> "The painful faces asked, can we not cure?
> We answer, No, not yet; we seek the laws.
> O God, reveal thro' all this thing obscure
> The unseen, small, but million-murdering cause."

And, on the day of discovery (p. 53) he wrote:

> "This day relenting God
> Hath placed within my hand
> A wondrous thing; and God
> Be praised. . . .
>
> I know this little thing
> A myriad men will save."

The benefit derivable from the discoveries of these three authorities is evident. Putting aside the well-acknowledged protection acquired by vaccination in the individual, it has been abundantly proved that whilst there exists a distinct type of small-pox known as amaas, alastrim, or "mild small-pox," in which no exacerbation of virulence has been proved, strains and substrains of the virulent form undergo attenuation, amidst other factors, in accord with the grade of diffuseness of vaccination in communities. Statistics show that malaria is heavily incident through huge areas of the world. Nations, as warlike powers, have been broken by it. Agriculturists have been decimated. The work of Ross has placed it within the power of representative public men in the areas concerned to relieve their people of this cause of mortality and grave labour disability. That the conquest of sepsis is of importance throughout the globe is also self-evident; for, where the saving of life and relief of suffering depends upon the wielding of the surgeon's knife, sepsis must be prevented. This Lister rendered possible.

If, therefore, the nation believes that "health is wealth," its existing

habit of placidly accepting, free of patents, discoveries gained by the toil of a sentimental caste—bound by archaic ethics no other human beings adopt—fulfils requirements. It suits admirably the politician's conception of political economy. Has it not, moreover, the merit of imitating closely Nature's mode in evolution of higher vertebrates—where fishes from the vasty deep are confronted with difficulties from which they escape only to find others on dry land? A close imitation, certainly; but with Nature time was not measured at Greenwich, and it was no waste to secure continuity of effort by providing millions of ova for replacement of failures in pertinacity. As stated by Sir Charles Sherrington, at a recent anniversary dinner of the Royal Society, in reference to advance of Science generally, "there must be a reasonable competence for those who have shown a capacity for original investigation"; and it may be added that, in the absence of exercising the right of patent, this dictum is essential to the Science of Medicine. Of course, the practical man who fails to understand that "within human limits health is purchasable" but would advocate the buying of rights by the nation of a special bullet or chemical, may find no economic analogy between these tangible objects and paying for results in medical research, of which the value may be merely potential. That, however, exists.

The connection between sickness, labour disability, and economic conditions of communities requires no argument. Lieutenant-Colonel Freemantle, M.P., recently showed that, under the influence of Applied Hygiene (the offspring of Medical Science), expectation of life has so increased as to place at the disposal of the nation a potential output of labour to the value of £200,000,000, against a still recurring loss by sickness of £150,000,000 *per annum*. No one who has witnessed the horrors of deaths by hydrophobia, tetanus or diphtheria, would haggle as to the monetary value of the vaccine of Pasteur and the anti-toxin sera of Behring and Kitasato; nor would one who has seen the devastation of troops and populations by typhoid, cholera, and plague regard public funds as wasted in purchasing the vaccines of Sir Almroth Wright and of Haffkine. These men might well have been millionaires under the protection of patents. When a section of the public in England realised the great benefits of vaccination, an effort was made to secure for Jenner a monetary award from Parliament. Witnesses before the Parliamentary Committee which investigated the claim stated that Jenner had not only given his skill and labour through many years, but, in doing so, had sacrificed his private practice, whilst had he patented his discovery he could have earned at least £10,000 per annum; and that the loss to the nation by small-pox was at the rate of 45,000 lives yearly at the cost of £100,000. The Government gave grants aggregating £30,000; but to Grattan it gave £50,000, in recognition of his "patriotism"! Lister had private means (irrespective of the necessity of adhering to his practice in giving scope for his special research) with which, through years of anxiety and toil, early and late, he met the cost of his own experiments. The relief to suffering humanity, in peace and in war, following the adoption of the broad truths of Listerism has been indescribably great, and the monetary saving, owing to decreased period of detention of surgical cases, must imply millions of pounds. Sir Ronald Ross conducted his experiments at his own cost. Confronted with the refusal by the Government of India to promise a trial of practical application of his discovery, he retired prematurely from the Indian Medical Service on a minimum pension—a matter which, if applied to the multiplication of years since that period, must represent, throughout his life, a heavy sacrifice. Had he not had the resolution to face this consequence, in the hope of demonstrating the applicability of anti-malarial measures to *communities*, there is reason to believe this discovery would for an indefinite time have been regarded merely as one of abstract Science. India, which has but fitfully used anti-malarial measures in the midst of the general population,

loses not less than one million inhabitants per annum from malarial fevers, coupled with an unproductive cost of (at a minimum) £20,000,000; but, following the efforts of the medical staffs attached to British troops, in the light of Ross's work, there occurred a rapid fall of admissions[1] for these fevers between 1899 (the year succeeding the discovery) and 1914—a period beyond which war factors are exhibited in consequence of which India must have saved enormously in expenditure,[2] owing to less detention in hospitals and invaliding. In the Near East during the late war, anti-malaria measures undertaken by the Medical-officers (although trammelled in completeness and therefore in results by active service conditions) contributed greatly to the fighting efficiency of our troops, in happy contrast with those of the enemy, in the same areas where like preventive measures were much neglected.

In the absence of anti-malarial measures, construction of the Panama Canal would have been impossible without extraordinary sacrifice of human life. Gorgas, in contrasting the American with the French effort, where no such advantage was available, estimated that a saving of 71,370 lives had been secured. Tolls to the value of £300,000 per month are now being obtained from the completed work. Malcolm Watson has proved that in Malaya a saving of 132,000 lives has resulted. In West Africa (formerly known as the "white man's grave," for sound reasons), the lives of European residents in areas where anti-malarial measures have been effected are as good as those in Great Britain, whilst, in the case of missionaries, who are exposed to conditions beyond these areas, mortality is nearly four times above normal.

If, then, "within human limits health is purchasable" and discoveries have a monetary value, why should the nation have the pettiness by "mercenary one-sided arrangements" to absorb the results of Medical Research without making due return? To appoint a Medical Research Council and make it dependent on casual outside contributions for many of its important duties, is a travesty of requirements. Moreover, whilst valuable team work confidently may be expected from the highly qualified experts of that body, the method neither meets the necessity of early aiding the young Research-worker, nor of rewarding the independent original worker, by money grant or pension. Canada, in the case of Banting, and France, in the case of Madame Curie, have shown gratitude by conferring life pensions. Germany, on its "according to plan" mode of pre-war days, largely owed its national prosperity to its guarantee of support for themselves and families of Research Chemists, and by the same method obtained the work of Koch, Zeiss, and Bayer.

Either the medical profession, in its own interests, should modify its ancient ethics as unsuitable for the present age; or the nation should mitigate difficulties which beset the evolution of Research-Workers in all branches of Science directly or indirectly concerned with the well-being of the people and the economics of production, by making adequate maintenance, suitable monetary awards in special cases, and liberal pensions—matters of certainty. The system adopted should require *the Research-worker be searched for* as an asset of value—not be left the sport of chances. The money thus sunk by the nation would represent a gilt-edged investment, with mercenary advantages not one-sided.

[1] The average duration of detention of British soldiers in India per case of malarial fever has been officially recognised as 11·77 days. The rate of admission of these troops under this head per mile, per annum, was 374 in 1898, 244 in 1908, 152·5 in 1910, and 117·2 on the average of the period 1910–14.

[2] In the Mediterranean garrisons, the subduing of undulant fever following the discovery of the *Micrococcus melitensis* by Sir David Bruce has also resulted in great economy under the same heads of expenditure.

THE DEVELOPMENT AND PRESENT POSITION OF COLOUR-VISION THEORY (Prof. W. Peddie, D.Sc., F.R.S.E.)

1. Colour vision is one of those subjects, the most interesting of all, which lie upon the borders of at least two great branches of knowledge. In its case the interest is more than usually great, for it is related unavoidably to three main fields of inquiry. Physics, physiology, and psychology have each to contribute to the solution of its enigmas, and the theory cannot attain to final completion until these sciences are in full agreement in regard to their data and their conclusions; for the complete solution must be unique.

The beginnings of knowledge regarding it lie far back in the centuries, hidden amongst the attempts to solve the practical problem of determining the least number of pigments which could serve for the pictorial representation of the manifold of colours exhibited throughout nature or in art. The former inquiry indeed could scarcely arise until after the discovery, definitely arranged for and made by Newton, of the coloured constituents of white light, and the production of white light by their recombination.

2. The next great step lay at hand in the formulation of the law of recombination. In taking this step Newton asserted his famous linear rule: Presuming that, in a suitable geometrical colour scheme, the positions of the various spectrum colours are mapped out, if masses proportional to the quantities of each component present in a compound light be placed at the corresponding loci, the locus of the resulting colour is at the centre of inertia of these masses. The mechanical illustration is used merely because the same linear law is obeyed in the case of colour mixture, and its high accuracy has been fully verified.

3. That step being settled, Newton proceeded to consider the question of the smallest number of colours which, by their admixture in suitable proportions, could produce a match to any observed colour. He said: "I could never yet by mixing only two primary colours produce a perfect white. Whether it may be compounded of a mixture of three . . . I do not know, but of four or five I do not much question but it may." He was very evidently seeking for the simplest condition; and, as Dr. Houston has pointed out, he actually gave a drawing in which three only were em-

these three steps taken by Newton the foundations of the modern theory of colour vision were laid deep and strong. Modern work has only confirmed them. And the form of his linear law of mixture gives at once the explanation of the variety of colours, spectral and non-spectral, which may be selected as primary for the purposes of colour mixture.

4. The adoption of three colours only as primary or fundamental in vision was first explicitly made by Young. Up to that stage inclusive, the development of the theory was on purely physical lines; and the results have been so fully confirmed by spectrum observations that this part of the theory has ceased to be theoretical. It was theoretical when Young initially advanced it; but the accurate quantitative measurements, first made by Maxwell and then by Helmholtz and his school, established it as a reality. As Helmholtz said, "That one finds three fundamental colours sufficient contains certainly the recognition of the fact that the quality of coloured light is a function of only three variables." Yet the entire unavoidability of this conclusion is not even now realised by some of the non-physical critics.

The linearity of the law of mixture carries with it the conclusion that the results of colour mixture can be expressed in terms of any greater number of fundamentals; but the fact that three are sufficient shows with certainty

that even one more is redundant. In other words, *we have the knowledge that there is no independent fourth organ in the mechanism of colour vision.*

5. Theoretical physics deals with the formal laws of colour vision, and practical physics seeks to establish their accuracy. These formal laws may, or they may not, impose conditions on the mechanism which obeys them. In colour vision they do impose the condition of a secure triplicity of inter-independence.

Now, the mechanism is complex. It consists of a retinal portion receiving the stimuli, a connecting optic nerve bundle transmitting the resulting action to the brain, and the brain mechanism itself, which seems to be of considerable complexity, wherein the physiological action finally originates sensation. And the triplex domination may occur at the eye end of the system, or at the brain end, or partly at the one end, partly at the other. In any such case, while actual knowledge regarding the structure and the nature of the mechanism is wanting, the process of supreme importance to the development of knowledge regarding its mode of action, after the establishment experimentally of a few definite laws or conditions, is the logical elucidation of the necessary consequences of these laws or conditions. If they are deduced without any further postulates, they must be in accordance with observation if the basic laws and conditions are rigidly or even just sufficiently correct. If fresh postulates are adopted in order to make possible the process of deduction, an experimental confirmation of the results is rightly regarded as a justification so far of the additional assumptions. In this way it is possible to attain to knowledge of phenomena while there may still be entire ignorance of the nature of the mechanism. *The results are true whatever be the structure in connection with which the phenomena are manifested*, provided only that it be subject to the previously ascertained and utilised laws. This procedure was followed strictly by Helmholtz in his great development of the trichromatic theory.

In addition to the Newtonian law of colour mixture, firmly based upon experiment, and to the condition of trichromasy, acceptance of which was also compelled by observation, one other observational law was required before progress could be made. This was the so-called psychophysical law of Fechner, which gave the relation between the intensity of the light incident upon the eye and the magnitude of the sensation evoked. It asserted that the sensation increases in geometrical progression as the intensity increases in arithmetical progression; and it was found to hold throughout an enormous range of intensity. Fechner himself gave the modification necessary at very weak illuminations, and Helmholtz used it in order to take account of the self-light of the eye. Helmholtz also took account of the modification necessary when the illumination was so strong as to produce dazzle. His entire subsequent logical development of the trichromatic theory was based only on these verified postulates, and is true in so far as the verifications extend whatever be the actual triplex mechanism involved.

6. One main problem to which he applied the theory was the determination of the absolute fundamentals. Fechner's law, applicable to light of any one wave-length, is of non-linear form, and therefore may furnish a discriminating test. How are we to express the change of sensation which occurs when the proportions of the stimulations vary? The answer was given by Helmholtz in an amazingly simple manner, as so often happens in the apparently effortless efforts of genius.

The matter may be put in this way. The specification of any coloured light requires a statement regarding intensity and colour. That is to say, it involves a statement of magnitude and also one of quality. But this is the characteristic of what is called, in physics, a directed quantity. Therefore the law of composition of colour may be expected to be that of all

independent directed quantities. If so, it may be thus stated: the square of the change of the resultant sensation is equal to the sum of the squares of the changes of the three component fundamental sensations. Helmholtz gave full reasons for adopting it, and pointed out that it was the simplest assumption that could be made. Nevertheless he recognised the necessity for testing it. He deduced from it the expression for the differential sensitivity in passing from one wave-length to another, when the tests were made under the condition that the change of sensation was a minimum; and he applied it to the direct observations of König and Brodhun on the sensitivity. The correspondence of the theoretical and the observed data was remarkably good. Hence the postulate that the three independent components are to be compounded according to the law of geometrical addition must be regarded as at least a good first approximation. And Helmholtz's absolute fundamentals, determined so as to get the best correspondence, must also be regarded as a good first approximation.

7. Another main region of application of the theory is that which deals with peculiarities of colour vision, ranging from abnormal trichromasy to dichromasy and monochromasy. The simplest assumption which will account for dichromasy is that one fundamental fails to be excited. This was the idea initially put forward by Young, and followed by Helmholtz, as being the most simple. Whenever it was found that, in examples of one-eyed dichromasy, this most simple view was not suitable, Helmholtz stated the general case of a linear relation connecting the three fundamentals, so that only two were independent; and he showed that a particular case of this general case was sufficient to explain the examples referred to. The full general case is far more than sufficient to account for all known examples of colour blindness of any type. Yet many, even recent, criticisms seem to have been made without realisation of the full meaning of Helmholtz's half-century-old work.

There is no limitation of colour blindness to three possibilities. There may, so far as theoretical necessities go, be actually, within limits, a triply infinite series, with continuous variation from monochromasy to full normal trichromasy. Most probably nature imposes restrictions upon the wealth which the theory admits.

8. In the preceding sections, the points discussed lie entirely apart from any question not only of the mechanism which is involved, but also of the physiological actions which occur. Amongst them may be included all questions of contrast and of after images. In fact the whole range of colour-vision phenomena can be taken account of in a statement, preferably in mathematical terms, of the formal laws which regulate the phenomena; and in the application of the strict logic of mathematical methods to the elucidation of their consequences.

This formal theory of colour vision is *unique*. There cannot be two such theories, for the field of inquiry is unique. As already remarked, its main groundwork, having become established fact, has ceased to be theoretical.

9. Now, in the development of physical theory, in the absence of actual knowledge of the mechanism and its mode of action, it is often of advantage to postulate a mechanism for illustrative and even for deductive purposes. In this way it may be possible to discriminate amongst types of mechanism in regard to validity. This procedure was freely used in the development of the theory of colour vision. Young postulated the existence of three sets of nerve fibres. He carefully stated that this view was the simplest and might be adopted until direct evidence against it were found. That is to say, a triple set of fibres, communicating with three independent brain centres, is not of the essence of Young's theory. Its essence is trichromasy.

Similarly, Helmholtz, in developing the specification of Young's

mechanism, introduced for the first time the idea that there may be three chemical substances present in the retinal mechanism, and that the nerve stimuli may be due to their decomposition by the action of light, the different substances responding most powerfully to light belonging to regions of respectively different wave-lengths. The visual purple (rhodopsin) is known to be decomposed by the action of light and to perform an essential function in vision, though views differ in regard to the nature of the action. But no direct evidence as to the presence of the three substances has ever yet been found. And, in discussions regarding colour vision, this fact has been repeatedly spoken of as furnishing evidence against the trichromatic theory, just as in the other case of the absence of evidence as to the existence of three sets of fibres.

In fact Helmholtz's photo-chemical hypothesis is frequently confounded with the trichromatic theory. Yet he most explicitly warned his readers that his postulate was introduced purely for illustrative purposes and was in no way essential to that theory. Nevertheless, he used the illustration very searchingly in his discussion of dichromasy and colour abnormalities. He used it, indeed, to enable a non-mathematical mind to apprehend the meaning of the linear substitutions which the linearity of Newton's law of colour mixture made possible amongst sets of fundamentals. He also agreed with Young as to the non-essential nature of the postulate regarding three sets of nerve fibres.

10. Any view put forward regarding the nature of the visual processes must take account of the fact of trichromasy before it can be regarded as complete. We cannot have a trichromatic physical theory and a non-trichromatic physiological theory. A non-trichromatic theory may deal completely and well with various visual and physiological conditions so long as the question of chromatic manifoldness does not arise. But, before it is entirely complete, it has to join hands with the established physical laws.

It is quite a natural thing that investigators on the physiological side should deal only with the visual appearances and base their discussions and conclusions upon these. Yet choice in that way varies.

Amongst all the theoretical views one alone stands out as being more fundamentally in opposition to the trichromatic view than all the rest. That is Hering's theory, which was originally advanced by him as a hexachromatic theory. But Helmholtz long ago showed that it is really trichromatic only, the difference between it and Young's theory being merely that, whereas in the latter positive stimuli alone are postulated, in the former negative values also are assumed. That is to say, three variables only exist, though both signs are attachable to them. It seems that the consensus of physiological opinion is now strongly against the existence of this type of action.

The theory of Mrs. Ladd-Franklin asserts tetrachromatism corresponding to a postulated set of four decomposition products of a single photo-chemical substance which itself gives rise to the sensation of white. Its first two decomposition products give rise to the complementary sensations of yellow and blue. A further decomposition of the yellow-exciting product gives rise to two products which excite the red and green sensations. Thus the so-called red and green "physiological" fundamentals are taken as compounding to yellow. The whole hypothesis is less simple than that of Helmholtz. It may correspond to actuality; but, if so, the observed Newtonian-law trichromasy could only be explained if there exists a linear cerebral connection amongst the four centres. It accounts for the presumable order of development of colour sensation from white; but so also can Helmholtz's view: for, if there are three latent sets of cerebral connectors proceeding from the retina in the original white perceptive stage, a subsequent isolation of the blue connector alone would originate yellow-blue dichromasy;

and a farther isolation of the red and green connectors would give trichromasy with no further postulate.

Other physiologists discuss six or more "physiological?" fundamentals, and do not take into consideration the three or more linear interconnections which must exist somewhere in order to give effective trichromasy such as is observed. Dr. Edridge-Green considers that a single impulse, dependent as to its nature on the wave-length, is propagated from any retinal centre, and that the brain responds similarly. There is nothing inconsistent with the physical theory in the assumption of a single nerve impulse proceeding from the retinal centre. But in that case triplicity of analysis must be provided at the cerebral centres. Dr. Edridge-Green recognises this, and so asserts that, whereas the brain responds to the whole gamut of wave-lengths, it is too defective to appreciate the distribution of energy in the spectrum except in so far as it is fixable by the values occurring at three suitably chosen wave-lengths. But this statement merely confesses ignorance as to the mechanism by which it is possible for the consciousness to lay hold upon the three suitable fundamentals. Whenever the mechanism can be described, should this view be otherwise correct, it will become the trichromatic theory. But many physiologists feel it to be inadequate.

The situation with regard to other views is precisely similar. This is so even in the case of the quantum theory, recently put forward by Joly in a remarkable paper published in the *Philosophical Magazine*, which gives a possible explanation of the origin of the quantal nerve impulses now made evident by the physiologists. So also Houston's mathematical investigation (illustrated experimentally by Barton's model) requires a mathematical continuation giving a formulation of the process of resolution into a variously selectable triple set of fundamental actions.

The duplicity theory, as is well known, is in no way opposed to the trichromatic theory. It merely deals, in so far as its essence is concerned, with observed differences in the mechanism at different parts of the retina. Recent work tends to show that the differences are quantitative only, and not really qualitative.

11. Experimentally, the present position is one of necessity for data enabling us to express the quantities a and x_0 in the Fechnerian expression

$$S = \log \frac{x + a}{x_0}$$

for the sensation arising in connection with the absolute fundamental of type x. Here a is the external equivalent of the self light, and x_0 is the corresponding threshold value. Three quantities of each kind have to be expressed as functions of very many variables—intensity and its duration, precedent illumination and its duration, their areal distributions, interconnections of effects, although the three final transformations into consciousness are independent, and so on.

This actually existing great complexity of secondary variables has contributed to the tenure of the erroneous idea that three ultimate independent sensations are not, or even could not, be sufficient for colour vision. The solution of the difficulty is that the three fundamental colour variables are themselves functions of several physical and physiological variables. And the future of colour-vision theory lies in the determination of the form and parameters of these functions. Amongst recent work contributing to this end, that of Prof. Frank Allen is specially noteworthy, for it indicates with some certainty a type of action exhibited by the mechanism.

ESSAY-REVIEW

LINNÆUS. By E. M. Cutting, M.A., being a review of **Linnæus (afterwards Earl von Linné: The Story of his Life.** Adapted from the Swedish of Theodor Magnus Fries, Emeritus Professor of Botany in the University of Uppsala, and brought down to the present time in the light of recent research by Benjamin Daydon Jackson. [Pp. xiv × 416, with 12 illustrations.] (London: H. F. & G. Witherby, 1923. Price 25*s*. net.)

Whatever may be the final judgment on the work of Linnæus, there can be no doubt that his work has had an immense influence on the science of Botany. His name is commemorated not only in connection with his well-known system of classification, but also in the names of many Natural History Societies scattered over the civilised world. This volume is primarily the outcome of the work of Prof. T. M. Fries, who devoted thirty years of his life towards the pious task of collecting materials for his monumental life—*Linné*. These materials were gathered partly from manuscripts in the possession of the Linnean Society of London, a portion of the collection bought by Sir E. Smith after the death of Linnæus and of his son, and zealously guarded in the Society's rooms in London, and partly from "letters and documents dispersed over the whole learned world." These were consulted with a view to correcting the many errors that had crept into the existing biographies. The present book is a translation and adaptation from the Swedish of Dr. Fries by Dr. Daydon Jackson. One can imagine no one better suited for the task, and, except that the work is too obviously derived from a foreign language, it must be regarded as a complete success.

Linnæus was born on the 23rd of May, 1707, the descendant of simple farmers and priests. His father's surname, we are interested to note, was derived from a famous lime-tree which was regarded with almost superstitious veneration by the neighbours. Ill-fortune, it was thought, would surely befall those who removed even a twig from the tree, and if one of the three main branches died, the corresponding family would die out. Tiliander and Lindelius were the names of the other two family branches, and their surnames also were taken from the sacred tree. Both of his parents showed a strong love of nature, and they awakened in the young Carl Linnæus a similar love. They wished that he should become a priest, as his father was, and it was a severe disappointment to them to learn in 1726, just before he left school, that he was declared to be utterly deficient in the subjects indispensable for this career. Nor did they find any great pleasure in the expressed opinion at that time by a friend, Dr. Rothman, that their son would become a famous doctor, as monetary difficulties were in the way of this project. Dr. Rothman undertook to give him private lessons in physiology, etc., and studied with him Boerhaave's *Institutiones Medicæ* and Valentin's *Historia Plantarum*. In this way he was introduced indirectly to Tournefort's method of classifying plants.

From school he went to the University of Lund, but the teaching was on the decline and the greatest influence here was not the academic training

but that of Dr. Strobæus, who, noting his diligence, offered him every facility for studying, and remained his friend afterwards when he removed, at Dr. Rothman's advice, to the University of Uppsala. The teaching and accommodation had, however, greatly deteriorated since Dr. Rothman had been a student here twenty years before. The two chief professors under whom he studied were getting on in life and were unable, through pressure of affairs and for other reasons, to give the necessary courses of study. A now well-known paper of Linnæus's upholding Vaillant's view on sex in plants attracted the attention of one of these and induced him to offer to Linnæus his first appointment, that of instructor in Botany.

His first lectures were a great success. Modelled on the practice of the day, they consisted of descriptions of the virtues, medical and otherwise, of the plants selected for the discourse, with etymological remarks and anecdotes from classical authors. By the money raised from this post, from scholarships, and from coaching, he maintained himself at the University and during this period undertook many journeys in Lapland, Dalecarlia, etc. These travels were in regions practically unknown scientifically, and were more full of dangers and more difficult of accomplishment than we could imagine possible. On them very valuable collections and observations were made, and Linnæus was prepared for the work which he accomplished on his European tours. These travels, which lasted for three years, were begun on Wallerius being promoted to the post of Adjunct, so that Linnæus had to give up his post as deputy to the Professor. It is thought that Rosen, who afterwards was his colleague at Uppsala and with whom he exchanged professorial chairs, brought about the promotion of Wallerius through jealousy, and Linnæus himself seems to have been of this opinion. This charge is, however, quite clearly refuted in this volume. He left Sweden a comparatively unknown student; he returned a celebrated and much-esteemed man of science.

The universities of Sweden at this time did not grant a degree in the faculty of medicine, and one of Linnæus's special objects in leaving home was to take his doctor's degree, the other being the hope of finding publishers for the many works he had written and had been unable to get printed in Sweden. After visiting Hamburg he took up residence at Harderwijk, where he entered as a student of medicine. This university was chosen partly because many Swedes had studied there and partly for economic reasons, for long residence was not required. He entered as a student on the 7th of June, and on the 24th was presented with his degree. Thence he travelled to Leyden, making here the acquaintance of the well-known naturalist Gronovius, who, on seeing his *Systema Naturæ*, offered to publish it at his own expense. A common friend, Isaac Lawson, joined in the same request. The joint offer Linnæus accepted. He also gained a still more eminent and influential friend in Herman Boerhaave, the chief medical man in Europe at that time. Boerhaave offered to send Linnæus to the Cape at his own expense to collect plants for two years for the University Garden in Leyden, promising him the status and emoluments of a Professor on his return. This offer was refused. Olof Celsius suggested that Linnæus should visit Dillenius in England instead.

Before the visit to England he was introduced by Boerhaave to a rich and eccentric Englishman named Clifford, who had extensive gardens at Hartecamp, between Leyden and Haarlem. He acted for some time as Clifford's physician and as the director of his gardens.

During the writing of his *Flora Lapponica* and the *Genera Plantarum*, he acquired a strong desire to visit England and see the large museums and possibly find new plants for Clifford's garden. The latter readily gave consent, and in 1737 Linnæus set sail for London. Here he and his views on classification were generally very well received. Dillenius and a few

other botanists at first regarded them with suspicion and were inimical to them, but in time succumbed. After his return to Sweden he was appointed a Professor in the University of Uppsala.

The artificial classification introduced by Linnæus was superior to other systems in that it was capable of being easily used and allowed of order where before chaos had reigned. His descriptive terms, which were very accurately defined, his acute powers of observation, and his concise diagnosis had a very great educative value. The binomial system introduced by the Bauhins in their *Pinax* and put into general use by Linnæus also assisted in the advance of systematic botany. It is difficult to uphold the claim that he changed all branches of botany. In some branches of the subject the authority of his name upheld ideas that were incorrect; but it was the followers of Linnæus who were to blame for this attitude. Linnæus, as is well known, did not regard his *Sexual System* as an end, and before his death was engaged on another system of classification which should consider natural affinities. Of this we have only the barest outlines, but, in spite of its publication, the older system was regarded by many botanists of the post-Linnean period as incapable of improvement.

Linnæus took a great interest in living plants, both in nature and in gardens, and in living animals as well. There is a legend that he first saw gorse in England at Putney Heath and thanked God for its beauty. It seems that this must be abandoned, as he had seen the plant before in flower at Hamburg.

He not only practised medicine, but with Rosen had charge of the complete medical faculty of the University and found time for coaching as well.

REVIEWS

MATHEMATICS

Vector Analysis. By C. RUNGE. Translated by H. LEVY, M.A., D.Sc. [Pp. viii + 226, with 36 figures.] (London: Methuen & Co., 1923. Price 9s. net.)

THE practice of translating foreign scientific textbooks, especially German ones, into English is becoming commoner. It is a concession to the curious dislikes which our scientists seem to have of reading books in languages other than their own. It is idle to pretend that they will be able to dispense with some knowledge at least of French, German, and Italian, but a translation may save them trouble. Also presumably it is easier and better to translate a foreign book than to get a competent authority at home to spend valuable time in writing a new one. But, clearly, to be worth translating a textbook ought to be not only the best available book on the subject, but also very good indeed. Prof. Runge's *Vector Analysis* does not seem to us quite to satisfy these requirements. It is an interesting treatment of vectors and tensors in three dimensions, but it strikes one as being unnecessarily complicated and not well arranged. We have not been able to compare this version with the original (published in 1919), but its pages, with their cumbersome asterisk notation for the system of vectors reciprocal to a given system of three independent vectors, do not look attractive. Great use is made of the vectorial area as the external product of two vectors, with its *complement* or *representation*, their vector product. The author himself says that the introduction of the idea of the complement of a vector is "unnecessary and superfluous" (p. 37); readers will agree with this verdict and wonder why he has thought fit thus to complicate a simple matter.

An annoying feature in the arrangement is in the illustrations from geometry and mathematical physics. They are introduced into the middle of the theory in such a way that it is difficult to discover whether any result is a general theorem or merely a consequence of the application of the analysis to a particular problem.

The chapter on Tensors is well worth reading; the treatment is original and instructive, though here again simplification would, we think, have been possible and desirable.

The work of proof-reading has been carelessly done. Misprints abound, and slovenly phrases, such as "this is the limiting value to whichΔ (p.q)/Δt approaches as *t limits to t*," are too common. The diagrams, too, might have been clearer; it is rather confusing when one is told (p. 43) that a figure represents two skew lines, "the one drawn heavier lying above the other," not to be able to detect the slightest difference between the two in the way of heaviness.

F. P. W.

The Elements of Co-ordinate Geometry. Part II: Trilinear Co-ordinates, etc. By S. L. LONEY, M.A. [Pp. viii + 228.] (London: Macmillan & Co., 1923. Price 6s.)

PROF. LONEY'S elementary textbook on analytical geometry, published in 1895, is a well-known and excellent introduction to the subject and is still

widely used. He has now written a sequel which will no doubt prove popular as a good elementary exposition of the old-fashioned kind, with numerous and well-chosen examples. Most of the matter, homographic ranges, trilinear co-ordinates, tangential co-ordinates, and so on, is of course to be found in Salmon; the last chapter, on invariants and covariants of systems of conics, contains, however, a good deal, both in the text and in the examples, which is not there. We are glad that Prof. Loney has included this chapter; the subject is too often omitted in elementary books.

But the treatment is old-fashioned. The whole point of homogeneous co-ordinates is to develop *projective* as distinct from *metrical* properties; the specification of the particular co-ordinates, trilinear or areal, is of secondary importance, and formulæ for lengths of perpendiculars and so on might well have been omitted. So, too, we could wish that the principle of duality had not been introduced as though it necessarily depended upon or had anything to do with reciprocation in regard to a conic.

F. P. W.

Elements of the Theory of Infinite Processes. By Lloyd L. Smail, Ph.D. [Pp. viii + 330.] (New York: McGraw-Hill Book Co., 1923. Price 17*s.* 6*d.* net.)

This book collects into a small compass theorems on limits, infinite sequences, series, products, continued fractions, and determinants. The treatment is clear and, on the whole, good. The subject-matter is familiar enough; practically all of it, with the exception of the chapters on Dirichlet series, continued fractions and infinite determinants, is to be found in greater detail in such a book as Bromwich's *Infinite Series*, and the additional chapters are disappointing, being mainly summaries of results, the reader being referred elsewhere for proofs and details.

We must say that the part dealing with the exponential theorem seems to us to be thoroughly unsound. The number e is defined in a perfectly correct manner, but then e^x suddenly makes its appearance, for any real value of x, without any mention of the exponential series. Later (p. 157) the author says: "For real values of x we shall assume that the *function* e^x has been defined as an exponential and its properties known," which apart from the doubtful grammar is mystifying enough. He then proceeds to show that

$$e^x = E(x) = 1 + x + \frac{x^2}{2!} + \dots$$

by differentiating the function $E(x)/e^x$!

This is a serious blemish on an otherwise valuable compilation.

F. P. W.

Principes et Premiers Développements de Géométrie Générale Synthétique Moderne. Par Émile Bally. [Pp. viii + 218.] (Paris: Gauthier-Villars et Cie., 1922. Price 20 frs.)

This book is dedicated "aux amis de la Géométrie" and is written by an amateur; the author has composed a general treatise on synthetic geometry of any number of dimensions from its foundations which he hopes can be read "par toute personne intelligente, même étrangère aux mathématiques, exercée au raisonnement, et ayant le goût des abstractions." But the present cost of printing has prevented the publication of the complete work, and we are here only given a preliminary chapter on arithmetic, chapter 1 and chapters 12–14. On the whole, this is not very much to be regretted. The preface is dated from Martinique, and the author excuses himself from

giving references by explaining that the only works accessible to him have been Darboux's *Cyclides*, Dumont's *Cubic Surfaces*, Duporcq's little *Géométrie moderne*, and the Geometry fascicules of the French edition of the mathematical encyclopædia—a curious collection.

The first chapter lays down the propositions of incidence for linear constructs of any number of dimensions—the author prefers to use the word "*polynarité*," a point having polynarité one, a line two, and so on—he deduces Desargues's theorem, states that Pappus's theorem must then be assumed, and proceeds to talk about projectivities and collineations. It is all very difficult reading and doesn't get anywhere in particular—phrases like "champs linéaires isopolynaires dualisimilaires" are common.

The last three chapters are devoted to the *Hexagrammum Mysticum* of Pascal, a configuration which has interested many geometers. The author probably enjoyed writing these chapters, but they are extremely complicated and there does not seem to be much justification for adding to the already abundant literature. (A summary of the results and the relation of the configuration to simpler configurations in three and four dimensions is to be found in an appendix to the second volume of Baker's *Principles of Geometry*). The author is bold enough to christen certain 45 points "*points de Bally* (si toutefois ces éléments n'ont pas encore été jusqu'ici mentionnés); " he has also 45 *droites de Bally*. But alas! they are nothing more than the Y points and y lines of Veronese (1877). Similarly, his *fausses pascales, faux kirkmanns*, and *points de Dépas* are respectively Veronese's σ lines, ξ points and E points.

M. Bally does, however (p. 154), point out an error in the *Encyklopädie* (German edn., III, 2, i, p. 40). It is not generally true that if a hexagon, taken in a given order, be both a Pascal and a Brianchon hexagon, then the three hexagons formed by taking two pairs of opposite sides and joining the free ends crosswise have the same property. Luröth, from whom the theorem is derived, had taken a special case. F. P. W.

Biomathematics. By W. M. Feldman, M.D., B.S. With an Introduction by Sir William M. Bayliss, F.R.S. [Pp. xix + 398.] (London: Charles Griffin & Co., 1923. Price 21*s*. net.)

"But I assert," wrote Kant, "that the claim of any particular branch of natural philosophy to be considered as a science, can be assessed only on the basis of the amount of mathematics employed in it." This is Dr. Feldman's text, and he has provided a book which should be of great service to the non-mathematical biologist. As Prof. Bayliss says in his Introduction, a time often comes in the biologist's career when he begins to realise the value of mathematical knowledge, only to find that he has forgotten most of what he learned as a student; it is then that he should turn to *Biomathematics*. There are chapters dealing with elementary Algebra, Trigonometry, the Differential and Integral Calculus, with Fourier's Theorem and Differential Equations and Curve Fitting, and examples are given throughout of the application of these subjects to problems in Biology, Chemistry, etc. It is a pity, however, that, after laying stress on the absurdity of retaining more figures than are correct in numerical results, Dr. Feldman should have left a considerable number of errors in his own calculations.

The final chapter, entitled "Biometrics," deals with the application of modern statistical methods to biology. The author does not claim to go far into the subject, but with the help of numerical illustrations shows the reader how, for example, he should test the significance of the difference between two samples, or should form a correlation table. It must, however, be said in criticism that in a single chapter of fifty-six pages, it would have been wiser for Dr. Feldman to have dealt more fully with the elementary but fundamental ideas underlying the subject, and not to have attempted to

consider such problems as the fitting of skew frequency curves or the test for goodness of fit. He should rather have emphasised the danger of drawing conclusions from small samples, and the importance of considering the probable error of any calculated constant. It is, for instance, a serious fault that the data chosen to illustrate the handling of a correlation table contain only twenty-nine observations, and that no reference is made to the high value of the probable error of the resulting correlation coefficient (about $\pm \cdot 07$).

At the end of the book are given four-figure Tables of Logarithms, of Natural Sines and Cosines, and also an abbreviated Table of the Probability Integral.

G. S. PEARSON.

Statique Cinématique. Par ROBERT D'ADHEMAR. [Pp. xi + 254.] (Paris: Gauthier-Villars et Cie, 1923. Price 16 frs.)

THIS is an elementary treatise on statics and dynamics written for engineering students. The scope is not unlike that of the first year's course in mechanics done at our universities, or what the more advanced pupils do at our secondary schools. The treatment is, however, as different from that of our elementary textbooks as one acquainted with French writings can expect. The author seems to be in a particularly happy frame of mind, and even if on occasion one receives the impression that he is more concerned with his own enjoyment of his disquisition than with the question of how the readers are absorbing it, one nevertheless cannot help admiring the lucidity and freshness of the exposition. English textbooks teach, French textbooks inspire. Each category has the virtues and the vices of its particular aim, so that while English books are utilitarian and heavy, French books are interesting but heady.

The present book contains a considerable number of examples: the average engineer will, however, be left gasping by them! S. B.

Gliding and Soaring Flight. A Survey of Man's Endeavour to Fly by Natural Methods. By J. BERNARD WEISS. With a Preface by C. G. GREY, and an Appendix by W. H. SAYERS. [Pp. xx + 164.] (London: Sampson Low, Marston & Co., 1923. Price 5*s.* net.)

THAT scientific progress is of an oscillatory character is well illustrated by the history of the evolution of human flight. Imitation of birds was the first desire of human beings aspiring to flight. Sir George Cayley seems to have abandoned this in his belief "that a suitable engine was a *sine qua non* of future progress." Lilienthal set the fashion again in the direction of gliding rather than mechanical flight. The war led to a marvellous development in what has been called "beetle" flight, namely, one in which the wings sustain but do not propel, as opposed to bird flight in which the wings both sustain and propel. Post-war achievement in motorless flight seems to have focussed attention on the possibilities of gliding flight, with the inevitable consequence of enthusiastic advocacy and loss of sense of proportion.

It must not be understood, however, that we charge Mr. Weiss with the fault just mentioned. His book is indeed a very sober and helpful account of the progress of motorless flight, and its historical value cannot be challenged. It is true that during the war, when actual flight had to be catered for under various and often difficult conditions, we tended to forget the possibilities of gliding and soaring, and aviators concentrated all their efforts on the production of aeroplanes regardless of cost and of optimum conditions; and in this sense Mr. Weiss's book should be of value in bringing to our notice the steady progress in motorless flight due to the pioneers of the present and of past generations. But we cannot accept the observa-

tions made by Mr. Grey without the proverbial grain of salt. We certainly admit the great value of gliding, both aerodynamically and meteorologically; we hail the wonderful achievements in Germany and in England as opening a new era in aeronautical evolution: but we claim that the aeroplane advance of the past ten years has not been so barren of scientific achievement as Mr. Grey implies.

It is natural that Mr. Weiss should give some prominence to the work done by his father, José Weiss. His desire to distinguish between gliding and soaring in the sense of making the latter independent of upward wind is hardly helpful, and a corrective to this tendency is supplied in Mr. Sayers's appendix. The book is useful and interesting, and well illustrated by means of a number of photographs of gliders and glides. S. B.

The Mathematical Theory of Relativity. By A. Kopff. Translated by H. Levy, M.A., D.Sc., F.R.S.E. [Pp. viii + 214.] (London: Methuen & Co., 1923. Price 8*s*. 6*d*. net.)

It is probably a fact that on no scientific theory has so much been written in four years as on the theory of relativity since the announcement of the verification of the light-bending made at the total solar eclipse in May 1919. Commendation and condemnation of the theory have followed one another in bewildering sequence, and even if we discard the journalistic type of article and opinions that are based on racial and nationalistic bias, we are still left with a vast array of articles, papers, and books on the subject of relativity and its implications. One is therefore disposed to regard a new book on the subject with some misgiving as to the exact niche that it can occupy in the rather crowded relativistic temple. The same applies to a translation, especially as there has been a glut in translations during the last few years.

Prof. Kopff's book is a somewhat heavy but nevertheless interesting and sound account of the mathematical theory of relativity. The treatment is not essentially mathematical, but the necessary mathematical arguments are included, with an elementary account of tensors. There is nothing very startling in the book, which proceeds on the whole on "orthodox" Einsteinian lines. The problem of rotation is touched upon, and what the engineer calls centrifugal forces are referred to the gravitational field of the totality of mass in the universe. Weyl's theory which establishes a connection between electricity and gravitation is omitted.

A careful perusal of this book will give the mathematically equipped reader a good knowledge of the main features of the theory. Yet one or two points may be mentioned here. A frankly mathematical discussion of the problem of the single gravitating centre, based upon the exact equations, is far easier to follow and to appreciate than the author's approximate methods. Further, the author has a very irritating habit of proving some elementary steps in a mathematical argument, and then quoting some more difficult results without proof: the result is to make the book less useful to mathematical readers without being more useful to general readers. Prof. Kopff defines the special theory of relativity for "two systems of co-ordinates moving relative to each other with uniform rectilinear speed": is this quite justified? Prof. Painlevé objects to this view very forcibly in a recent book.

Prof. Levy has done his task fairly well: we must nevertheless object to a sentence like this: "An electron which at rest has the shape of a sphere during uniform translation is flattened in the direction of motion." Far better books already exist in the English language, but it is always interesting to see how scientists in other countries deal with the great international movements in scientific thought. S. B.

The Theory of Relativity. Three Lectures for Chemists by ERWIN FREUNDLICH. Translated by HENRY L. BROSE, M.A., with an Introduction by VISCOUNT HALDANE. [Pp. xii + 98.] (London: Methuen & Co. Price 5s. net.)

So many books are now appearing with the object of instructing non-experts on the subject of Relativity as recently enunciated by Einstein and other distinguished mathematicians and physicists that we can do no more than mention some of them. A very distinguished mathematician told us the other day that he had not yet even "grasped Einstein." He was, of course, quite capable of doing so, but was so immersed in his own work that he had no time for the effort. Other people are in the same case; and though textbooks are many, readers are frequently few. Viscount Haldane commends this book very strongly in his Preface, and it is certainly very clearly written; but even in it we find technical mathematical terms which would perhaps befog some chemists, not to mention laymen. Dr. Erwin Freundlich's book is one of the clearest expositions for laymen (and scientists of one group of science are unfortunately nearly always laymen as regards other groups), but we are not certain that it is the last word in this line. After all, would not a sequence of simple propositions in the Euclidean manner be more illuminating, beginning, say, with the old illustration of two boatmen rowing, one to a point across a river and another to a point at the same distance up the river? In the present book this fundamental proposition does not occur until near the middle of the volume. The non-expert will learn most by comparing several similar works, including those by Einstein himself; but in any case he cannot hope to grasp the matter without a considerable amount of hard work—and thought.

The same publishers have issued a second edition of *The Foundations of Einstein's Theory of Gravitation* by the same authors (price 6s.), which was originally published at the Cambridge University Press in 1920. Considerable changes have been made, and this work is for much more advanced students than the one previously mentioned. Non-physical readers had better begin with the former.

We doubt whether any recent works of this kind are so well done as *The Rudiments of Relativity*, by Dr. J. P. Dalton, Professor of Mathematics at Johannesburg, published in 1921 (and to be had from Wheldon and Wesley, 28 Essex Street, Strand.) It is to be hoped that a new edition of these lectures will follow the recent verification of Einstein's Third Criterion.

PHYSICS

The New Physics: Lectures for Laymen and Others. By ARTHUR HAAS. Translated by ROBERT W. LAWSON. [Pp. xi + 165, with diagrams.] (London: Methuen & Co. Price 6s. net.)

THERE is no doubt that this book is a delightful little work. It is written "for laymen and others," but the professional scientist will find it by no means without charm. There is a certain relish in an occasional digestion of the fact that the mind of man is gradually, but none the less surely, acquiring a grip of the beautiful design of the tremendous universe in which he appears so much alone and insignificant. The book contains six lectures: (1) The Electromagnetic Theory of Light; (2) Molecular Statistics; (3) The Electron Theory; (4) The Quantum Theory; (5) The Theory of the Chemical Elements; (6) The Theory of Relativity and of Gravitation. None of these is beyond the comprehension of the intelligent laymen, except, perhaps, the most recent developments of the theory of relativity.

In one or two places there appears the continental habit of forgetting little

points having bearing on the discovery of fundamental scientific laws. For instance, the author refers glibly to the Principle of the Conservation of Energy as Mayer's Law, not mentioning the fact that Joule was the first to make an accurate determination of the mechanical equivalent of heat, and that many other illustrious names—Colding, Clausius, Rankine, and Thomson—are all inseparably connected with the principle. Similar remarks might be made about Lavoisier's Law of the Conservation of Mass and about the Lorentz Contraction. The statement, too, that "crystals represent single giant molecules" is hardly correct. In some cases the identity of single chemical molecules is lost in ionisation, but in others the molecules exist and are capable of identification by means of X-rays.

At the end of the volume, in addition to the usual Subject Index, there is a very useful Historical Summary and Name Index. There is a misprint for "propagation" on p. 72.

W. T. A.

Modern Electrical Theory. Supplementary chapters, Chapter XVII. **The Structure of the Atom.** By NORMAN ROBERT CAMPBELL, Sc.D. [Pp. x + 158.] (Cambridge: at the University Press. Price 10s. net.)

IN the supplementary chapter on Relativity of his *Modern Electrical Theory* Dr. Campbell gives us a very clear and precise exposition of the Principles of Relativity which should be especially appreciated by those to whom he addresses himself in particular, namely, experimental physicists. It is for this very reason that the methods employed in developing the new formulæ may appear somewhat round-about and tedious to those accustomed to regard the matter from a purely theoretical standpoint. The meaning and range of application of the various principles are very clearly analysed and the common misconceptions and difficulties concerning them are traced to their origin, that origin being, in the majority of cases, the conception of simultaneity of events and the preconceived ideas which we have regarding time sequence. Emphasis is laid on the fact that Relativity does not attempt to explain phenomena in accordance with the usual interpretation of the term, but seeks rather to express the relations which must hold between the measurements made of some particular system by observers moving relatively to one another; the importance of relativity, the General Theory in particular, lies not so much in the results which it predicts—these being mostly of the nature of extremely small corrections—but rather in the new ideas and conceptions which it introduces. In discussing the plausibility of these new conceptions, Dr. Campbell is careful to consider the matter from both points of view, and in this he certainly remains true to the promise which he makes in the Preface to refrain from being dogmatic.

On reading the book one is impressed rather by the sharp line of distinction drawn between the mathematician and the physicist; there is no doubt that this distinction would be a hard-and-fast one if only one kind of mathematician existed, namely, the pure mathematician, and only one kind of physicist, the experimental physicist. However, as in all classes of society, there are individuals in both these groups who take as much interest in other people's business as in their own, and it is perhaps just as well that they do, as otherwise there would be no general theory of relativity. That very useful individual, the mathematical physicist, is not referred to, but surely it is he who is chiefly responsible for the co-ordination of the two branches of study. His activities in relation to mathematics and physics are analogous to those of the physical chemist in relation to physics and chemistry. The pure mathematician may handle the problems of the physicist in a very callous and cold-blooded fashion in order to "get his numbers to argue about," as Dr. Campbell puts it, but it is the mathematical physicist who looks after the interests of the physicist in this respect and guides the investigation

along lines likely to lead to results useful to the experimental investigator. There can be little doubt that Einstein, Lorentz, and others are distinguished members of this latter class. Again, the purest of pure mathematicians is not in general devoid of all normal aspirations, on the contrary the most famous mathematicians have exhibited a broadness of outlook characteristic of greatness in all its forms; surely they may be excused if occasionally they meddle with the problems of the physicist; one can calculate the relation between surface tension and the pressure in a soap bubble without ever having seen one, nevertheless blowing bubbles is quite a fascinating pastime.

The book is, like most works on the subject, divided into two parts, the first of which deals with the Special Theory. The fundamental expression of the Special Theory is taken as the invariance of the expression

$$c^2(t)^2 - (x)^2$$

for observers moving relatively to one another in the x direction. It is shown that the invariance of the above expression leads to the principle of constant light velocity and to the other well-known results of the Special Theory.

The application of the above to electromagnetism is then discussed; it is rather surprising that no mention is made in this connection of the pioneer work of Lorentz. Following an account of the variation of mass with velocity and the relation between mass and energy is a brief account of Minkowski's contribution to the theory. This seems to lose considerably in clearness by comparing the observations of observers of events other than the propagation of a light signal; the conception of a rotation of axes does not force itself upon one so much in the former case as in the latter.

The second half of the book gives a very good account of the General Theory and its consequences. An unfortunate misprint occurs on p. 86 in the formula defining a covariant tensor. It is a pity that a mistake of this character should occur at such a critical juncture. Dr. Campbell does not agree with Einstein's prediction of a spectral shift, for reasons which he very clearly states. To sum up, the book gives a very good account of Relativity in as far as the non-mathematician is likely to understand it, and parts of it are worth reading even for those conversant with the subject, on account of the novel form in which some of the problems are presented.

J. W. F.

A Treatise on the Analysis of Spectra. By W. M. Hicks, Sc.D., F.R.S. [Pp. 326, with 25 diagrams and an Appendix containing tables of data.] (Cambridge: The University Press, 1922. Price 35*s*.)

In the whole domain of Physics, there is probably no more fascinating study than spectroscopy. On the one hand, it offers great opportunity to the experimental genius who can manipulate with exquisite skill the various forms of spectrometers, spectroscopes, and interferometers now procurable, or can design some new type of instrument for observing and accurately placing lines and their components. On the other, it offers a wide field of investigation for those who are interested in discovering the relationships which exist between lines of the same series for one element or between the series spectra of different elements, or in unravelling the intricacies which surround the lines not yet brought under the sway of a series formula. Moreover, it is almost a truism to say that, with all the keen interest displayed in many quarters at present on the problem of atomic structure, the study of spectra provides an ultimate test which any theory of the atom must pass if it is to survive as sound addition to the body of Theoretical Physics. The position of a line can now be measured with an accuracy superior to the degree of precision obtainable in observing any other phenomenon where the behaviour of an individual atom is concerned.

This treatise by Professor Hicks is based on an essay to which the Adams Prize was awarded in 1921. It is an attempt to present as a connected whole knowledge already obtained as to the relationships between different spectra, and thus to provide an introduction to the subject for those who are desirous of entering on a study which, although not so fascinating as experimental work or the weaving of theories, is an indispensable preliminary to further progress. In this respect alone it is an invaluable work whose appearance renders a great service to those entering on the study of the *facts* of spectroscopy. Prof. Hicks presents the data in a purely objective manner, without any preconceptions as to theoretical reasons for the production of spectra, and the scope of the treatise excludes not only the consideration of spectroscopic technique, but also theories of spectroscopic effects. Indeed, the effects produced on spectral lines by change in physical conditions are only considered in so far as they are capable of throwing light on the immediate question of line relationships. This attitude of mind on the part of the author considerably restricts the discussion, but it has one inestimable advantage: it enables him to perform the great service of presenting in a connected fashion just what has been accomplished by the computer in the orderly arrangement of series. Unfortunately, a perusal of works devoted solely to theories of atomic structure produces in the mind of the reader an impression of simplicity in line relationships, which should be dispelled as soon as possible. So complex are the relationships that their decipherment involves great patience and an extremely laborious disentanglement of a mass of details. Every spectroscopist will recognise the debt he owes to the author in presenting here the result of patient and complicated computations.

The volume is, of course, not merely a summary of spectroscopic facts. It is stamped with the individuality of the author, who, as is well known, has made contributions to the discussion of spectral series of a quite special character. Briefly his position is that series spectra do not follow a strict mathematical law. In this connection he has introduced the concept of the "oun" ($\omega\nu$) of an element, which is closely connected with its atomic weight. Thus, not only does he attempt to show that the wave-number differences between the components of doublets and triplets can be expressed in terms of the "oun" of the element in question, but he also maintains that series lines in general deviate from a "mean" formula and that the deviation can be expressed in terms of integral multiples of the "oun." It should, however, be stated that some distinguished spectroscopists regard Hicks's procedure in this respect as somewhat arbitrary and doubt if there be any physical reality behind it; they suggest that still greater accuracy of observation will be necessary before a close test of his views becomes possible. No one, however, fails to recognise the enormous body of computation involved in Hicks's work; and it is certainly very necessary at present to bear in mind that formulæ for series spectra may involve only mathematical convenience and that the physical facts may prove irreducible in terms of mathematical elegance.

To sum up, this work is an indispensable book in the library of everyone who wishes to know something more than the mere skeleton of what has been done towards the orderly arrangement of lines in series.

J. R.

Four Lectures on Relativity and Space. By CHARLES PROTEUS STEINMETZ, A.M., Ph.D. [Pp. x + 126.] (London: McGraw-Hill Publishing Co., 1923. Price 10s.)

THE problem of introducing to the layman the new ideas and conceptions underlying the theory of Relativity has been tackled by numerous writers,

including Einstein himself, but in point of view of clear and bold exposition and consistency of treatment it is hard to find a parallel to the admirable little book by Steinmetz. This book embodies a series of four lectures delivered by Steinmetz to a section of the American Institute of Electrical Engineers. This, however, need be no discouragement to the man in the street; Steinmetz addresses himself exclusively to the layman and, as he states in the preface, he makes no attempt at a rigid exposition, but seeks rather to illustrate his points by means of simple analogies and familiar examples. The justification for this is obvious and needs no comment.

The first lecture deals with the rudiments of the Special Theory and illustrates the relativity of all motion. The constancy of the velocity of light is taken as an axiom and the relative character of length and time measurements is deduced as a consequence of this, the usual hypothetical railway-train and billiard-table playing an important rôle in these deductions. Finally, the close correspondence between the effects of a gravitational field of force and the inertial forces due to acceleration is pointed out. The second lecture deals with the various consequences of the Special Theory, and, among other things, the wave theory of light is discussed and the ether hypothesis is rejected in favour of the conception of electromagnetic field energy in space. It seems a little difficult, however, to find sufficient justification for this on the grounds put forward, namely, that the ether, if it existed, could not be both at rest and in motion with respect to the earth, or, in other words, that the phenomena of aberration and constancy of the velocity of light could not exist simultaneously. This argument might be sound enough according to the older ideas, but, as is well known, the transformation equations of the Special Theory would predict both of these phenomena altogether irrespectively of any ether theory. The most convincing argument against an ether of material nature is to be found in the discrepancies and difficulties which arise when one tries to reconcile the elastic properties of such a material ether with the electromagnetic equations for optical media. The key to the mystery probably lies in the fact, as Steinmetz himself points out, that we have been attempting to explain fundamental things in terms of complex ones, and so when we have reduced all phenomena to the geometrical characteristics of some generalised space, we may have reached the end of our tether, or, to use an example cited by Eddington, we may discover all the laws of a game of chess by close observation, but we shall be no nearer to finding out what the chessmen are made of. Lecture four deals with the relativity conception of the gravitational field, while the last and most significant chapters deal with the characteristics of the various kinds of space. An excellent, and in many respects unique, account is given of the distinguishing features of Euclidean, Elliptic, and Hyperbolic Space, after reading which even the most lay of lay readers might be expected to come to the conclusion that there is perhaps something in all this after all.

J. W. F.

Theoria Philosophiæ Naturalis. By Boscovich, 1763. Latin text with English translation by J. M. Child. [Pp. xviii + 463.] (Open Court Publishing Co., 1922. Price £3 3s.)

To many students of mechanics the history of their subject is represented by a few important and outstanding works. The *Principia* of Newton marks the birth of modern dynamics, and a century later the works of Lagrange and Laplace present an elaboration of the Newtonian theory not essentially different from that which is accepted to-day. The less well-known intervening period was occupied by sharp controversy in which the Newtonian theory gradually gained ground against rival systems and established itself

in European thought. Boscovich's *Theoria Philosophiæ Naturalis* may serve, perhaps better than any other single work, to fill in this gap. It is of great value to have an English translation such as that which is now published.

Although the work deals with many special problems, interest will concentrate mainly on its fundamental assumptions. Boscovich postulates primary elements of matter which are indivisible and non-extended points interspersed in a vacuum and floating in it. Between any two such points there is a definite interval which can be increased or diminished but can never vanish altogether. The motions of these points are determined by two principles; whether these depend upon an arbitrary law of the Supreme Architect or upon the nature or attributes of the points, Boscovich does not seek to know. Each point, if it exists by itself in nature, has an inherent propensity to remain in a state of rest or uniform motion in a straight line. Any two points of matter are subject to a determination to approach one another at some distances and to recede from one another at other distances. Thus the *vires* of Boscovich are accelerations rather than forces. The particles, since they have no size, have no mass in the Newtonian sense of quantity of matter. The actual motion of a given particle is found by rules which are equivalent to the composition of accelerations by the parallelogram law.

Although Boscovich is careful to emphasise the differences between his theory and that of Newton on the one hand, and that of Leibnitz on the other, it is clear that he is strongly under the influence of both. The identity of indiscernibles demands that the non-extended particles can never come into absolute contact. Hence, the law of gravitation must give place at small distances to a repulsion which increases indefinitely as the distance diminishes. Newton's forces of attraction, cohesion, and fermentation remain, but continuity demands that they should be smoothed out and comprehended in a single law. This is taken to mean that the law of force must be expressible for all distances, by a single algebraic expression. From this it follows that even for great distances the Newtonian law is only an approximation—a result of far-reaching theoretical importance.

The work (including the figures) is beautifully printed, with the English translation facing the Latin text. There are unusually long lists of Corrigenda and Errata, but these are evidence of a careful revision and are doubtless due to difficulties of access to the original text, as explained in the Preface. The interest of the work is increased by a reproduction of the title-page and the Printer's Preface from the Venetian edition of 1763, and by the inclusion of the author's Dedication, Preface, and detailed Synopsis from the Vienna edition of 1758.

The book should find its place in every mathematical and philosophical library, and will prove full of interest to all those to whom mechanics means more than engineering.

G. B. J.

CHEMISTRY

Elementary Physical Chemistry. By W. H. Barrett, M.A. [Pp. viii + 247, with 61 figures.] (London: Edward Arnold & Co. Price 6s. net.)

Mr. Barrett has been very successful in his presentation of Physical Chemistry as a subject within the curriculum of a well-established school. The correct perspective of historical development has been well preserved, and by an exclusive application of the kinetic hypothesis his explanations have been made extremely lucid. He is to be congratulated upon his choice of problems for solution. These are of a practical nature throughout, and it could only be wished that he had included a larger number of these very important aids to the teaching and understanding of what can be

turned easily into a purely abstract science. Experimental details are clearly and fully given, and the book should prove of considerable use from both the practical and the theoretical points of view. The inclusion of numerous references to original works is an innovation for this type of book, and should prove of considerable advantage to teachers.

A. E. MITCHELL.

Radioactivity and the Latest Developments in the Study of the Chemical Elements. By K. FAJANS. Translated by T. S. WHEELER and W. G. KING. [Pp. xiv + 138, with 11 figures.] (London: Methuen & Co. Price 8*s*. 6*d*. net.)

MANY monographs and textbooks on the constitution of the atom have appeared during the last two or three years; the most successful of these have been written by men who have themselves contributed largely to the experimental and theoretical work which they describe. The present work is one of the best examples of this class.

The subject-matter of the book has been to some extent covered by Aston's *Isotopes*, Russell's *Chemistry of the Radioactive Elements*, Born's *Constitution of Matter*, and other monographs. It is, however, valuable on account of the new angle from which the recent work has been viewed by the author. Written originally, in the first edition, from the point of view of natural radioactivity and isotopy, it has been necessary on account of recent developments to extend its purview to all investigations which throw light on the constitution of the nucleus.

The radioactive transformations and the chemical properties of the radioactive elements have not been described at great length, for these have been dealt with in numerous publications. Attention is directed especially to the displacement laws of radioactive transformation, to the end products of the dissociation series, to isotopy of the ordinary elements, and to the artificial decomposition of atoms, and these phenomena discussed from the point of view of their bearing on the constitution and structure of atomic nuclei. A law of instability of the elements is now published for the first time, and a description follows of the chemical and physical properties of isotopes.

The author proposes that the term "atomic weight," referring to mean atomic weight of the isotopes of the elements, should in future be replaced by "combining weight." While there is a good deal to be said in favour of this proposal, in view of the long standing of the term "atomic weight" in chemistry, it is doubtful if this change in nomenclature will be acceptable to chemists. The German Commission on Atomic Weights designates the atomic weight of ordinary elements as "practical atomic weights."

W. E. G.

Practical Physical Chemistry. By ALEXANDER FINDLAY, M.A., D.Sc. Fourth edition, revised and enlarged. [Pp. xvi + 298, with 117 figures.] (London: Longmans, Green & Co., 1923. Price 7*s*. 6*d*. net.)

THE arrangement and subject-matter of Prof. Findlay's new edition is essentially the same as that of the last. Several notable additions have been made in the description of standard apparatus. Included in the optical instruments are the Abbé Refractometer, in addition to the Pulfrick type, and the Hilger Wave-length Spectrometer (Constant Deviation type). The description of the Cottrell boiling-point apparatus is an important addition to the section on ebullioscopic methods. The modern tendency for the application of electromotive force measurements to analytical methods

has been responsible for the introduction of electrometric analysis, including hydrogen ion determinations. A section on the measurement of oxidation and reduction potentials is also included. A short chapter on experimental colloid chemistry is added, and although the number of experiments is comparatively small, these should serve as a very useful groundwork for a more advanced laboratory course in this section. Prof. Findlay's book can still claim to maintain its position as the best on practical physical chemistry.

A. E. Mitchell.

An Introduction to Theoretical and Applied Colloid Chemistry. "The World of Neglected Dimensions." By Dr. Wolfgang Ostwald. Translated by Dr. Martin H. Fischer. [Pp. xii + 266, with 46 illustrations.] (New York: John Wiley & Sons, London: Chapman & Hall, 1922. Price 12*s.* 6*d.* net.)

This book, now in its second American and eighth German edition, has been brought up to date by the addition of some thirty pages. The subject is presented in the form of five lectures, the first three of which are devoted to the fundamental properties of the colloidal state, the classification of colloids, and changes in the state of colloids; the scientific and industrial applications of colloid chemistry are dealt with in the last two lectures.

The clear and lucid method of presentation of the author is a source of great pleasure to the reader, which is enhanced at every turn by the discovery of phenomena which are rarely referred to in most textbooks on colloid chemistry. One may mention, as an example, the syneresis of colloids (p. 98), which has hitherto received scant attention, in spite of its evident bearing on the problems of the rubber, collodion, and silica gel industries.

It is one of the best surveys of the field of colloid chemistry and should be read, not only by students, but also by those industrial chemists who wish to learn in what manner this subject may bear on their industries. The carefully chosen experiments and examples will prove of great service to teachers of physical chemistry, and this book should be read by them in conjunction with the same author's *Small Practical Book on Colloid Chemistry*, published in German by Theodor Steinkopff, 1922, where the experiments illustrating the lectures are given in greater detail than was possible in the present work.

W. E. G.

A Method for the Identification of Pure Organic Compounds. By Samuel Parsons Mulliken, Ph.D. Vol. IV, Compounds of Higher Orders. [Pp. vi + 238.] (New York: John Wiley & Sons; London: Chapman & Hall, n.d. Price 30*s.* net.)

In this volume Professor Mulliken continues and as far as possible completes his system of analysis for the commoner organic compounds. It contains the classified descriptions of some 3,700 of the more important compounds belonging to fourteen of the higher orders of his system, dealing with substances containing the halogens and sulphur in addition to some or all of the elements carbon, hydrogen, oxygen, and nitrogen. The descriptions are given with the author's well-known care, though in some cases a happier choice of derivatives might have been made; for example, 2 : 4-dinitroaniline cannot be prepared from 4-chloro-1 : 3-dinitrobenzene quite so easily as is indicated, and 2 : 4-dinitrodiphenylamine would have been a better derivative to describe; again, 6-chloro-2-nitrobenzaldehyde is mentioned as giving dichloro-indigo with acetone and alkali, as this latter is not easily identified and as most substituted o-nitrobenzaldehydes give products of similar properties under such conditions it would have been better if a deriva-

tive of definite melting-point such as the semicarbazone or oxime had been described. In other cases no derivatives are given, or if given their melting-points are omitted. There is no doubt that the value of the book would be increased if more attention were paid to this question, as the most frequent difficulty in characterisation is the choice of the most suitable derivative to prepare.

As it is, however, the book is a useful work of reference and a monument to the industry and patience of Prof. Mulliken and his associates. One cannot avoid wondering, however, whether the value of the result is commensurate with the toil involved. The systematising of qualitative organic analysis destroys its real value as a method of training students, since this value consists in the necessity for the exercise of common sense and a thorough knowledge of the simpler reactions of various types of organic compounds. Others, though they will refer to the book from time to time, are unlikely to devote the time necessary to master the method thoroughly since the identification of an unknown organic compound is a problem which only occasionally presents itself.

O. L. B.

Chemical Technology and Analysis of Oil Fats and Waxes. Vol. III. By J. Lewkowitsch, M.A., F.I.C. Sixth Edition, entirely revised by George H. Warburton. [Pp. viii + 508, with numerous illustrations and tables.] (London: Macmillan & Co., 1923. Price 36*s.* net.)

This volume completes the new edition of the late Dr. Lewkowitsch's standard work on this subject and deals with the technology of manufactured oils, fats, and waxes and the examination of the various products concerned. The revision in this volume has been more thorough than in that previously noticed and the various methods of analysis described are treated in a more critical manner; in addition, the need of elimination of older work is less necessary than in the volume dealing with the more chemical aspect of the subject. This book has such an established reputation that it is obvious that everyone interested in the subject must have a copy at hand.

O. L. B.

Organic Syntheses. Vol. II. By James B. Conant, Editor-in-Chief. [Pp. vi + 100.] (New York: John Wiley & Sons; London: Chapman & Hall, 1922. Price 7*s.* 6*d.* net.)

The second volume of this work maintains the high standard of the first. Methods, tested in such a way as to ensure that they are reliable, for the preparation of twenty-five more organic compounds are given with full details and notes. As many of these compounds are of use in research and are frequently difficult to obtain of a requisite degree of purity the importance of this work is patent. The book is really a report of a scientific investigation of the methods of preparation of the compounds, carried out with great thoroughness. Though not designed for that purpose, it provides, in addition, an extension of the number of preparations which can be entrusted to students as exercises, providing a welcome variation from the stock experiments; in this aspect the reliability of the methods described gives it an advantage over other books which not infrequently reproduce methods from the literature which even a skilled experimenter frequently finds difficult to carry out successfully. One must congratulate those concerned on their work and wish them every success.

O. L. B.

A Comprehensive Treatise on Inorganic and Theoretical Chemistry. By J. W. Mellor, D.Sc., Vol. IV. Ra and Ac families; Be; Mg; Zn; Cd; Hg. [Pp. x + 1074, with diagrams and illustrations.] (London: Longmans, Green & Co., 1923. Price 63*s.* net.)

In these days of "Outlines" of this, that, and the other, it is a welcome change to receive the more solid food provided by Dr. Mellor, and one has looked forward to this volume with especial interest as it includes Radium, the problem of Radioactivity, the Structure of Matter, and the Architecture of the Atom.

The first two hundred pages are devoted to the study of these highly important subjects, the remaining 870 odd pages covering the elements Beryllium, Magnesium, Zinc, Cadmium, and Mercury. So far as concerns the latter chapters, readers of the previous volumes of the Treatise will find the general treatment similar in all respects to that already adopted; the references are as complete as ever—those for Mercury itself, for instance, occupying no less than twelve pages of small type! The descriptive portions are comprehensive and clear, though one could wish that the contractions such as soln., press., temp., aq., had not been used in the actual text.

Regarding the first three chapters opinion will, no doubt, be divided. Dr. Mellor has made a laudable attempt to summarise the present state of our knowledge of radioactivity and atomic structure and has attained some measure of success. The chief fault to be found, however, is that the summary in inclined to be too uncritical, including, as it does on the one hand, references to quite trivial and ephemeral papers, and on the other hand allowing statements to pass that are occasionally quite inaccurate. Thus, on page 129, the well-known radioactive Displacement Law is given, in italics, in the original form suggested in 1913, which is recognised as being no longer an accurate statement of the facts of the case.

Probably it would not be unfair to say that the theoretical portions of the book are less admirable than the purely descriptive parts, but in any case the wealth of material provided makes the volume, like its predecessors, an invaluable compendium of information upon Inorganic Chemistry.

F. A. Mason.

GEOLOGY

Petrology for Students. Sixth Edition, revised. By A. Harker, M.A., LL.D., F.R.S. [Pp. 302, with 100 figures.] (Cambridge: University Press, 1923. Price 8*s.* 6*d.* net.)

The fact that a sixth revised edition of Harker's *Petrology for Students* has been called for is proof, if proof were needed, that the book adequately meets the needs of a large body of students. It is, however, misnamed, as it is really a "Petrography for Students." It is almost entirely descriptive; the inquiry into causes, or even methods, which we should expect in a "Petrology," is of the slightest. Not that the reviewer quarrels with the book on that account. Within its limits it is a long way the best book of its kind; but, while not forgetting his *Natural History of Igneous Rocks*, which only covers part of the field, we wish that Dr. Harker, out of the fulness of his unrivalled knowledge of the subject, would give us the complementary "Petrology" on a similar scale. There is not much scope for criticism in this edition, unless we comment on theoretical points of classification and nomenclature. Dr. Harker notes the artificiality of the present mode of distinction between diorite and gabbro on the basis of containing hornblende or augite respectively; but he does not adopt any better way, as for example, basing the distinction on the nature of the plagioclase, or on the relative amounts of felsic and mafic minerals. Similarly, he rightly regards

the hypabyssal group of igneous rocks as of a very artificial character. The majority of these rocks should be associated in classification with their parent plutonic types, and the remainder with their respective volcanic representatives. There is an unnecessary duplication of nomenclature in retaining both the terms *porphyry* and *porphyrite*. All that is connoted by the latter term, and more, can be conveyed by prefixing the name of the parent plutonic rock to *porphyry*. Dr. Harker still uses the term *granulitic* to express a characteristic texture of basalts in preference to the more suitable and less equivocal, term *intergranular* proposed by Dr. J. W. Evans.

The chapters on the sedimentary and metamorphic rocks still remain as the sole systematic statement in English of the microscopical characters of these rocks. Dr. Harker's pen-drawings of thin rock sections are amongst the finest that we have seen. A few new ones have been added, and a few old ones withdrawn, for the present edition. We have discovered no misprints; but "Lamlash near Arran" (p. 194) is rather an ambiguous placename.

G. W. T.

An Introduction to Stratigraphy (British Isles). By L. D. STAMP, B.A., D.Sc. [Pp. xv + 368, with 84 figures.] (London: Thos. Murby & Co., 1923. Price 10*s*. net.)

THIS book introduces elementary British stratigraphy to students of geology along somewhat novel lines. The common method of presentation of this subject, with "successions" in unfamiliar regions, lithology and thicknesses of various groups of strata, and long lists of fossils, is compared with an obsolete method of teaching geography by memorising lists of capes and rivers. Instead of this deadening routine the author begins his accounts of the various geological periods by an interesting statement of the geographic and climatic conditions which prevailed within the British region during the given period, and thereafter exhibits the stratigraphical facts as necessarily following from these conditions. We have no hesitation in commending this procedure as far more likely to interest the student at the outset of the work, and to get the facts into him almost painlessly. After all, the geological history of any region told by the stratigraphical and palæontological facts is a fascinating story, which deserves to be presented in an interesting way.

In the following points the book is open to criticism, but they are merely spots on the sun of its general excellence. It is not necessary, for example, to correlate different kinds of petrographic provinces with Atlantic or Pacific types of *coast-line* (p. 10). It is unsafe now to refer to the Older Palæozoic as *Proterozoic* (p. 27), when the latter term is so much used for a late division of the Pre-Cambrian. Although the above use of the term may have the priority it has not been generally adopted. The continued use of the term *Lakes* (after Sir A. Geikie) for the Old Red Sandstone basins of deposition, the sediments of which Dr. Stamp rightly regards as of mainly fluviatile origin, is much to be deprecated.

Dr. Stamp adopts the view that all the Lower Palæozoic earth movements were merely preliminary to the great Caledonian orogenic climax at the end of Silurian time; further, that the movements continued *diminuendo* during the Devonian. This is consonant with the latest views of Scandinavian geologists regarding the same series of events in their country.

The Devonian and the Tertiary are treated with especial fulness and clarity, as would be expected from the author's record of research.

Throughout the book the stratigraphical relations of igneous phenomena, no less than those of sediments and earth movements, are admirably displayed. Detailed matter suitable for advanced students is given in small type. There

is an unusually full index. Finally we must make especial mention of the extraordinarily good and illuminating series of sections, palæogeographic maps, and diagrams, which, as is stated in the Preface, are essential to the proper understanding of the text.

G. W. T.

BOTANY

A Handbook of the Larger British Fungi. By John Ramsbottom, M.A. [Pp. iv + 222, with 141 figures in the text.] (London: British Museum, 1923. Price 7*s*. 6*d*. net.)

Mr. Ramsbottom has not only provided a compact and handy guide to the genera and very many of the species of the larger British Ascomycetes and Basidiomycetes, but he has contrived, by the judicious use of small type, to compass within 222 pages a remarkable amount of miscellaneous information not usually found in systematic works. This not only adds materially to the value of the work for students, but results in a volume in which even the lay reader will find much to interest and not infrequently to amuse.

Under *Coprinus atramentarius* we can find how to prepare ink from the deliquescent cap. *Marasmius oreades*, the Fairy-ring fungus, is not only described in company with some twelve other species of the genus, but the causes of fairy-ring formation are briefly indicated. The account of *Armillaria mellea* is the occasion for dealing with its destructive character as a parasite and its remarkable relation to the flowering of the Japanese orchid *Gastrodia elata*, whilst the causes and control of dry rot are treated of in connection with *Merulius lachrymans*.

The work is based on W. G. Smith's guide to Sowerby's Models, and like the latter is intended primarily as a means of distinguishing the edible and poisonous species. Unlike some mycologists, whose taste in fungi is very catholic, the author would appear to have a discriminating palate which distinguishes between species which are edible and species which are good to eat. Nothing would appear to be omitted in this connection to provide for those who should desire to enlarge their fungal diet. Adequate descriptions to recognise the species, how to prepare the more delectable kinds, the symptoms resulting from failures in the use of the botanical diagnoses and even the treatment to be adopted, are all enumerated.

When we add that there is an illustration of one or other species for most of the genera cited, it will be realised that the work should prove of great utility to students, and the British Museum authorities are to be congratulated on producing so admirable a handbook at so reasonable a figure.

E. J. Salisbury.

Botany: Principles and Problems. By Edmund W. Linnott, Professor of Botany, Connecticut Agricultural College. First Edition. [Pp. xix + 385, with 240 figures and a frontispiece.] (New York and London: McGraw-Hill Book Co. 1923. Price 16*s*. net.)

This book is not written around any particular syllabus, but is intended as a presentation of the subject for the use of first-year undergraduates. The idea of the author, as he states it in his Foreword to the student, is to give not knowledge but understanding, and to foster the spirit of enquiry which is not only curious but is also critical. With this idea in view a very large number of "questions for thought and discussion" and "Reference Problems" are provided at the ends of the chapters. These are intended to help the individual student by stimulating thought and also for use in promoting class discussion. Quite a fair portion of the book is given to short illustrated

historical surveys, and a very large part is concerned with the structure of the higher plant especially in connection with function. There are specially good chapters on the soil and on the plant and its environment, while heredity and variation come in for a much fuller and interesting treatment than is customary in a book of this size A very general account is given of the lower types, attention being specially directed to the parts they play in nature and in human affairs. The systematics of the Angiosperms is only very lightly touched on, a very short account of the most important orders being provided.

This book is clearly printed, strongly bound, and illustrated with a large number of clear and well-selected line-drawings and well-thought-out diagrams as well as by a considerable number of interesting half-tone illustrations. Its general appearance in fact is such as should make an immediate appeal to the student. It is an interesting and stimulating volume.

E. M. C.

Studies in Plant Respiration and Photosynthesis. By H. A. Spoehr and J. M. McGee. [Pp. 98.] (Publication No. 325 of the Carnegie Institution of Washington. Price $1.50.)

It has become increasingly clear that the problem of photo-synthesis is too complex to be explained except as a result of a more intensive study of the metabolism of chlorophyllous organs. This has been the object of the authors, and the above publication comprises the results of their investigations during the years 1919–22. By means of carefully refined experimental methods, they have obtained some interesting results with regard to the carbohydrate-amino-acid relation in the respiration of leaves. In the dark the amino-acid content of leaves increases and this stimulates respiratory activity. They put forward the interesting suggestion that this augmented respiratory activity is at least one important factor contributing to the increased rate of growth of plants during periods of darkness.

The experiments on the relationship of photosynthetic and respiratory activities described in the later portion of this publication are rather incomplete. It is stated, however, that they are being extended, and it seems likely that they will lead to important results.

There are a few printer's errors. Table 38 should obviously be in the place occupied by Table 37, and on p. 91 Table 53 should read Table 54. The authors are to be congratulated on the ingenious apparatus and methods they have adopted. They have undoubtedly produced a valuable contribution to the literature on the respiration of plants.

Nigel G. Ball.

ZOOLOGY

An Introduction to Zoology, through Nature Study. By Rosalie Lulham, B.Sc. [Pp. xviii + 513, with 355 illustrations.] (London: Macmillan & Co., 1923. Price, 10s. net.)

This book, now appearing in its second edition, is intended for pupils in secondary schools, students of training colleges and for the solitary worker. It covers the whole animal kingdom with the exception of vertebrates and a few rare and aberrant groups.

Beginning with the Protozoa, the principal subdivisions of each phylum are described, most attention being given to external features and habits, which can be easily observed without elaborate equipment. Internal structure is described where necessary for the correct definition of a group. Each chapter concludes with guidance for the practical study of types easily ob-

tained in this country; it is intended that this should be worked through before the descriptive portion of each chapter is read.

The book abounds in practical information as to how the various types may be kept in captivity, which are the best species to obtain, and how they should be fed.

The second edition has been enlarged by the introduction of fresh matter on Cephalopods and Echinoderms, many new illustrations, and especially by the addition of appendices on the construction of a subterranium for keeping burrowing animals, a formicarium for ants, and how to make artificial sea-water and keep it aerated in the aquarium.

The book is thoroughly scientific and well adapted to the needs of those to whom it is addressed. We can join the writer of the Foreword in wishing the new edition the success it deserves. J. H. W.

Game Birds and Wild-fowl of Great Britain and Ireland. By A. THORBURN, F.Z.S. [Pp. vii + 79, with 30 coloured plates.] (London: Longmans, Green & Co., 1923. Price £5 5s. net.)

MR. THORBURN's latest contribution to British ornithology is a noteworthy one. Although the book in its general arrangement and appearance resembles its predecessors by the same author and publishers, it is larger in size, a fact of which the artist has taken full advantage. The plates, with two exceptions, differ from the ones with which one has become familiar in the earlier volumes in that they are complete pictures, and not just illustrations of birds, so many to a plate, arranged as best as circumstances would permit. Several species that may naturally be seen together simultaneously, are included here and there on a single plate. Many of the pictures figure but a single species, whilst in some cases more than one plate has been devoted to a single bird. This is no doubt owing to the fact that the scope of the book is limited to fifty-eight species, for the portrayal of which thirty plates have been available.

The birds dealt with include all the British Game Birds and a small, and apparently arbitrary, selection of shore birds. It seems a pity that more of the latter could not have been included in view of the popularity of shore shooting. The title of the book is somewhat misleading in this respect, since the term "wild-fowl" is commonly used to include shore birds. "Game Birds *and* Wild-fowl" will probably convey the impression to most people that all waders are included.

We notice the omission amongst the rarer game birds of four species and subspecies of geese, as well as Baer's Duck.

Mr. Thorburn is particularly happy in painting game birds, and the present volume is probably the best of this fine series that has yet appeared. The well-chosen frontispiece, Plate 17, showing Mallards and Shovelers on the edge of a frozen lake, is particularly pleasing. Plates 1, Capercaillie, and 26, Buffle-head, Hooded Merganser and Green-winged Teal, are probably two of the best of a very beautiful collection of pictures. For some reason the Pheasants (plates 7 and 8) are not as satisfying as the majority.

The settings are in Thorburn's most characteristic and pleasing style. The little additions that add so much to the peculiar charm of his work have not been forgotten, a brilliant fungus here, a vivid Kingfisher there, or again, a pair of Bearded Tits in the reeds over the head of some duck.

The author has unfortunately followed the nomenclature of Howard Saunders's *Manual*, but the letterpress is better than that of the earlier works. Each plate is accompanied with a selection of well-chosen notes, some original, many borrowed. We are glad to note the descriptions of plumages. From the pen of an artist, and non-technical, they are of particular interest.

W. R.

Indian Bird Life. By M. R. N. Holmer. Pp. ix + 100 with one coloured plate.] (London: Oxford University Press, 1923. Price 3s. 6d. net.)

This little book deals with the commoner birds to be found in Northern India. The authoress says of it " But it isn't merely a book to read: it is a book to work with." Her method is to describe her birds briefly and colloquially, selecting only those species that are common or characteristic in the districts with which she deals; her aim, to give the novice, in every-day language, the rudiments of knowledge that will enable him later to study scientific ornithological works. To what extent the book will achieve its object it is difficult to say, for one gains the impression that the writer is unconsciously assuming a knowledge of British birds on the part of her readers which many will not possess.

The birds referred to in the text are listed in an appendix with reference to " Fauna of British India." Keys to the various orders touched upon, abstracted from the " Fauna," are also included as appendixes.

The book no doubt fills a gap in Indian bird literature. It is pleasantly written and easy to read.

W. R.

Life: A Book for Elementary Students. By Sir Arthur E. Shipley, G.B.E., F.R.S. [Pp. xvi + 193, Index, and 71 illustrations.] (Cambridge: at the University Press, 1923. Price 6s. net.)

When, as stated in the Preface, the University Press asked Sir Arthur Shipley to write a book that would make elementary students of biology think, their selection of an author was more than fully justified. It is no mere biological textbook that is before us, but one of the most charming books on science that have ever been written. To students of biology it will certainly give much food for thought, and moreover a deeper purpose in their reading and research. As the author states, he has aimed at emphasising the unity of life and the interrelation of living organisms with one another and with their surroundings, and he has succeeded in giving a wonderfully clear and comprehensive view of world life as a whole, understandable even of those not well versed in biological science. Sir Arthur Shipley has a charming literary style, and there is every likelihood that the hope with which he closes his Preface will be fulfilled, namely, that the book will be of interest to the public that is not preparing for examinations and thank heaven that public is still in the great majority.

It is a book that should be read by every teacher, whether of the sciences or the humanities. To them all it cannot but make an irresistible appeal, widen their outlook, and afford a splendid example of scientific exposition.

W. C. B.

A Biology of the British Hemiptera-Heteroptera. By Edward A. Butler, B.A., B.Sc., F.E.S. [Pp. viii + 682, with 7 plates.] (London: H. F. & G. Witherby, 326 High Holborn, W.C. Price £3 3s. net.)

Bugs—in the British, not the American sense—have not attracted the attention of many specialists. This may be partly due to the unpleasant associations inseparable from the very word, but is mainly because the members of the order live a singularly detached, retiring life. Insects in general play active and complicated parts in the cosmic scheme, fertilising flowers, scavenging, providing food for higher forms, and so on: mutual adaptation of parts and habits links many orders with each other, with other classes of animals, and with the vegetable kingdom, so that a proper under-

standing of any group must involve some research into the forms and ways of other groups.

The Hemiptera stand apart, living a monotonous self-contained existence. They are not attractive in appearance, have few striking structural features, and rarely become sufficiently prominent to demand attention from the economist, who devotes much time to locusts and aphides, because of their menace to human food. Except the abhorred *Cimex lectularius*, which can be conquered by the most elementary sanitation, and a few Capsids which have been found to damage fruit trees, bugs can scarcely be charged with doing man any injury. On the other hand, it would appear that the unpleasant odours emitted by many species, render them distasteful, so that they do not form a very general pabulum of higher animals.

Mr. Butler modestly disclaims any intention to supplant the standard work of Saunders on the British Hemiptera-Heteroptera, and by omitting descriptions of the imagines, he has proved the bona fides of his desire, merely to supplement that work by giving particulars of the earlier stages, and amplifying data regarding the life-cycle, habitat, and distribution. He has spared no pains to set down his own extensive field work, to gather together all the information scattered through the entomological publications of the last thirty years, and to arrange it in a readily accessible form. In addition to the descriptions of 462 species, there are well-arranged tables of seasonal and regional distribution, and a flora, giving the index numbers of the species associated with each plant. The arrangement of the work is admirable, and the chief object of a book of this kind—easy reference—is excellently achieved.

Mr. Butler has an easy and lucid style, and states his facts in an interesting manner without obscurity. To the collection of facts, he appears to confine himself, and he makes no attempt to theorise or generalise upon them. This is a little unusual, and we should have thought the temptation hard to resist. Why, for instance, do *Cytisus*, *Ononis*, *Tanacetum*, *Erica*, and *Urtica*, harbour many species of bug, while not one has so far been discovered on an orchid ?

H. M.

British Hymenoptera. By A. S. Buckhurst, A.R.C.S., D.I.C., L. N. Staniland, A.R.C.S., D.I.C., and E. B. Watson, A.R.C.S., D.I.C. With an Introduction by H. M. Lefroy, M.A., Professor of Entomology at the Imperial College of Science, London. [Pp. 48, with 8 plates.] (London: Edward Arnold & Co., 43 Maddox Street, W.1. Price 9*s*. net.)

Even in these days of tabloid literature, it seems a tall order to dispose of such a varied collection of insects as the four thousand British Hymenoptera in eight thousand words !

With the benediction of Prof. Lefroy, this trio of authors declares that the book "is intended to be a help to those who desire to commence the study of a large and much neglected group of insects. . . . Technical terms have . . . been avoided ; where these are necessary they have been carefully explained." One wonders what the innocent reader will make of such words as "vestigial," "styliform," "chitinised," and "phytophagous," all of which are used without explanation.

If the book is intended to introduce the reader who has some general knowledge of entomology, to the special group, it seems strange that more stress is not laid on the fundamental characters of the Hymenoptera. The wings are dismissed in a few words, which do not even refer to the interlocking in flight by means of hooks. If the authors were really limited to eight thousand words, they would have every justification for the excuse made in the preface that "the subject-matter devoted to each family is necessarily brief," but this scarcely warrants its being inaccurate. The two paragraphs

devoted to the hive bee contain serious mistakes, the worst of which is that after the swarming, "the young queens strive for the mastery of the nest; the victor, having killed off her rivals, leaves the nest on her nuptial flight." Every beekeeper knows that virgin queens live together in harmony, and it is not till one has mated that she destroys her sisters. It is also incorrect to say that bees affected by *Tarsonemus* die in the hive, for the most conspicuous feature of this complaint is the issue of the affected, crawling bees from the hive, to perish in vain attempts to fly.

The plates are well produced, and the selection of species for illustration does much to atone for the deficiencies of the text. On the last plate a beetle is put under the heading "Diptera" and a fly under "Coleoptera." It seems a pity that the diagram of a bee's hind leg on p. 38 should be upside-down.

H. M.

Butterfly Lore. By H. ELTRINGHAM, M.A., D.Sc., F.E.S., F.Z.S. [P. 180, with one plate and 52 figures.] (London: Oxford University Press, 1923. Price 4*s*. 6*d*. net.)

WRITERS of popular books about butterflies have, hitherto, scarcely attempted to take their readers below the skin-deep beauty of the insects' lovely wings. Perhaps they feared to destroy popular interest by the multiplication of technical terms and descriptions which require close application to follow. They skimmed lightly over details of structure and habit, sketched the early stages of the life-history faintly, and saved their best efforts for the patterns and colours of the wings.

Dr. Eltringham's book is a bold departure from this tradition. One is struck by the fact that it would scarcely be possible to identify any of the butterflies to which he refers, for his descriptions are only intended to draw attention to some aspect of butterfly life and its relation to general biological problems.

It is a fascinating little book, summarising, in plain language, the vast store of knowledge accumulated by entomologists during the post-Darwinian period. Many who have been interested in butterflies for years will be introduced for the first time to facts concerning the senses and structure. The extraordinary association of the "Blue" butterflies with ants is here set forth in popular form as a connected story, and the wonders of mimicry and polymorphism, perhaps the most fascinating side of butterfly study, are treated of clearly and with many examples which will be new to most of those for whom the book is intended.

Dr. Eltringham's extensive field work, at home and abroad, and his standing as one of the secretaries of the Entomological Society, are sufficient guarantees of the scrupulous accuracy of every detail in this most welcome book.

H. M.

The Founders of Oceanography and their Work: An Introduction to the Science of the Sea. By SIR WILLIAM A. HERDMAN, C.B.E., F.R.S., D.Sc. [Pp. 340 + xii, with 28 plates.] (London: Edward Arnold & Co., 1923. Price 21*s*. net.)

THE modern science of oceanography began with the three years' voyage of the *Challenger* in 1872–6. This was a landmark, not only in the history of marine investigation, but also in the history of British science. A little group of scientific men were able to persuade the Government of the time to send out a big ship, with a highly efficient naval and civilian crew, for a long voyage of circumnavigation. Then they induced our permanent officials to set up an office for the working-out of the collections and data, and in a

relatively short time they published fifty big quarto volumes of reports, a fair number of which were presented to the University libraries. Nowadays government departments (with the exception of those of the Irish Free State) no longer present libraries with their scientific reports, and the impulse towards economy went so far, about the end of the war, as to suggest to the Treasury officers that the remaining volumes of the *Challenger* Report should be "pulped." Fortunately this characteristic proposal was made public and the stupidity was averted.

No oceanographical expedition on the scale of the *Challenger* one has since been made, either in this country or abroad, but the study of the sea has, nevertheless, been developed to a remarkable degree. This has resulted, on the marine biological side, from the establishment of the laboratories at Naples, Plymouth, Monaco, and elsewhere. Several expeditions, dealing predominantly with marine biology (those of the German *Valdivia*, the Dutch *Siboga*, the Norwegian *Michael Sars*, the various Antarctic expeditions and others), have added largely to our knowledge of the species of animals and plants inhabiting the great oceans. Hydrography, in the older sense—that is, the knowledge of the depths of the deep oceans and shallow seas and also of the surface currents—has been studied, from the point of view of practical navigation, by the various Admiralty departments, and oceanic meteorology has been investigated in the same way. The investigation of the tides practically began about the middle of the nineteenth century. Earlier than that, tide-tables were constructed empirically by secret methods and proprietary almanacs were published. The mathematicians, Whewell, Lubbock, and George Darwin, in this country, discovered how to make much better tables. Many workers studied the dynamical theory, and Harris, in America, and then the investigators at the Liverpool Tidal Institute, made a new start. The invention, by Lord Kelvin, of the tidal-predicting machine, the development of the method of harmonic analysis by Darwin, and the Liverpool work of the last ten years are the landmarks in this department of oceanography.

A very important advance in organisation was made early in the twentieth century when the International Council for the Exploration of the Sea was established by the principal European Governments. This meant systematic, routine biological and physical investigation of the North Atlantic tributary seas by about half a dozen exploring vessels. Primarily the object of the work was an economic one—research into the marine conditions affecting the climate of north-west Europe and the study of the sea-fisheries. Much that will ultimately be of assistance in weather forecasting has already been discovered, and results of some interest in connection with practical fishery administration have been obtained. Still the effect of the international investigations is predominantly purely scientific, and its greatest value will doubtless be the invention and development of new and very beautiful apparatus and methods of marine physical and biological research. The organisation has survived the war, though grievously weakened by the non-inclusion of the German oceanographers.

To a large degree the purely descriptive part of oceanographical research is within sight of completion: hydrography, in the older sense, has been very well done, and marine biology, though far from complete, is represented by an overwhelming mass of data. Future research will be largely physico-mathematical (tides and currents and winds and their causes), or physiological (the study of the physico-chemical conditions of life in the sea). From the point of view of geophysics nearly everything has to be done, for there is as yet no marine counterpart to what is now known as geomorphology. The development of oceanography on this side awaits an enormous advance in our knowledge of the nature of the oceanic floors and their deposits. Progress in this direction will probably be most rapid in relation to the fine research

which has now been started by the Americans in the Pacific Ocean. With respect to purely laboratory and mathematical work, the science is well developed, but on the observational side it is very poorly equipped. The sea-going work is at the present time largely fishery investigation, and, as such, it is restricted in scope and methods to a degree that is irritating in the extreme to the oceanographers. The work is very costly, and until some ship-owners of vision come to its assistance it must depend on government departments and remain limited in its outlook and restricted as to its methods.

The book now under notice treats mainly of the modern development of the science on its marine biological side. That development is so recent as to be within one man's lifetime. The author has, himself, been a marine biologist and oceanographer, and has had opportunities of seeing the laboratories and institutes of both hemispheres. He has had the great privilege of acquaintance with the founders of the science, and a charm of intimacy will be found in the earlier chapters, making the book a rather unusual one. This personal knowledge of such men as Sir Wyville Thomson, Sir John Murray, the Prince of Monaco, Alexander Agassiz, and others makes the book one of great delight for the general lover of science. The interest, and even romance, that is associated with the great voyages of exploration has been related by Sir William Herdman in a way that is really very attractive. The book is handsomely got up and well illustrated. It may be recommended very sincerely to the general reader, the amateur biologists, and the geographers, as well as to students of oceanographical science.

J. J.

Animal Life in Deserts. By P. A. Buxton, M.A. [Pp. xv + 176, with 43 illustrations.] (London: Edward Arnold & Co. Price 10*s.* 6*d.* net.)

The adaptation of living creatures to their environment is one of the most fascinating of biological studies and of unfailing interest to all naturalists. No one, whether he is interested in nature or not, who has travelled in a desert can fail to be struck by the scarcity of the flora and fauna of such a region. The less casual observer, however, will be agreeably surprised at the comparatively large number of species of birds, beasts, and plants which inhabit the earth, particularly in the rainy season.

Mr. Buxton does well to start off with his definition of a desert, which he describes as "a place in which the climate is hostile to animals and plants, in which normal agriculture is impossible, and in which nearly all the existent forms of animal and plant life are modified to endure life in their peculiar environment." In other words, a desert might be described as a region in which human life is insupportable and animal and plant life have a hard and perpetual struggle for the continuation of their species.

After considering the climatic influences, such as rainfall, variations of temperature, wind, and sunlight, which affect animal and plant life in deserts, the author goes on to give a description of the floral environment, animals, and the physical environment, and concludes with a long chapter on the colours of desert animals.

He divides the desert flora into three groups, namely, the annuals propagated by seed, which are short-lived and grow rampantly after the rains; secondly, the perennial plants which grow from bulbs, corms, tubers, or fleshy roots; and, thirdly, the perennial forms which are so specialised that they can exist all the year round above ground more or less in their green state with or without leaves. The desert flora is usually quite characteristic—cacti in the New World replaced by euphorbias in the Old World are the most conspicuous, while a large proportion of the plants possess fleshy and

hairy leaves. To the traveller, however, the most striking characteristic of the bushes and trees is their thorniness—a feature doubtless originating as a form of protection against herbivorous animals, although at the present time such animals as camels, goats, and certain browsing antelopes have adapted their labial and buccal mucous membranes to negotiate all but the very stoutest.

The phenomena of æstivation and hibernation probably depend on the great question of food-supply. The bear in Canada hibernates when food is scarce—the rivers being frozen and the roots and berries buried deep in snow. Æstivation likewise among insects and certain lesser mammals is due to the paucity of food during the season when certain plants on which they depend for sustenance are resting.

As regards the question whether mammals, large and small, are capable of existing without water for long periods, this has been proved over and over again by many naturalists and careful observers in Africa. The reviewer many years ago transported a number of young gazelles (*G. pelzelni*) from the mainland to a small waterless island where rain frequently did not fall for a year or more, and where the flora numbered less than a score of species most of which were of a salsolaceous variety, and some of these gazelles were alive and very fit several years later when last seen. It is well known that some of the Dik-diks (genera *Madoqua* and *Rhyncotragus*) depend entirely on the green herbage and the dew for the little moisture they require. The various species of desert hares (*Lepus*), dassies (*Procavia*), and rodents belonging to the genera *Xerus, Jaculus, Gerbillus*, etc., appear to be able to exist and thrive for very long periods in localities where rain seldom falls, such as the Red Sea littoral and the shores around the Gulf of Aden.

On p. 104 Mr. Buxton speaks of the little owl as living in and being dependent on caves, but as a matter of fact this delightful species has little difficulty in finding a suitable hole for resting during the heat of the day and for nesting purposes in such places as the old dried and hardened stumps of the desert acacias and the numerous white anthills which dot the landscape in so many places. Its plaintive " me-eu " is frequently the only sound that breaks the silence of the bush at sundown. In these same anthills, throughout the numerous tunnels and compartments with an occasional hole leading to the exterior, is often found a strange collection of lodgers. In them the little owl, the common sparrow, the roller, babblers, and other birds are frequently found nesting, while lizards, dormice, and spiny mice are quite commonly found as inmates.

In his final chapter on the colours of desert animals Mr. Buxton will not accept the theory that the colours have been evolved for protective purposes, although he himself, as others before him, have been struck by the prevalent grey, buff, white, and sandy colours. He thinks that " the explanation will eventually be found in studying the effects of physical conditions upon the animal life." One strong point in favour of the protective coloration theory is not sufficiently stressed by the author, and that is the undoubted benefit derived by the birds that nest on the ground from dull and sombre coloration. Such species as the desert and crested larks, nightjars, coursers, and sandgrouse will let one almost tread on them before running away from their nests or taking to flight. It is during the nesting season that the various species require protection and are most anxious to avoid molestation, and this end is undoubtedly attained by their unobtrusive, unattractive, and ground-like coloration.

Mr. Buxton is to be congratulated on having broken new ground by collecting the data and reviewing from all aspects the vicissitudes of plant and animal life in deserts. The photographs and the line-drawings which illustrate the text throughout are excellent.

R. E. Drake-Brockman.

Across the Great Craterland to the Congo. By T. ALEXANDER BARNS, F.R.G., F.E.S. [Pp. 276, with 82 illustrations and 2 maps.] (London: Ernest Benn, Ltd. Price 25*s.* net.)

IN this new volume Mr. Barns has given us a sequel to *The Wonderland of the Eastern Congo*, and as he explains in his preface the journey was undertaken primarily in quest of a gorgeous butterfly which he had himself seen in the Congo forests. Prof. Gregory, of Glasgow University, one of the greatest authorities on African geology, has written a learned introduction on the great craters and the great rift valley which he himself visited many years ago and thus named. Mr. Barns puts in a plea for the preservation of the wonderful fauna of the regions he visited, and makes the wise suggestion that the Ngorongoro crater with its well-marked and distinctive boundaries should be turned into a game sanctuary. There seem to be no administrative reasons whatever against this, and it appears to be none too early to bring this suggestion to the notice of the proper authorities so that action may be taken without delay, as there seems to be a steady stream of sportsmen yearly visiting these little-known regions. Not that they can do very much harm at present, but as their numbers increase, so will the game get wilder and wilder. In any case, whether this particular spot is added to the existing Game Reserves or not, it is hoped that the farm of the late German owner, Herr Siedentopf, will not be sold to anyone nor any part of the crater leased. The only way to prevent this is to proclaim it a Game Reserve.

The first four chapters give a most interesting description of the crater country, and particularly of the Ngorongoro and its flora and fauna. Mr. and Mrs. Barns then part from Sir Charles Ross and his party and make their way to the Congo. After reaching the nearest station on the Dar-es-Salaam—Kigoma railway, they entrain for Kigoma and reach it the following morning. From here by steamer and rail they pass through the Belgian Congo to Stanleyville.

From Stanleyville the author, with the added burden of a severe attack of sciatica, had to undertake a long and wearisome march of twenty days to Bafwasendi in the heart of the Ituri Forest, which he had decided to make his headquarters, while he and his trained and untrained native assistants hunted the elusive and magnificent *papilio*, which was the main object of his journey. The author gives an amusing description of his experiences while collecting in this unhealthy region, where he stayed some months. After leaving the Ituri Forest, he and his wife travelled south and revisited Lake Kivu, the home of Beringe's gorilla and the Kivu chimpanzi. The chapter on these great apes is of very great interest and value, as it adds greatly to our very meagre knowledge of the habits of these fine beasts. After leaving the country around Lake Kivu, the author proceeds along Tanganyika, thence across the Mweru Plateau to hunt elephant in the Luvua Valley.

All Fellows of the Society for the Preservation of the Fauna of the Empire will be glad to hear that the author wishes to "explode one fallacy" by telling us that "there is no immediate possibility of the African elephant being exterminated. On the contrary, their numbers have increased prodigiously of late years. . . . The close preservation of these animals which has taken place throughout the Continent has had its effect." This is very welcome news, coming as it does from such an excellent authority, but the reviewer can state from personal experience that huge herds that once roamed wild, uninhabited, and waterless regions, where they were neither harmful nor dangerous, have long ceased to exist because they were unprotected by Game Laws. The author has no belief in elephant cemeteries; he suggests, as is doubtless the case, that they are probably the sites of some

old dry pools in the desert to which these great pachyderms have retired, when sick or wounded, for a drink, and have died there. Not the least interesting part of this most fascinating book is the description of the training of the African elephant at Api by the Belgians. All animal lovers will wish them increasing success in their enterprise.

The photographs which adorn this volume are, as one would expect after seeing Mr. Barns's last volume, excellent, while the two maps at the end of the book add greatly to its value.

R. E. Drake-Brockman.

ANTHROPOLOGY

The Home of the Indo-Europeans. By Harold H. Bender. [Pp. 59, with 5 illustrations and 2 maps.] (London: Oxford University Press, 1922. Price 4*s*. 6*d*. net.)

The author of this pamphlet is Professor of Indo-Germanic Philology in Princeton University, and he deals with the Aryan Question almost exclusively from the linguistic point of view. The essay being so brief and the subject so large, the author's arguments can hardly be anything but sketchy, but his remarks on the philological evidence appear in the main to be sound and convincing. He rebuts the attacks of the extreme sceptics, who doubt if it is possible to reconstruct primitive Indo-European conditions from the common vocabulary, and he points out truly that the objections urged (such as the objection that a common word may be borrowed from the same source by all the Aryan languages) can apply only to exceptional cases and cannot have a general application. Dr. Bender's view is that the original home of the Indo-Europeans was in the interior of Lithuania—the large Lithuania of history, not the small modern State. He shows that not only is the Lithuanian tongue itself very archaic, but that Lithuania is situated on the border between the two great groups of Aryan languages, the "centum" group to the west (Greek, Latin, Keltic, and Germanic) and the "satem" group to the east (Indo-Iranian, Slavic, etc.).

There is much to be said for the author's conclusions, although of course careful correlation of the philological evidence with archæological and other data is necessary.

A. G. T.

Roman Britain. By R. G. Collingwood, F.S.A. [Pp. 101, with 51 illustrations.] (London; Oxford University Press, 1923. Price 2*s*. 6*d*. net.)

This book has been written for those who, though unfamiliar with the subject with which Mr. Collingwood deals, are nevertheless anxious to learn about the sojourn of the Romans in Britain, and to such this small work may be recommended. The author has described with much lucidity, and aided by well-chosen illustrations, the conquest and occupation of this country, and has outlined the manner of living, the art, language, and religion in vogue during this occupation. Mr. Collingwood holds to the view that, though at the time of the original conquest there would have been no difficulty in deciding whether any given man was a Briton or a Roman, yet, before the end of the occupation, such differentiation would have been impossible owing, no doubt, largely to the practice of the Romans of intermarriage with the inhabitants of a conquered country. The author is emphatic in his belief that the Britons did not remain a subject race, held down by a Roman army, but became Roman in speech, in habits, and in sentiment. *Roman Britain* is an admirable little book, but exception must be taken to the suggestion, on p. 88, that, because of the knowledge of education, and other civilised

things, possessed by the Romans, a belief in the progress of man is not possible. But, though it could no doubt be easily established that we have progressed since Roman times, yet to attempt to form an opinion upon the question of human progress by comparing our present state with that of the quite recent Roman period is entirely misleading. To gain an adequate perspective in this matter, it is necessary to realise the state of culture of the earliest human beings, existing, probably, 500,000 years ago, and to compare it with that existing to-day.

J. Reid Moir.

Outlines of Palæontology. By H. H. Swinnerton, D.Sc., F.Z.S., F.G.S. [Pp. xii + 420, with 368 illustrations.] (London: Edward Arnold, 1923. Price 30s. net.)

Good textbooks dealing with the systematic aspects of palæontology are fairly common, but a comprehensive treatise on general principles has been lacking till now. In the present volume Prof. Swinnerton supplies the want.

The method of treatment is determined by the fact that beginners cannot appreciate the morphogenetic connotation of systematic designations, even when these designations are accompanied by more or less extended diagnoses. The author treats the organism as an entity of unit characters, each of which has attained a certain stage of development in an evolutionary series. When these characters are studied simultaneously in the genus, or in any systematic group whatsoever, it is difficult for the student to cultivate the dynamic conception, to avoid regarding the genus as static, or its characters as immutable. Accordingly, in each phylum the author traces the evolution of the significant structures to their somatic expression in various representative genera, each structure having attained its own stage of development in any particular genus independently of the others, yet in equilibrium with them, to prevent the occurrence of monstrosity. The differentiæ separating groups of related organisms, such as genera, are thus to be interpreted in terms of the differential evolutionary movements of their biocharacters, the groups themselves necessarily taking their appropriate places in an evolutionary plexus.

In the fourth section of the book, which deals with the Cœlentera, the author discusses the evolution of the gens *Zapphrentis delanouei* to illustrate the significance of variation and mutation, and Section V is an interesting interpolation on genetic affinity. The author describes Mendelian experimentation, discusses the germ-plasm as the material basis of heredity and variation, and describes its physical expression in the soma. The thirteenth, and concluding section, treats of the nature of biocharacters and bioseries, and discusses the general principles of palingenesis, tachygenesis, etc., which have been formulated on observation of the facts studied in the previous sections.

The book is copiously illustrated, and the figures are all diagrammatic. Comparative studies of certain phenomena are illustrated graphically. The technique of this method is discussed elsewhere by Prof. Swinnerton, but its practical application in this volume is important, because it brings to the notice of students the possibility and desirability of applying in palæontology the methods of exact science.

There are good general and systematic indexes, and a bibliography of the most recent works, carefully selected to form a nucleus of more extensive references.

A very stimulating book for students, and those interested in the teaching of the subject.

John Weir.

MISCELLANEOUS

Mine Examination Questions and Answers. By J. T. Beard, C.E., E.M. [Pp. 872.] (New York and London: McGraw-Hill Book Co., 1923. Price 37*s.* 6*d.*)

The title of this work is descriptive of its contents, subject to the qualification that it is primarily intended to be of assistance to those who are taking State examinations for subordinate official positions under the mining regulations of the United States of America. Within these limits it is undoubtedly complete, the work including practically 3,000 questions and answers, most of the former being drawn from State papers, including some from Canada, while the answers are made as educational as possible.

To miners having the laudable ambition to rise step by step, it should prove of assistance in their preparation for special examinations. Similar questions and answers appear in several colliery periodicals, but the advantage here is that they are collected and arranged under appropriate headings, namely: Air and Gases, Occurrence and Properties of Gases, Mine Explosions, Supervision and Inspection, Safety Lamps, Ventilation, Instruments, Mine Airways, Circulation of Air in Mine, Mensuration Mechanics, Steam, Steam Engines, Hoisting Requirements, Drainage and Pumping, Explosives, and Blasting, Geology and Prospecting, Working Seams of Coal, Mine Timbering. At the end there is, in addition, a subject index which will prove useful when matters of information, apart from direct questions, are concerned. The book is well got up in three volumes of handy size.

S. J. Truscott.

The Domain of Natural Science. Being the Gifford Lectures delivered in the University of Aberdeen in 1921 and 1922. By E. W. Hobson, Sc.D., LL.D., F.R.S. [Pp. xvi + 510.] (Cambridge: at the University Press. Price 21*s.* net.)

This book endeavours to indicate the position which the "scientific view" of the world should occupy in relation to the ideas of philosophy and religion. In pursuance of this end Prof. Hobson has found it necessary to make an examination of the historical development, aims, and true characteristics of various departments of natural science, with a view to the characterisation of the proper position of natural science in relation to thought in general. He has sought to demonstrate, throughout the book, that natural science is in no way concerned with questions as to the nature of reality, or with efficient causation; but his arguments are not always very convincing.

C. C. R.

The Boys' Own Book of Science. By Lloyd L. Darrow. [Pp. 325. Index, with 24 illustrations.] (New York: The Macmillan Company. Price 1*s.* 6*d.* net.)

It is difficult to place this book for British use. The author has designed it for the host of boys who wish to experiment at home. The bulk of our boys have not the means to acquire the apparatus or materials required for the experiments chosen, most of which are chemical. Further, the experiments are not arranged at all from an educative point of view, the idea of magic being indulged in freely, such as: freezing water by magic (crystallisation). Changing wine to water (the bleaching of permanganate of potash).

On the other hand, as the author says, this work may well be a source of experimental hints for teachers, seeking lecture and laboratory demonstrations of chemical principles.

W. C. B.

Acoustics of Buildings. By F. R. WATSON, Professor of Experimental Physics in the University of Illinois. [Pp. viii + 155, with illustrations.] (New York: John Wiley & Sons; London: Chapman & Hall, 1923. Price 15s. net.)

THIS is the first book on Architectural Acoustics since the pioneer work of the late Prof. W. C. Sabine was carried out. It is written as a guide to architects and builders both in the correction of acoustic difficulties in buildings already complete, and in their avoidance in the construction of new buildings. To this end theory and mathematical formulæ are not dwelt on at length, but the results of experimental tests are set forth in detail. Many numerical examples are worked out in illustration of the principles.

The book is divided into three main sections, the first being introductory and the second and third dealing respectively with the acoustics of the auditorium and the sound-proofing of buildings. In Part II an account is given of Sabine's work, and there is a useful table of absorption coefficients. Materials are in some instances specified only by their American trade-names, *e.g.* "Insulite," "Celotex"; the latter is now available for use in Britain. From the conditions prevailing in halls that have a good acoustic reputation the author plots curves showing the desirable reverberation period in rooms of different sizes used exclusively for music, and also for halls used for music and speaking. How far it is advantageous to reduce the period in a hall used exclusively for speech is not discussed. Consideration is also given to the question of the comparative loudnesses of sources desirable in different sizes of auditorium, and the author concludes that the number of instruments in an orchestra should, ideally, be varied. The ear, however, is able to perceive sound over a considerable range of intensity. The section concludes with curves and tables for various halls of different types which have been satisfactorily designed or corrected, and it should be useful as a general guide. The information regarding the effect of varying the shape of the room is not so complete as it might be.

Part III discusses the results of experimental tests and practical applications of these in the sound-proofing of rooms and buildings. Experience shows that escape or entry of sound is usually greatest through ventilation ducts and similar openings, and that, unless special precautions are taken here, it is useless to arrange for special walls and doors. Next in order of magnitude is the sound transmitted by the vibration of walls as a whole, and rigidity of partitions is therefore of prime importance. Very little sound is transmitted in elastic waves through the material of a partition. This section embodies the results of the author's own experimental work and practical experience and gives a great deal of valuable information.

On the whole, the book is exceedingly useful, as it collects and arranges information that has hitherto been scattered in papers not easily accessible and seldom arranged to give the architect the practical assistance he requires. The type and paper are good, and the book will doubtless be widely read. The price is a little high.

G. A. S.

Studies in Evolution and Eugenics. By PROF. S. J. HOLMES. [Pp. vi + 261.] (New York: Harcourt, Brace & Co.)

THE chief defect of this book is its lack of cohesion; indeed, it makes no claim to it, since it represents "some of the peculiar interests of the writer which have grown out of several years of occupation with the fields of heredity, evolution, and eugenics."

Most of the discussions make interesting reading, but have but little

new matter in them, and perhaps the most instructive part of the book is concerned with three problems which are ever important to the American—namely, the problems of immigration, racial mixture, and the negro.

Prof. Holmes takes the view which is common enough with most American thinkers, who see with alarm the swamping of the native stock by hordes of immigrants, often of a low type of culture. He advocates increased restrictions on the more undesirable type of immigrant, and it is not for us to say whether the policy he desires is right or wrong.

On the question of race mixture he is cautious. He does not encourage mixture on the ground that not enough is yet known about the effects of such mixture, but he does not categorically denounce all cross-bred races on account of degeneracy. And similarly, in the case of the negro, he neither predicts the swamping of the whites in America nor the rapid extinction of the negro through natural causes; but he admits that the negro's prospects are not good.

C. C. R.

Elements of Graphic Statics. By C. W. Hudson, C.E., and E. J. Squire, C.E. [Pp. viii + 91.] (London: McGraw-Hill Publishing Co., 1923. Price 6*s*. 3*d*. net.)

The authors state that their wish is to present the elementary principles of Graphic Statics in a form adapted particularly to the needs of engineering students, and more especially for those working under the guidance of a teacher.

The matter is very condensed both as regards text and diagrams, and an elementary student working unaided would probably find it rather difficult reading.

The usual fundamental constructions are given, and there are a number of excellent worked examples scattered through the text. Special attention is paid to the problem of the masonry arch.

There are two matters of convention in which the book is not quite in line with the modern British treatment. In the one case, moments are expressed in foot-pounds instead of pound feet; and in the other, the forces in the bars of a structure are called stresses, which term is better reserved for forces per unit area.

For students who either have help or are revising the subject quickly it is a useful little work.

H. T. D.

Analytical Microscopy. Its Aims and Methods. By T. E. Wallis, B.Sc. [Pp. viii + 179, with 75 figures.] (London: Edward Arnold & Co., 1923. Price 6*s*.)

"The Public Analyst, the Analyst in general consulting practice, and the Pharmacist frequently meet with problems which can only be satisfactorily and completely solved by a skilled use of microscopical methods." The preface of this book commences with this statement, and it is not any exaggeration to say that it might equally well refer to any work on applied microscopy.

Unfortunately the skilled use of microscopical methods is not common in any branch of science, nor does it appear to be realised that much training is required for this to be attained. Not only is some knowledge necessary of the optical principles involved if the instrument is to be used at its best, but experience is essential to avoid errors of interpretation. The book under notice deals with a branch of study that has many difficulties peculiar to it, but it can hardly be said that these are fully dealt with. Without some

further instruction than is provided on the use of the microscope, any chemist or analyst would not advance far, but perhaps it is assumed that some other treatise would be at hand to provide more detailed information. The method of illumination, for instance, commonly referred to as "Critical," is, judging from his description of it, apparently not understood by the author.

The book is mainly devoted to the description of methods of preparation of material for observation, and in this respect it fulfils its purpose. The chapters on sedimentation and centrifugation, and on elutriation, are good. As an example, the section dealing with the examination of water is very fully illustrated and shows the various forms of life likely to be encountered. Quantitative microscopy is treated briefly, but sufficiently well for a novice to understand its intention. A bibliography is added which covers nearly all branches of the subject, and forms a valuable addition to a book of this character.

J. E. B.

Electrical Measuring Instruments and Supply Meters. By D. J. Bolton, B.Sc., A.M.I.E.E. [Pp. xvi + 328, with 180 figures.] (London: Chapman & Hall, 1923. Price 12*s.* 6*d.* net.)

A comprehensive work of the "Directly Useful" series covering a very wide field, and passing from a consideration of moving iron, moving coil, dynamometer, and kindred apparatus to the standardisation apparatus in general use. Supply meters are treated very fully, and commercial testing sets and pyrometers are included in the field covered.

The relative advantages of the various types are discussed, and design compared from electrical, mechanical, and financial points of view, having due regard to the limitations of principle.

Numerous line-drawings and half-tone plates accompany the descriptive matter, and illustrate typical examples of modern practice.

In a publication devoting considerable space to supply meters, the omission of a detailed description of a pre-payment meter calls for comment, while the kathode-ray oscillograph described does not represent the latest practice.

The first edition of a work embracing such a wide field inevitably includes minor imperfections, and occasional inconvenience is caused by illustrations not being on pages facing the descriptive matter. The frequent insertion of remarks in parentheses is also a drawback.

From the practical man's point of view the volume has a great deal to recommend it in that it fills the gap usually found between the extremes of a descriptive catalogue conveying no information as to the design, and the purely theoretical work in which treatment is too involved for ready reference.

A. N. Jackson.

Carènes de Formes Nuisibles, ou Favorables à leurs Grandes Vitesses; et Résistances de l'Eau à leur Translation. Par M. le Vice-Amiral F.-E. Fournier. [Pp. 27.] (Paris: Gauthier-Villars et Cie., 1923. Price 3.50 frs.)

This is really a "paper" rather than a "book," since it deals with a definite restricted topic in a purely technical manner. Its object is to discuss which forms of boats are suitable for high-speed motion and which are definitely unsuited to this purpose; the author also explains how the distinction between the two types arises. Starting off with a formula giving the resistance as proportional to the square of the velocity multiplied by a factor ψ depending on the velocity and on the shape of the hull, M. Fournier

derives from model experiments carried out in Paris, empirical formulæ for ψ under various conditions of shape and speed. Assuming that the hull has no cylindrical portion and that the longitudinal section is rectangular, he shows that for velocities greater than a certain critical velocity (defined for a given shape and size), ψ increases to infinity as the velocity increases to infinity for some vessels, and increases to a maximum and then tends to a finite asymptotic value for other vessels. The former are not suited to quick motion, while the latter can be used for high speeds.

As explaining the difference between these two categories, M. Fournier considers the waves that accompany the ship, due to a sort of piling-up of water by the front part of the vessel. In the forms unsuited to quick motion we get a double wave, one at the bow and one at the stern, with water piled up right in front. As the shape becomes more favourable to high speeds, the piled-up water recedes backwards, until in the really favourable ship there is only one wave accompanying the vessel, at about amidships. In this last case the longitudinal direction of the ship is changed, so that it points upwards, out of the water.

S. B.

The Numeral Words: their Origin, Meaning, History, and Lesson. By MARIUS DE VILLIERS, M.A., LL.B. [Pp. 124.] (London: H. F. & G. Witherby, and Cape Town, 1923. Price 4*s.* 6*d.* net.)

THE author's frank admission that what he brings forward as new does not claim to be matter of certain knowledge, but of probability, enhances the readableness of this treatise, which professional philologists should welcome as testifying to a sustained interest in their pursuits. Extensive study of ways of counting among some of the less civilised peoples has led the author to believe in a similar origin for the Indo-European numeral words, and it is ingeniously argued that the numerals *one* to *ten* originally meant, in order, This, That, Yon, Hand less one, Whole hand, Three-three, Three off two-hands, Two off two-hands, One off two-hands, Two hands. The final chapters give much information on the representation of the numerals by figures, and on words derived from the numerals, and in conclusion evidence is found that language is a thing of purely human invention, brought to such perfection as it can claim by human means, through a long period of progressive growth.

One remark on terminology. The *Oxford English Dictionary* (O.E.D. = N.E.D.) shows that "Indo-European," the term generally preferred by French and Scandinavian scholars (*e.g.* Meillet, Noreen), was introduced by Dr. Thomas Young in 1814, while Klaproth's "Indogermanisch," much employed at present in the propagation of confusion and false doctrine, came into existence in 1823. The Germans at that time did not realise, as Young had done, that Celtic was a member of the family. In adopting the German substitute, "Indo-Germanic," Mr. de Villiers does but follow the lead of numerous works in English, by writers who ought to know better.

W. PERRETT.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Analytical Conics. By D. M. Y. Sommerville, M.A., D.Sc., F.N.Z.Inst. Professor of Pure and Applied Mathematics, Victoria University College, Wellington, N.Z. London: G. Bell & Sons, 1924. (Pp. vii + 310.) Price 15*s*. net.

Advanced Vector Analysis, with Application to Mathematical Physics. By C. Weatherburn, M.A., D.Sc., Professor of Mathematics, Canterbury University College, Christchurch, N.Z. London: G. Well & Sons, 1924. (Pp. xvi + 222.) Price 15*s*. net.

Lectures on Cauchy's Problem in Linear Partial Differential Equations. By Jacques Hadamard, LL.D., Member of the French Academy of Sciences, New Haven: Yale University Press; London: Oxford University Press, 1923. (Pp. ix + 316.) Price 15*s*. net.

Differential Equations in Applied Chemistry. By Frank Lauren Hitchcock, Ph.D., and Clark Shove Robinson, S.M. New York: John Wiley & Sons; London: Chapman & Hall, 1923. (Pp. vi + 110.) Price 7*s*. 6*d*. net.

History of Mathematics. Vol. I. General Survey of the History of Elementary Mathematics. By David Eugene Smith. London and New York: Ginn & Co. (Pp. xxii + 596 with illustrations). Price 21*s*. net.

The Calculus of Observations. A Treatise on Numerical Mathematics. By E. T. Whittaker, Sc.D., F.R.S., Professor of Mathematics in the University of Edinburgh, and G. Robinson, M.A., B.Sc., Lecturer in Mathematics in the University of Edinburgh. London: Blackie & Son, 50, Bailey, 1924. (Pp. xvi + 395). Price 18*s*. net.

Substance and Function and Einstein's Theory of Relativity. By Ernst Cassiver. Authorised Translation by William Curtis Swabey, Ph.D., and Marie Collins Swabey, Ph.D. Chicago and London: The Open Court Publishing Company, 1923. (Pp. xii + 465.) Price 18*s*. net.

La Méchanique Nouvelle. Par Henri Poincaré, Conférence, Mémoire et Note sur la Théorie de la Relativité. Introduction de M. Édouard Guillaume. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins. (Pp. 81.) Price 12 frs.

A Text Book of Physics. Including a Collection of Examples and Questions by W. Watson, C.M.G., A.R.C., D.Sc., F.R.S., late Professor of Physics at the Imperial College of Science and Technology. London, Eighth Edition Revised by Herbert Moss, D.Sc., A.R.C.S., D.I.C. London: Longmans, Green & Co., 39, Paternoster Row, E.C.4, 1923. (Pp. xx + 925.) Price 16*s*. net.

The Mechanical Properties of Fluids. A Collective Work by C. V. Drysdale, D.Sc., and others. London: Blackie & Son, 50 Old Bailey, 1923. (Pp. xiv + 362.) Price 20*s*. net.

Clerk Maxwell's Electromagnetic Theory. The Rede Lecture for 1923. By H. A. Loretz. Cambridge: at the University Press, 1923. (Pp. 35.) Price 1*s*. net.

L'Atomistique Exposé Élémentaire suivi de la démonstration des principales formules. Par Bernard Ravink et Traduit par André Juliard. Préface de M. Marcel Boli. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1923. (Pp. 25 + 152.) Price 12 frs.

A Course of Experimental Mechanics. By H. J. E. Bailey, M.A., Assistant Master of Shrewsbury School. With an Introduction by H. W. Turnbull. M.A., Regius Professor of Mathematics in the University of St. Andrews, London: Chapman & Hall, Ltd., 11 Henrietta St., W.C.2, 1924. (Pp. xv + 223.) Price 7*s.* 6*d.*

Physics. Volume I. Mechanics, Heat and Heat Engines. By W. J. R. Calvert, M.A., Science Master at Harrow School. London: John Murray, Albemarle St., W. (Pp. x + 260.) Price 3*s.* 6*d.* net.

X-Rays and Crystal Structure. By Sir W. H. Bragg, K.B.E., M.A., D.Sc. F.R.S., Director of the Royal Institution of Great Britain and of the Davy Faraday Laboratory, and W. L. Bragg, F.R.S., Langworthy Professor of Physics in the University of Manchester. Fourth Edition, Revised and Enlarged. London: G. Bell & Sons, 1924. (Pp. xi + 322, with 8 plates.) Price 21*s.* net.

Radium. X-Rays and the Living Cell with Physical Introduction. By Hector A. Colwell, M.B., D.P.H., and Sidney Russ, D.Sc., F.Inst.P., Second Edition Revised. London: G. Bell & Sons, 1924. (Pp. xi + 365, with 62 figures.) Price 21*s.* net.

The Natural History of Crystals. By A. E. H. Tutton, D.Sc., M.A., F.R.S., Past President of the Mineralogical Society, London: Kegan Paul, Trench, Trübner & Co.; New York: E. P. Dutton & Co., 1924. (Pp. xii + 287, with 166 illustrations). Price 15*s.* net.

Galvanomagnetic and Thermomagnetic Effects. The Hall and Allied Phenomena. By L. L. Campbell, Ph.D., Professor of Physics, Simmons College, Boston, U.S.A. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1923. (Pp. xii + 311.) Price 16*s.* net.

Conférences de Chimie Minérale. Par Marcel Guichard, Professeur à la Sorbonne, Troisième Édition. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins. (Pp. xxix + 353.) Price 30 frs.

What Industry owes to Chemical Science. By Richard B. Pilcher, O.B.E., and Frank Butler-Jones, B.A., A.I.C. With an Introduction by Sir George Beilby, LL.D., F.R.S. Second Edition, Revised and Enlarged. London: Constable & Company, 1923. (Pp. xv + 158.) Price 6*s.* net.

The Chemical Basis of Growth and Senescence. By T. Brailsford Robertson, Ph.D., B.Sc., Professor of Physiology and Biochemistry, University of Adelaide, South Australia. London: J. B. Lippincott Company, 16 John Street, Adelphi. (Pp. viii + 389.) Price 12*s.* 6*d.* net.

Organic Synthesis. An Annual Publication of Satisfactory Methods for the Preparation of Organic Chemicals. By Hans Thacker Clarke and others. Vol. III. New York: John Wiley & Sons; London: Chapman & Hall, 1923. (Pp. v + 104.) Price 7*s.* 6*d.* net.

Fats: Natural and Synthetic. By W. W. Myddleton, D.Sc., Lecturer in Chemistry, Birkbeck College, University of London, and T. Hedley Barry, Chemist to Messrs. E. Winstone & Sons. London: Ernest Benn, Ltd. 5 Bouverie St., E.C. 4, 1924. (Pp. xi + 182, with 35 figures.) Price 25*s.* net.

The Vegetable Proteins. By Thomas B. Osbone, Ph.D., Sc.D., Research Chemist in the Connecticut Agricultural Experiment Station, New Haven, Connecticut, Research Associate of the Carnegie Institution of Washington, D.C. Second Edition. London: Longmans, Green & Co., 39, Paternoster Row, E.C.4, 1924. (Pp. xiii + 154, with diagrams.) Price 9*s.* net.

Manuring of Grass Land for Hay. By Winifred E. Brenchley, D.Sc., Rothamsted Experimental Station. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1924. (Pp. viii + 144.) Price 12*s*. 6*d*. net.

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